HISTORY

153.A.13

PHYSICAL ASTRONOMY,

FROM

THE EARLIEST AGES

TO

THE MIDDLE OF THE NINETEENTH CENTURY.

COMPREHENDING

A DETAILED ACCOUNT OF THE ESTABLISHMENT OF

THE THEORY OF GRAVITATION BY NEWTON,

AND ITS DEVELOPEMENT BY HIS SUCCESSORS;

WITH AN EXPOSITION OF THE

PROGRESS OF RESEARCH ON ALL THE OTHER SUBJECTS OF

CELESTIAL PHYSICS.

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ROBERT BALDWIN, PATERNOSTER ROW, LONDON.

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XIII.C.2

HISTORY
OF
PHYSICAL ASTRONOMY.

INTRODUCTION.

Astronomy is not only one of the most ancient of the physical sciences, but also one of those which present the most alluring invitations to the contemplative mind. The starry heavens, spangling with countless luminaries of every shade of brilliancy, and revolving in eternal harmony round the earth, constitute one of the most imposing spectacles which nature offers to our observation. The waning of the placid moon, the variety and splendour of the constellations, and the dazzling lustre of the morning and evening star, must in all ages have excited emotions of admiration and delight. Sometimes the occurrence of an eclipse, or the sudden appearance of a comet, would create universal astonishment and terror; for these unusual phenomena have been generally regarded in early times as manifestations of divine displeasure, and the precursors of some impending calamity. But the wants of mankind rendered indispensable some degree of attention to the appearance of the heavens, even in the rudest state of society. The sun and moon minister so obviously to our subsistence and comfort, that their motions could not fail to be watched with interest in all ages. The stars, too, would soon be found to subserve some useful purposes. The mariner would find in them an unerring guide, while pursuing his way through the ocean; and the husbandman, by observing the times of their rising and setting throughout the year, would obtain indications of the change of the seasons, and would thereby be enabled to regulate the labours of the field.

But a powerful incentive to study Astronomy in early ages originated in a delusive opinion, that the destinies of human life were affected by the aspects and positions of the stars. Nor is it to be wondered at that these unapproachable objects should have been invested with a mysterious influence before science had disclosed their real nature. If the sun, by advancing with majestic regularity in his annual course, exercised so benign an influence on the animal and vegetable world, the planets, on the other hand, by their wayward evolutions and ever varying configurations appeared, naturally enough to minds imbued with imperfect notions of the purposes of creation, meetly to foreshadow the countless vicissitudes of human life. Hence the phenomena of the planetary movements were watched with feelings of superstitious awe; and all the particulars relating to them were carefully recorded for future guidance.

It is in Asia, the seat of all the early inventions of mankind, that we discern the dawn of this sublime science. The annals of the Chinese contain the earliest records of celestial phenomena; but the Chaldean observations are more interesting to Europeans on account of their connexion with modern astronomy. The serene skies and mild climate of central Asia were eminently favourable for contemplating the heavens. Accordingly we find that at Babylon eclipses and other phenomena were assiduously observed from a very remote antiquity.

But mere observation cannot constitute science. Facts, however carefully recorded, must be subsequently scrutinized, compared, and classified, before any general conclusions can be derived from them, relative to the arrangements of the material universe. The astronomers of Asia, although patient observers, do not appear to have in any age aspired to this more exalted occupation of the mind. The Greeks first reduced the knowledge relative to the celestial motions into a systematic form. This object was not, however, effected during the early period of Grecian history. The Chaldeans, by confining their attention to the mere occurrence of phenomena, were unable to arrive at general views of the celestial motions; the philosophers of the Grecian schools, on the other hand, long wasted their transcendent talents in groundless speculations, which were equally ineffectual in producing any permanent influence on the progress of Astronomy.

Amid the numberless ideas which perpetually occurred to the speculative minds of the Greek philosophers, it is perhaps not surprising that the true system of the world should have suggested itself to them. Pythagoras is said to have taught his followers that the sun is placed immoveable in the centre of the universe; and that the earth moves round him in an annual orbit. This system was first taught publicly by Philolaus, and was adopted by several ancient philosophers. Nicetas of Syracuse, on the other hand, is said to have explained the diurnal appearance of the heavens by the motion of the earth round a fixed axis. The ultimate abandonment of these sublime doctrines by the Greek philosophers, has been attributed to the hostility of the Aristotelians, who had placed the earth immoveable in the centre of the universe. It is doubtful, however, whether they were at any time supported by sound arguments drawn from observation. We know at least that the Pythagoreans, like the other sects of the Greek philosophers, were more prone to indulge in speculation than to examine facts.

It was not until the reign of the Ptolemies commenced at Alexandria, that Astronomy, under the munificent patronage of those princes, was cultivated as a science of observation and theory. HIPPARCHUS, who flourished about the year 160 A.C., is the most illustrious astronomer of antiquity. The island of Rhodes is known to have been the principal scene of his labours. He is also alleged to have made observations at Alexandria; but this is a point which cannot be easily decided. This great man was at once a mathematician, an observer, and a theorist; and in all these capacities he exhibited powers of genius of the highest order: only two or three individuals can rank with him in the history of physical science. We owe to him the earliest catalogue of the stars, and the first theories of the sun and moon, in which their motions were submitted to strict calculation. He also executed the greater portion of the observations for a similar theory of the planets; discovered the precession of the equinoxes, and invented the sciences of plane and spherical trigonometry. He represented the motions of the sun and moon by means of epicycles revolving on circular orbits. This ingenious hypothesis had been already imagined by the Greek philosophers; but it proved of little value so long as it was unaccompanied by a calculus. Hipparchus supplied this desideratum by his invention of trigonometry, and computed tables of the sun and moon. The epicyclical theory did not indeed accord with the real state of the heavens; but it served the valuable purpose of enabling the astronomer to group together the facts derived from observation, and to predict the places of the celestial bodies with all the accuracy demanded by the existing condition of practical science. Nor should it be forgotten, in estimating the merits of this theory, that it was by a comparison of its results with those derived from actual observation that the real nature of the planetary motions was finally discovered.

The most eminent astronomer of ancient times after Hipparchus is Ptolemy, who flourished about the year 140 A.D. He devoted his attention chiefly to the task of extending and improving the theories of Hipparchus. He established the theory of the planets in accordance with the principles of that astronomer. He also discovered the inequality in the moon's longitude, termed the evection, and was the first who pointed out the effect of refraction in altering the place of a heavenly body. He is the author of a treatise on Astronomy called in Greek the Syntaxis, but which has been more frequently designated by the Arabian name of the Almagest. This work, which has come down to us entire, is remarkable for containing nearly all the knowledge we possess of the ancient Astronomy. Ptolemy adopted as the basis of his work, the system of the world which places the earth immoveable in the centre of the universe, the sun, moon, and planets revolving severally in orbits of different magnitudes, and the whole heavens turning round it every twenty-four hours. This system has been termed the Ptolemaic, because it was defended by the author of the Syntaxis; but, if we are to look for its origin, we must ascend to a much higher antiquity.

With the irruption of the followers of Mahomet into Egypt, and the destruction of the famous library of Alexandria, about the middle of the seventh century, the science of Astronomy, which had long been declining among the Greeks, finally ceased altogether to be cultivated by that people. The Arabians, who now succeeded to the empire of the civilized world, devoted themselves with laudable assiduity to the study of the Greek authors, and Bagdad henceforth assumed the place of Alexandria, as the centre of literature and philosophy. Astronomy was cultivated by them with great ardour; but, like all other oriental nations, they exhibited an incapacity for speculation, and consequently the science did not acquire any extension from their labours. They generally adhered with superstitious reverence to the theories of the Greek astronomers, which they sought to amend only by means of more accurate observations. In the practical department of the science they indeed displayed a marked superiority to their masters, whose natural genius was averse to the monotonous task of observation. The Arabian astronomers may be said to have acted merely as the faithful guardians of science until the progress of events transferred it to a race of greater intellectual vigour.

After ages of profound slumber, Western Europe finally awoke to pursue her glorious career. In the ninth and tenth centuries, several enlightened persons travelled from France and England into Spain, to study mathematics and astronomy at the Moorish universities, and upon their return home diffused a knowledge of those sciences among their countrymen. In the thirteenth century the Almagest was translated from Arabic into Latin, under the auspices of the emperor Frederick the

Second. This step was attended with the most beneficial consequences to the study of Astronomy, which was now rendered generally accessible to persons of learning throughout all those countries where the Latin language prevailed. In the thirteenth century, Alphonso X., King of Castile, conferred a great benefit on science by causing the publication of new tables of Astronomy. They were executed at an immense expense, under the superintendence of the most eminent astronomers who could be found at the Moorish universities. Alphonso is reported to have said of the prevailing system of Astronomy, teeming with

> " Cycle upon epicycle, orb on orb,"

that if the Deity had consulted him at the creation of the world he would have given him good advice. This remark, though irreverent in the highest degree, was doubtless meant to convey a censure upon the cumbrous mechanism by which the system of the world was represented, rather than upon the actual arrangements of the system itself.

About the close of the fifteenth century the study of Astronomy received a great impulse from the labours of Purbach and Regiomontanus, two Germans of very original genius. They introduced some modification of the ancient theories, and improved the methods of calculation. Nearly about the same time the art of observation was revived by Waltherus, an astronomer of considerable merit, and a native also of Germany.

NICHOLAS COPERNICUS, the restorer of the true system of Astronomy, was born at Thorn, a town in Polish Prussia, on the 12th of February, 1473*. This illustrious man was gifted with a profound sagacity, which enabled him to distinguish the genuine principles of nature from the contrivances of the human imagination. He had long meditated on the system of the world, and was struck with the complication of the theory representing it when contrasted with the harmony which everywhere pervaded the arrangements of creation. The earth was placed immoveable in the centre of the universe, while the sun, moon, and planets, and even the starry heavens, revolved round it with inconceivable velocities. He, however, considered it impossible to reconcile this hypothesis with the variable appearance presented by the superior planets in different parts of their orbits relative to the sun. He remarked especially that when Mars was in opposition, he almost rivalled Jupiter in brilliancy, while towards conjunction he dwindled to a star of the second magnitude. This fact appeared to him to offer irresistibly conclusive evidence that the earth could not be the centre of the planet's motion. He now began to ponder upon the opinions of some ancient philosophers on this subject. He found in the writings of Martianus Capella an opinion ascribed to the Egyptians, which supposed Mercury and Venus to revolve in orbits round the sun, while they accompanied him in his annual motion round the earth. He perceived that this theory would offer a most satisfactory account of the alternate appearance of the planets on each side of the sun, and would also determine the limit of their digressions. The increasing magnitude of the

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[*] Some writers of Germany have represented Copernicus as a native of that country; this is, however, a mistake, for he was of Sclavonic extraction. His grandfather, Nicholas Copernicus, was a native of Bohemia, but about the close of the fourteenth century he removed to Poland and established himself in Cracow. His name appears inscribed in the records of Cracow as a citizen of that place in the year 1396. His Bohemian origin was attested by the citizen Dambrowa. See Kopernik et ses Travaux, par Czynski, Paris, 1847.

value so long as it was unaccompanied by a calculus. Hipparchus supplied this desideratum by his invention of trigonometry, and computed tables of the sun and moon. The epicyclical theory did not indeed accord with the real state of the heavens; but it served the valuable purpose of enabling the astronomer to group together the facts derived from observation, and to predict the places of the celestial bodies with all the accuracy demanded by the existing condition of practical science. Nor should it be forgotten, in estimating the merits of this theory, that it was by a comparison of its results with those derived from actual observation that the real nature of the planetary motions was finally discovered.

The most eminent astronomer of ancient times after Hipparchus is PTOLEMY, who flourished about the year 140 A.D. He devoted his attention chiefly to the task of extending and improving the theories of Hipparchus. He established the theory of the planets in accordance with the principles of that astronomer. He also discovered the inequality in the moon's longitude, termed the evection, and was the first who pointed out the effect of refraction in altering the place of a heavenly body. He is the author of a treatise on Astronomy called in Greek the Syntaxis, but which has been more frequently designated by the Arabian name of the Almagest. This work, which has come down to us entire, is remarkable for containing nearly all the knowledge we possess of the ancient Astronomy. Ptolemy adopted as the basis of his work, the system of the world which places the earth immoveable in the centre of the universe, the sun, moon, and planets revolving severally in orbits of different magnitudes, and the whole heavens turning round it every twenty-four hours. This system has been termed the Ptolemaic, because it was defended by the author of the Syntaxis; but, if we are to look for its origin, we must ascend to a much higher antiquity.

With the irruption of the followers of Mahomet into Egypt, and the destruction of the famous library of Alexandria, about the middle of the seventh century, the science of Astronomy, which had long been declining among the Greeks, finally ceased altogether to be cultivated by that people. The Arabians, who now succeeded to the empire of the civilized world, devoted themselves with laudable assiduity to the study of the Greek authors, and Bagdad henceforth assumed the place of Alexandria, as the centre of literature and philosophy. Astronomy was cultivated by them with great ardour; but, like all other oriental nations, they exhibited an incapacity for speculation, and consequently the science did not acquire any extension from their labours. They generally adhered with superstitious reverence to the theories of the Greek astronomers, which they sought to amend only by means of more accurate observations. In the practical department of the science they indeed displayed a marked superiority to their masters, whose natural genius was averse to the monotonous task of observation. The Arabian astronomers may be said to have acted merely as the faithful guardians of science until the progress of events transferred it to a race of greater intellectual vigour.

After ages of profound slumber, Western Europe finally awoke to pursue her glorious career. In the ninth and tenth centuries, several enlightened persons travelled from France and England into Spain, to study mathematics and astronomy at the Moorish universities, and upon their return home diffused a knowledge of those sciences among their countrymen. In the thirteenth century the Almagest was translated from Arabic into Latin, under the auspices of the emperor Frederick the

Second. This step was attended with the most beneficial consequences to the study of Astronomy, which was now rendered generally accessible to persons of learning throughout all those countries where the Latin language prevailed. In the thirteenth century, Alphonso X., King of Castile, conferred a great benefit on science by causing the publication of new tables of Astronomy. They were executed at an immense expense, under the superintendence of the most eminent astronomers who could be found at the Moorish universities. Alphonso is reported to have said of the prevailing system of Astronomy, teeming with

"Cycle upon epicycle, orb on orb,"

that if the Deity had consulted him at the creation of the world he would have given him good advice. This remark, though irreverent in the highest degree, was doubtless meant to convey a censure upon the cumbrous mechanism by which the system of the world was represented, rather than upon the actual arrangements of the system itself.

About the close of the fifteenth century the study of Astronomy received a great impulse from the labours of Purbach and Regiomontanus, two Germans of very original genius. They introduced some modification of the ancient theories, and improved the methods of calculation. Nearly about the same time the art of observation was revived by Waltherus, an astronomer of considerable merit, and a native also of Germany.

Nicholas Copernicus, the restorer of the true system of Astronomy, was born at Thorn, a town in Polish Prussia, on the 12th of February, 1473*. This illustrious man was gifted with a profound sagacity, which enabled him to distinguish the genuine principles of nature from the contrivances of the human imagination. He had long meditated on the system of the world, and was struck with the complication of the theory representing it, when contrasted with the harmony which everywhere pervaded the arrangements of creation. The earth was placed immoveable in the centre of the universe, while the sun, moon, and planets, and even the starry heavens, revolved round it with inconceivable velocities. He, however, considered it impossible to reconcile this hypothesis with the variable appearance presented by the superior planets in different parts of their orbits relative to the sun. He remarked especially that when Mars was in opposition, he almost rivalled Jupiter in brilliancy, while towards conjunction he dwindled to a star of the second magnitude. This fact appeared to him to offer irresistibly conclusive evidence that the earth could not be the centre of the planet's motion. He now began to ponder upon the opinions of some ancient philosophers on this subject. He found in the writings of Martianus Capella an opinion ascribed to the Egyptians, which supposed Mercury and Venus to revolve in orbits round the sun, while they accompanied him in his annual motion round the earth. He perceived that this theory would offer a most satisfactory account of the alternate appearance of the planets on each side of the sun, and would also determine the limit of their digressions. The increasing magnitude of the superior planets as they approached towards opposition, when contemplated in connexion with this doctrine, naturally led him to conceive that they also might probably revolve round the sun as the centre of their motions. This conclusion was strengthened by the opinion of Pythagoras, who had placed the sun immoveable in the centre of the universe, and assigned to the earth an annual motion in the ecliptic. Finally it occurred to him, that Nicetas, of Syracuse, and some other ancient philosophers, had supposed the heavens to be at rest, and sought to explain their diurnal changes by ascribing to the earth a motion round a fixed axis. Having reflected profoundly upon these various principles, he found that, by combining them together, the resulting system accounted with the most scrupulous fidelity for all the phenomena of the celestial motions, while it was distinguished by a union of harmony and simplicity which admirably accorded with the general economy of nature. The alternate vicissitudes of night and day, the varied circle of the seasons, the stations and retrogradations of the planets, and their variable appearance at different times of the year, all offered themselves as immediate consequences of this beautiful system.

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* Copernicus died in the year 1543. He was of Sclavonic extraction. His grandfather, Nicholas Copernicus, was a native of Bohemia; but about the close of the fourteenth century he removed to Poland, and established himself in Cracow. His name appears inscribed in the records of that city for the year 1396. His Bohemian origin was duly attested on the occasion of his enrolment.

According to Copernicus, then, the sun is placed immoveable in the centre of the universe, and all the planets, including the earth, revolve round him in the order of the signs in concentric orbits, Mercury and Venus revolving within the earth's orbit, and all the other planets without it. While the earth is traversing her annual orbit, she is also constantly revolving from west to east round a fixed axis passing through the celestial poles, accomplishing a complete revolution every twenty-four hours. Copernicus explained the motion of the moon by supposing her to revolve in a monthly orbit round the earth, while at the same time she accompanied her in her annual motion round the sun. He also very ingeniously accounted for the precession of the equinoxes, by attributing to the earth's axis a slow conical motion in a direction opposite to the apparent motion of the stars. This great man has given to the world a full exposition of his principles in his famous work, " De Revolutionibus Orbium Celestium." It is said, that he received the first copy of this work, upon the contents of which he had meditated thirty-six years, only a few hours before his death.

Although Copernicus greatly simplified the system of the world, he still retained the machinery of epicycles to represent the motions of the planets, and therefore left an ample field of research to his successors. But before the investigation of the actual form of the planetary orbits could be prosecuted with any hopes of success, it was necessary that a great improvement should be effected in practical astronomy. The art of observation still continued in the same condition in which it existed among the Greeks and Arabians. Copernicus was less conspicuous for the qualities of an observer than for his sagacity in unfolding the principles of nature. The various tables of astronomy had all fallen considerably into error, and the necessity of reconstructing them upon a more accurate basis appeared indispensable to the future progress of the science. It is clear, then, that the present crisis required less a theorist of the first order, than an astronomer who might possess sufficient genius and practical skill to perfect the methods of observation, to imagine new instruments, and by these means to establish a number of accurate facts relative to the motions of the planets. These qualities were eminently fulfilled in Tycho Brahé, whose labours introduce a new era in the art of observation. This illustrious astronomer was born in the year 1546 at Knudsthorp, a province of Sweden, then attached to the Danish monarchy. His great celebrity induced Ferdinand, king of Denmark, to build for him in the island of Huena, at the mouth of the Baltic, a magnificent observatory, which he designated by the appellation of Uraniburg, or the City of the Heavens. Herein he deposited a magnificent collection of instruments, and under the munificent patronage of his sovereign he continued to prosecute researches in astronomy during a period of nearly twenty years. Several important discoveries relative to different branches of the science, and a vast mass of observations, infinitely superior in point of accuracy to any that had ever before been executed, were the happy result of his labours. Strange to say, he rejected the Copernican system of the world, adopting in its stead a system of his own, called, in consequence, the Tychonic. According to this system the earth is placed immoveable in the centre of the universe, while the sun revolves in an annual orbit in the ecliptic, accompanied by all the planets circulating round him as the centre of their motions. The inferiority of this system to the Copernican is so obvious that it found only a very small number of followers, and it soon fell into total oblivion.

This eminent astronomer, who had contributed so much towards the glory of Denmark, had the misfortune, in the latter part of his life, to incur the hostility of the ministers of his sovereign, Christian VII., who succeeded Frederick on the throne. They were mortified to find themselves completely eclipsed by their illustrious countryman, who had won his laurels in a field which they had been always accustomed to regard with contempt. They were especially chagrined on account of the number of distinguished individuals who annually resorted from all parts of Europe to the island of Huena, to pay their respects to its renowned inhabitant. Under the pretence that the finances of the kingdom could no longer admit of maintaining the establishment of Uraniburg, they totally withdrew from him the revenues which Frederick had assigned to him for that purpose; and he was compelled, in consequence, to look out for an asylum in a foreign land. He finally selected Germany as his future place of residence. Embarking, therefore, in a small vessel with his family, after putting on board his books, his instruments, and all his effects, he set sail from the beloved scene of his labours, and bade a final farewell to his ungrateful country. He was kindly received by the Emperor Rodolph, who bestowed on him the appointment of imperial astronomer, and assigned to him a splendid mansion near the city of Prague. He was not destined, however, to enjoy long the favours of his new patron; for, only two or three years after his arrival in Germany, he was seized with a severe illness, of which he expired on the 14th of October, 1601, in the fifty-fifth year of his age.

While the study of Astronomy continues to delight the human mind, the name of Tycho Brahé will be held in grateful remembrance. The vast extent of this astronomer's observations, the ingenuity of his methods, and the patience and skill which he exhibited in carrying them into effect, have deservedly earned for him an immortal reputation. He did not indeed scan the heavens with the philosophic eye of a Kepler or a Newton, but his labours were no less essential to the progress of astronomy, than the more captivating discoveries of these illustrious geniuses. His catalogue of the stars, his researches on comets and on refraction, and his beautiful discoveries in the motion of the moon, will remain enduring monuments of his glory. Nor can it be accounted the least of the obligations which posterity owes to him, that his accurate observations on the planets were the means of conducting Kepler to the discovery of those famous laws which form the groundwork of modern Astronomy.

Few of those philosophers who have extended the boundaries of science have accomplished results of equal importance with those due to the illustrious KEPLER. This eminent astronomer was born at Wiel, in the Duchy of Wirtemberg, in the year 1571. Gifted with a genius of the highest order, and a strong tendency towards speculation, he seemed destined by Providence to effect a complete revolution in the theories of Astronomy. Tycho's observations on the planets were eagerly seized by him; and, after seventeen years of incessant application, during which he continued to submit them to a searching scrutiny, he finally arrived at those famous theorems which embody the true principles of the system of the world. Copernicus, as we have already remarked, did not attack the principle of the epicyclical theory: he merely sought to make it more simple by placing the centre of the earth's orbit in the centre of the universe. This was the point to which the motions of the planets were referred, for the planes of their orbits were made to pass through it, and their points of least and greatest velocities were also determined with reference to it. By this arrangement the sun was situate mathematically near the centre of the planetary system, but he did not appear to have any physical connexion with the planets as the centre of their motions. The Copernican theory continued in this incomplete state until Kepler, in the course of his consummate researches, demonstrated the important fact that the planes of the orbits of all the planets, and the lines joining their apsids, passed through the sun. This discovery alone, by assigning to the sun his just relation to the planets, contributed in a vast degree towards a more accurate knowledge of the true state of the solar system.

Kepler's famous laws of the planetary motions are known to every reader. The first is, that all the planets move in ellipses, having the sun in one of the foci; the second, that a line joining the planet and the sun sweeps over equal areas in equal times; the third, that the squares of the periodic times are proportional to the cubes of the mean distances from the sun. Kepler was conducted to the first and second of these laws by researches on the motion of the planet Mars, the orbit of which, being more eccentric than that of any of the other superior planets, exhibited in stronger relief the errors of the ancient theories. They were first announced by him in the year 1609, in his famous work, "De Motibus Stellæ Martis."* The third law, although apparently more easy to arrive at, did not yield to his researches until nine years afterwards. The delight he felt upon finding he had discovered this law may be imagined from the following passage of his work on "Harmonics," in which he first mentioned it. "What I prophesied twenty-two years ago, as soon as I discovered the five solids among the heavenly orbits—what I firmly believed long before I had seen Ptolemy's harmonics—what I had promised my friends in the title of this book, which I named before I was sure of my discovery—what sixteen years ago I urged as a thing to be sought—that for which I joined Tycho Brahé, for which I settled in Prague, for which I have devoted the best part of my life to astronomical contemplations—at length I have brought to light, and have recognised its truth beyond my most sanguine expectations. . . . . . It is now eighteen months since I got the first glimpse of light, three months since the dawn; very few days since the unveiled sun, most admirable to gaze on, burst out upon me. Nothing holds me; I will indulge in my sacred fury; I will triumph over mankind by the honest confession that I have stolen the golden vases of the Egyptians* to build up a tabernacle for my God far away from the confines of Egypt. If you forgive me, I rejoice: if you are angry, I can bear it: the die is cast, the book is written; to be read either now or by posterity, I care not which: it may well wait a century for a reader, as God has waited six thousand years for an interpreter of his works."†

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* Astronomia Nova, seu Physica Cœlestis tradita Commentariis de Motibus Stellæ Martis. Pragæ, 1609.

This great man was harassed with poverty throughout his whole career. He filled the office of imperial astronomer, to which a munificent salary was attached; but he found by sad experience that the remuneration was rather nominal than real; for only a miserable pittance of his claims was doled out to him at distant intervals; and, in order to prevent his family from starving, he was compelled to publish a low prophesying almanack, for which he entertained the utmost contempt. In hopes of recovering the arrears due to him, he resolved to proceed to Ratisbon and represent his claims to the Diet. Pursuant to this design, he set out upon his journey in the month of November, 1630, and arrived in Ratisbon worn out with ill health and anxiety. In this last appeal to his country he was unhappily unsuccessful; and the disappointment he felt in consequence, reacting upon his debilitated frame, threw him into a violent fever, which carried him off a few days afterwards, in the sixtieth year of his age.

Kepler was one of those exalted geniuses who appear from time to time on the theatre of the world to give an impulse to the progress of physical science. In acuteness and sagacity he is equalled among modern philosophers only by Galileo and Newton. He did not indeed exhibit the wariness of these illustrious sages in his researches, but he compensated by his daring adventure for his want of stratagetic skill. Gifted with an ardent imagination, which revelled in the formation of theories, and possessing indomitable powers of application, he threw the whole strength of his intellectual faculties into his researches, and continued to prosecute them with unceasing energy, until he assured himself of the truth or falsehood of the principles on which they were founded. He was no doubt frequently induced, by the specious illusions which conjured themselves up before his mind, to waste his powers on a mere phantom; but, even in his wildest aberrations, we discern flashes of genius which threw a bright gleam upon many obscure points of nature, and served like so many guiding stars to succeeding philosophers. His candour in dismissing hypotheses as soon as he found them untenable, was no less remarkable than the aptitude he evinced in their formation; and to these valuable qualities, combined with the fertility of his inventive powers and his unconquerable perseverance, may be ascribed the brilliant success with which his labours were rewarded.

The advantages which accrued to the science of Astronomy from Kep-

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[*] Kepler alludes in this allegory to Ptolemy, who had fixed with remarkable accuracy the ratio of the orbit of each planet to the earth's orbit, or, in the language of the Ancient Astronomy, the ratio of the deferent to the epicycle. These ratios, slightly corrected by Tycho Brahé, formed the data by means of which Kepler was conducted to his great discovery.

[†] Harmonices Mundi, p. 178. See also Life of Kepler, Library of Useful Knowledge.

ler's labours are obvious to every reader. By his discovery of those remarkable laws with which his name has been immortally associated, he razed the existing theories to their very foundation, sweeping away the whole machinery of cycles and epicycles, with which the human mind in the weakness of its earlier investigations had defaced the fair arrangements of the heavens, and introducing in its stead the sublime spectacle of the planets revolving with majestic simplicity and harmony in elliptic orbits round the sun in the foci. In all his investigations he sought to shape his theories so as to accord with the physical principles which he conceived to govern the celestial motions; and, although he has nowhere succeeded in demonstrating by legitimate reasoning the reality of those principles, still the practice which he pursued in this respect had the advantage of continually leading him to concentrate his ideas on the main object of his researches, and thereby of finally assuring a triumphant issue to his labours.

Our own island was about this time adorned by a discovery that was destined to prove of incalculable advantage to the astronomer in his future labours. It is manifest that, as the observations on the celestial bodies continued to acquire greater precision, it became necessary to introduce a corresponding degree of refinement into the calculations to which they gave rise. The sines and tangents of arcs, which form the basis of such calculations, cannot be expressed in finite numerical terms, and therefore admit only of approximate values, which are more accurate in proportion to the number of terms they contain. The arithmetical operations performed on such functions become in consequence exceedingly laborious; and this is very apparent when we consider that the questions of plane and spherical trigonometry generally consist in finding a fourth proportional to three given numbers. The illustrious NAPIER*, by his invention of logarithms, supplied astronomers with an easy and universal method of abbreviating all such calculations, its effect being to replace all operations of multiplication, division, and evolution, by the more commodious and agreeable processes of addition and subtraction. "This admirable artifice," says Laplace, "engrafted on the ingenious algorithm of the Indians, by reducing to a few days the work of several months, doubles, if we may so speak, the life of the astronomer, and spares him the errors and the disgust inseparable from long calculations; an invention which is the more gratifying to the human mind, in so far as it has derived it entirely from its own resources. In the arts man avails himself of the materials and forces of nature to increase his power; but here everything is his own work."†

While Napier was pondering in remote seclusion over his immortal invention, and Kepler, amid continual struggles with poverty and misfortune, was engaged in those toilsome researches which resulted in placing the science of Astronomy on its present basis, universal Europe was ringing with the fame of a philosopher whose labours produced no less important effects on the progress of science than those of his illustrious contemporaries, and ushered in with fitting splendour the train of magnificent discoveries by which the seventeenth century was so eminently distinguished.

GALILEO GALILEI was born at Pisa, a city in the Grand Duchy of Tuscany in Italy, in the year 1564. While a student at the university, he distinguished himself by his powers of discussion, and by the freedom with which he questioned some of the leading doctrines of the Aristotelian philosophy. At this period of his life, also, the idea of employing the pendulum for the purpose of measuring time first suggested itself to him, on seeing a lamp suspended from the roof of the cathedral of Pisa continuing for some time to swing to and fro. In 1609, having heard that a Dutch spectacle maker had succeeded in combining lenses so as to make distant objects appear larger and nearer, he very soon succeeded in tracing this effect to the refraction of the visual rays in passing through the glass; and upon this principle he constructed the first telescope used for scientific purposes. Turning his instrument towards the heavens, his ingenuity soon found its reward, in the discovery of a multitude of beautiful phenomena. To him we are indebted for the first announcement, that the sun is covered with dark irregular spots—that the moon is diversified with hills and valleys like the earth—that the planets have a round appearance like the sun or moon—that Venus exhibits phases depending on her position relative to the earth and sun—that Jupiter is accompanied by four satellites—that the appearance of Saturn is totally unlike that of the other planets—and that the milky way consists of a countless multitude of stars. He also discovered the diurnal libration of the moon; and, from the solar spots, he drew the important inference that the sun has a rotatory motion round a fixed axis. Galileo is still more famous for his researches in mechanical science. He was the first who announced, in distinct terms, the principle of virtual velocities, and its utility in determining the relation between the power and the weight in all combinations of machines. He also discovered the law of acceleration of falling bodies, whether descending vertically or along inclined planes, and he determined the path of a projectile by considering the horizontal motion to be uninfluenced by the vertical action of gravity.

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* Born in 1550, at Merchistoun, near Edinburgh; died in 1617.

† Exposition du Système du Monde, Liv. v., chap. iv.

The brilliant success which rewarded the physical researches of Galileo, and the withering influence which his discoveries exercised on the doctrines of the Aristotelian philosophy, excited against him the implacable animosity of his opponents, who saw with dismay the boasted citadel of learning, within which the human mind had for ages reposed in complacency, now exposed to the powerful and reiterated assaults of a daring innovator. Unable to vanquish him in the field of argument, they sought to recover their sinking position by enlisting the church under their banners; and, with this view, they proceeded to represent the Copernican theory as dangerous to religion, by contending that it was at variance with the received interpretation of the Holy Scriptures. Galileo became, in consequence, involved in a quarrel with the Church, which finally resulted in his being summoned before the Inquisition, and compelled to abjure on his knees the doctrines which taught that the sun is placed immoveable in the centre of the universe, and that the earth revolves in an annual orbit round him. It is said that the venerable philosopher had no sooner finished this humiliating recantation than he stamped the ground with his foot, and whispered to one of his friends "E pur si muove."* He was condemned to strict seclusion during the remaining few years of his life, and died in 1642, at the age of seventy-eight years.

Galileo's merits as a philosopher are at once great and varied. He broke down the barrier which had so long interposed between the human understanding and the beautiful system of the material world, denouncing with unrivalled force the pernicious subtleties of the schools, and contending for the necessity of constant observation and experiment, as the only reliable guides in conducting the mind to general principles in physical science. He may, therefore, be considered, in conjunction with our illustrious countryman, Bacon, to have founded the inductive method of investigation, by the aid of which man has achieved so many brilliant conquests over nature during the last two centuries. Nor is it his sole merit that he overthrew the idols of the ancient philosophy, and recommended by his powerful reasoning the necessity of a careful examination of facts in all physical researches. He supplied the most conclusive arguments in favour of his principles, in the multitude of splendid discoveries which he had the glory of first announcing to the world. His example also stimulated a band of ardent minds to embark in the same hopeful career; and an impulse was thus given to the study of experimental philosophy which has continued to be maintained with unabated vigour until the present day.

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* "It moves however."

The astronomical discoveries of Galileo, although remarkable for their brilliancy, derived their chief value from the support they lent to the Copernican theory, and the influence they exerted in overthrowing the false system of philosophy which then prevailed. But it is in his important researches relative to mechanical science, that the genius of this great philosopher is most apparent. The science of motion could not indeed be said to have existed before his time, for the sole knowledge on this subject consisted of a few unintelligible maxims scattered through the works of Aristotle. It required no common degree of penetration to expose the errors which lurked amid the sophisms of the illustrious Stagyrite; but a genius of a higher order was necessary to establish the clear and immutable laws of nature, in the room of the unmeaning subtleties of the schools. The sagacity and skill which Galileo displays in resolving the phenomena of motion into their constituent elements, and hence deriving the original principles involved in them, will ever assure to him a distinguished place among those who have extended the domains of science. It is, perhaps, impossible, in the present advanced state of mechanical philosophy, to form a just estimate of the difficulties which then interposed towards a precise and luminous view of the fundamental principles of motion. It is universally admitted that those phenomena which come under the daily observation of mankind, and which on that account do not possess any salient features on which the imagination can repose, are generally those which are most liable to elude the inquiries of ordinary minds. The principles which Galileo established by his sagacious researches had the effect of elevating mechanical science to the dignity of one of the most important subjects which can concern the attention of mankind. They were essential elements in the train of investigation which conducted Newton to the sublime discovery of Universal Gravitation; and, in fact, they constitute the basis upon which the vast superstructure of the physico-mathematical sciences has been reared.

Descartes was undoubtedly one of the greatest geniuses of the seventeenth century; but it can hardly be said that his labours had a direct tendency to promote the progress of Astronomy. In order to account for the motions of the various bodies of the solar system, he imagined the famous system of ethereal vortices. The planets were all supposed to revolve in a vortex, of which the sun was the centre, and the satellites revolved in smaller vortices round their respective primaries. This system offered a plausible explanation of the motions of the planets and satellites in one common direction; but it was inconsistent with the motions of comets, and a multitude of other phenomena, and, besides, was nothing else than a mere gratuitous assumption. Some writers commend the Cartesian system of vortices, as the earliest attempt to explain the motions of the planets by mechanical principles; but Delambre has justly remarked, that, by misleading men's minds from nature, this fiction of the imagination retarded rather than promoted the progress of true science. Descartes, however, deserves honourable mention in the history of Astronomy on account of the vigorous efforts he made to overthrow the Aristotelian philosophy, and especially for his important discoveries in the pure mathematics. By his happy innovation of expressing the fundamental property of a curve by means of an equation between two variable co-ordinates, he extended incalculably the powers of analysis, besides thereby preparing the way for the discovery of the infinitesimal calculus, and its application to the vast domain of Celestial Dynamics.

Nearly about the same time with Descartes flourished HUYGENS, a philosopher endued with equal genius, but exhibiting greater caution in his researches. Posterity is indebted to him for one of the most admirable inventions of modern times—the application of the pendulum to clocks. Mechanical constructions moved by weights had been employed to measure time as early as the thirteenth century, and Galileo had already conceived the idea of using the pendulum for a similar purpose. The Italian philosopher failed, however, in all his attempts to construct an accurate timekeeper, because he constantly sought to apply the pendulum as the prime mover. Huygens accomplished this object with the most complete success, by simply making the pendulum to regulate the descent of the weight in the ancient clocks. It would be difficult to say whether the ordinary concerns of life, or the more refined purposes of science, have gained most by this valuable improvement. Huygens is distinguished by his telescopic discoveries in the heavens. He it was who first established the real character of the appendage with which Saturn is furnished, having found it to consist of a luminous ring, encompassing the body of the planet, at an appreciable distance from his surface. He also discovered the most conspicuous of the satellites of that planet; but he forgot his habitual caution when he asserted that as his discovery made the number of satellites equal to that of the planets, no others of a similar kind would be made in the solar system. Huygens discovered the principal theorems relative to the motion of a body compelled to revolve in a circular orbit, under the influence of a force acting constantly at the centre. These theorems were announced at the end of his treatise, "De Horologio Oscillatorio," published in the year 1671; but no demonstration was given of them. By his elegant speculations on the evolutes of curves he also facilitated the application of the same principles to orbits of variable curvature. This philosopher is indeed universally admitted to be one of the most original geniuses who flourished in the seventeenth century. In his intellectual character there appears the rare union of all those qualities which form the mathematician and the experimental philosopher. In this respect he approaches more nearly to the illustrious Newton than any other individual of modern times.

CASSINI, the contemporary of Huygens, was one of the greatest nomers of the age in which he lived. We owe to him a multitude of discoveries, which have secured for him an imperishable reputation. He constructed the first tables of Jupiter's satellites which could lay any claim to accuracy. He discovered four of Saturn's satellites; determined the rotations of Jupiter and Mars, and arrived at a very approximate value of the solar parallax. He also discovered the belts of Jupiter and the zodiacal light; established the singular coincidence of the nodes of the lunar equator and orbit; and, lastly, constructed an excellent table of refractions.

The establishment of the Royal Society of London in 1662, and of the Academy of Sciences of Paris a few years afterwards, contributed very much to the advancement of Astronomy. Great advantages were also destined to accrue to the science from the observatories of Greenwich and Paris, which were founded, the former in 1675, and the latter in 1667. Towards the close of the seventeenth century a great improvement was effected in the art of observation by the application of the telescope to quadrants fixed in the meridian, and by the invention of the micrometer and the transit instrument.

ROEMER, a Danish astronomer, has immortalized himself by his discovery of the gradual propagation of light. Having found that the eclipses of Jupiter's satellites took place earlier or later than the computed time, according as the earth was nearer or more remote from the planet, he was hence led to suspect that light might occupy some time in its transmission. This suspicion was confirmed by a more minute examination of the errors in the times of the eclipses, which he found to correspond exactly to the variations of the distance between the earth and the planet. Finding that the eclipses took place eight minutes earlier than the computed time when the distance between the earth and the planet was least, and eight minutes later when it was greatest, he hence inferred that light occupies sixteen minutes in traversing the earth's orbit, and, consequently, that it moves with the amazing velocity of 190,000 miles in a second *. This important fact was established beyond all doubt in the following century by Bradley's discovery of aberration.

Among the successors of Tycho Brahé in practical astronomy, may be cited HEVELIUS, one of the most talented observers of any age or country. FLAMSTEED, also, has rendered his name for ever illustrious by his great catalogue of the stars. This vast monument of patience, assiduity, and skill has recently undergone a complete renovation, through the care and attention of the late lamented Mr. Baily. HALLEY would be entitled to honourable mention in the history of Astronomy, were it only on account of his catalogue of southern stars; but he possesses much higher claims to the remembrance of posterity. His beautiful researches on comets, so remarkably illustrated by the comet which bears his name; his method for determining the solar parallax by means of the transits of Venus; his discovery of the long inequality of Jupiter and Saturn, and of the secular acceleration of the moon's mean motion, are all incontestable claims in his favour as deserving of being ranked among the greatest astronomers of modern times.

We are now arrived at the memorable epoch of Newton's discoveries. In the following pages we shall attempt to trace the progress of Physical Astronomy from the commencement of that illustrious philosopher's career;

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* This is rather the estimation of modern astronomers. Roemer made the time of traversing the earth's orbit, first eleven minutes, and afterwards fourteen minutes.

down to the most recent discoveries of our own day. In order that the reader may more fully appreciate the researches of geometers on the theory of Jupiter's satellites, we have introduced that part of our subject with an historical account of the labours of astronomers in establishing the various irregularities in the motions of those bodies before any attempt was made to explain them by the principle of universal gravitation. We have been induced more especially to enter upon this digression, as we are not acquainted with any work in the English language containing a detailed account of the successive steps by which the beautiful theory in question has advanced towards its present high state of perfection. We hope, therefore, that the two chapters which will be devoted to that subject will not fail to prove in some degree interesting to the reader, inasmuch as he will find in them a statement of all the principal facts connected with the history of the satellites, accompanied with a familiar exposition of the various results to which astronomers and mathematicians have been conducted by their respective researches. The history of Physical Astronomy will terminate with an account of the circumstances connected with the discovery of the planet Neptune. This brilliant triumph of the theory of gravitation is due to the independent labours of two mathematicians who were simultaneously pursuing researches for that purpose, without mutual concert, and who arrived about the same time at similar results. The researches of M. Leverrier undoubtedly first led to the actual discovery of the planet; but those of our distinguished countryman, Mr. Adams, while they had the advantage in point of priority, were equally calculated to effect the same object, and there is undeniable evidence that they would eventually have done so. The independent theoretical discovery of the planet by two individuals is now admitted by the most eminent philosophers of Europe, who have all concurred in awarding due honour to each discoverer. "Far be it from us," says the illustrious astronomer of Pulkowa *, speaking for his colleagues and himself, "to have any intention of withholding our entire admiration from the eminent merit of M. Leverrier. But impartial history will in the future make honourable mention also of the name of Mr. Adams, and recognise two individuals as having, independently of one another, discovered the planet beyond Uranus. In the same way it attributes the discovery of the infinitesimal calculus at once to Newton and Leibnitz." † The concluding part of this work will be devoted to an account of the recent discoveries in the other branches of the science, with an especial reference to the labours of HERSCHEL and STRUVE in Stellar Astronomy.

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* M. Struve.

† See Phil. Mag. vol. xxx. p. 184.

nomers of the age in which he lived. We owe to him a multitude of discoveries, which have secured for him an imperishable reputation. He constructed the first tables of Jupiter's satellites which could lay any claim to accuracy. He discovered four of Saturn's satellites; determined the rotations of Jupiter and Mars, and arrived at a very approximate value of the solar parallax. He also discovered the belts of Jupiter and the zodiacal light; established the singular coincidence of the nodes of the lunar equator and orbit; and, lastly, constructed an excellent table of refractions.

While astronomical science was thus flourishing on the Continent, it had already dawned upon England.

HARRIOT, the celebrated mathematician, was an assiduous observer of celestial phenomena. We owe to him some valuable observations of the comet of 1607, which have since been found to refer to one of the periodical apparitions of the famous comet of Halley. He was one of the first individuals who employed the telescope in exploring the heavens. His observations of Jupiter's satellites date from the 17th of October, 1610. He also observed the solar spots very soon after their discovery on the Continent.

JEREMIAH HORROCKS, a native of the north of England, displayed a capacity of the highest order for the cultivation of astronomy; but unfortunately his career was soon brought to a close by a premature death. We owe to him the earliest observation of the transit of Venus. He and his friend Crabtree were the only two individuals who witnessed this rare phenomenon on the 24th of November, 1639. He effected an important improvement in the lunar theory, and made many sagacious remarks on other subjects relating to astronomy. He died suddenly on the 3rd of January, 1641, at the age of about twenty-two years.

WILLIAM GASCOIGNE, the contemporary of Horrocks, had the merit of originating some remarkable improvements in practical astronomy. He was one of the first who employed the Keplerian telescope in astronomical observations. He introduced the use of telescopic sights. He was the original inventor of the micrometer, and was also the first who applied it to divided instruments. Like Horrocks, this highly-gifted individual perished in the flower of his age. He fell at the battle of Marston Moor, on the 2nd of July, 1644, when he had only attained the age of twenty-four years.

HEVELIUS was one of the most eminent observers of the seventeenth century. His labours extended over a period of about fifty years; but as he continued throughout his whole career to adhere to the ancient methods of practical astronomy, the results achieved by him do not possess a value commensurate with his merits as an observer.

During the seventeenth century, a great revolution was effected in practical astronomy. The application of the pendulum to clocks by Huyghens had the effect of introducing a method of observation which had been devised in the preceding century, but which was found to be impracticable in consequence of the difficulty attending the measurement of time. It consisted in observing the altitude of a celestial body on the meridian, and noting the instant of its passage. By this means the declination and right ascension were obtained without any trigonometrical calculation. In consequence of this improvement, the observations of the celestial bodies were henceforward made chiefly with instruments fixed in the meridian. The micrometer, the invention of which is due originally to Gascoigne, was reinvented on the Continent, and was brought to great perfection by Auzout. About the same time, the use of telescopic sights was introduced both in England and on the Continent.

The establishment of public observatories was another circumstance which imparted a strong impulse to the cultivation of astronomy. The earliest of these institutions is the Observatory of Copenhagen, which dates from the year 1656. The Royal Observatory of Paris was founded in 1667, and the Royal Observatory of Greenwich in 1675.

One of the most eminent astronomers of this period was Picard. We owe to him the first careful measurement of an arc of the meridian upon strictly scientific principles. He was also one of the first astronomers who employed telescopic sights in astronomical observations. His remarks on various subjects relating to astronomical science are characterised by great sagacity. He died in the year 1682.

Roemer, the Danish astronomer, has immortalized himself by his discovery of the gradual propagation of light. This important fact was suggested to him by observations of the eclipses of Jupiter's satellites, which he found to take place earlier or later than the computed time according as the distance between the earth and planet was less or greater than the mean distance. Its truth was established beyond all doubt in the following century by Bradley's discovery of Aberration. Roemer effected many important improvements in practical astronomy, one of the most valuable of which was the invention of the transit instrument.

During the latter part of the seventeenth century, astronomy was cultivated in England by various eminent individuals. Wren and Hooke applied their attention to the advancement of the practical department of the science. They also contributed by their splendid talents to throw light on various interesting points relating to theoretical astronomy. James Gregory is known to most readers by the invention of the reflecting telescope which bears his name. We owe also to this celebrated mathematician the original suggestion of the utility of the transits of the inferior planets for determining the value of the solar parallax. Flamsteed had commenced his long series of valuable observations at the Royal Observatory of Greenwich. Halley had returned from St. Helena, and was ardently engaged in promoting the objects of astronomical science.

We are now arrived at the epoch of the immortal discoveries of Newton. Before attempting to give an account of them, it will be desirable to notice briefly the ideas of celestial physics which prevailed before his time.

PREFACE.

THE main object of the work here submitted to the reader is to exhibit a view of the labours of successive enquirers in establishing a knowledge of the mechanical principles which regulate the movements of the celestial bodies, and in explaining the various phenomena relative to their physical constitution which observation with the telescope has disclosed. It may, perhaps, be desirable to trace out briefly the plan I have pursued in attempting to execute this undertaking.

The first part of the work, extending to the close of the thirteenth chapter, is devoted to the history of the Theory of Gravitation. In the first and third chapters I have endeavoured to give some account of the immortal discoveries by which Newton established this theory in its utmost generality. The researches of the learned Prof. Rigaud have recently disclosed some interesting details respecting the original publication of the Principia, of which I have not failed to avail myself in the execution of this portion of the work.

The future history of Celestial Mechanics naturally admits of a division into two distinct periods. The first comprehends the researches of geometers from the time of Newton to the commencement of the nineteenth century. Towards the close of this period the analytical methods devised for the developement of the Theory of Gravitation had attained a high state of perfection, and the various phenomena which had seemed irreconcilable with its principles, were all satisfactorily accounted for. The second period embraces the further developement of the theory down to the present time.

The third and following chapters to the ninth inclusive, are devoted to the first of the above-mentioned periods. The third chapter contains an account of the early researches of Euler, Clairaut, and D'Alembert on the Problem of Three Bodies, and of the application of their respective solutions to the lunar theory. The difficulty which for some time attended the computation of the movement of the lunar apogee, was at length effectually removed by Clairaut, and the triumph of the Newtonian principles was practically exhibited in the construction of lunar tables by Mayer, which possessed sufficient accuracy to be employed with confidence in the solution of the great Problem of the Longitude.

It is a curious fact that, in the original edition of the Principia, Newton gave the results of an investigation of the movement of the lunar apogee, which seemed to imply that he had treated the subject by a method of a sufficiently comprehensive character. These results were suppressed by him in the second edition, doubtless in consequence of their not exhibiting so complete an accordance with observation as was manifest in his other researches on the lunar theory *. That Newton really was in possession of a method adequate to a complete investigation of the subject, is rendered still further probable by the recent researches of Mr. Adams, who, by the aid of geometrical considerations, analogous to those expounded with so much elegance in the Principia, has obtained results relative to the movement of the lunar apogee, which present a complete accordance with observation.

The fourth chapter is devoted to the early researches of geometers on the perturbations of the planets and the stability of the planetary system. While occupied with the former of these subjects, the illustrious Euler devised a method of investigation which must be regarded as one of the most remarkable in the annals of science. It consisted in regarding the perturbations of a planet as arising from an incessant change in the elements of its elliptic motion. This fertile idea was destined to acquire an immense developement from the labours of succeeding geometers.

The sublime results which the analytical researches of Lagrange and Laplace have disclosed, relative to the stability of the planetary system, while they have served to invest astronomical science with additional features of interest, are entitled to be classed among the noblest triumphs which the human mind has achieved in the investigation of the laws of the physical universe. The labours of these great geometers, which were of a kindred nature throughout their whole career, are on this occasion more especially interlaced. As some misapprehension appears to have not unfrequently arisen from this circumstance, I have endeavoured, by a careful reference to the volumes of the Academy of Sciences and other original sources, to exhibit the results independently arrived at by each geometer in the course of his researches on the subject.

The fifth chapter contains an account of the physical explanation of the great inequality in the mean longitudes of Jupiter and Saturn, and of the secular inequality in the mean motion of the Moon, as well as an allusion to several points of minor importance in the Theory of Gravitation. The irregularities in the mean longitudes of Jupiter and Saturn long continued to form an inexplicable enigma to geometers. In vain did Euler employ all the resources of his fertile genius in endeavouring to account for their existence by the principles of the Theory of Gravitation. Equally fruitless was the result of Lagrange's application of his commanding powers of analytical research to the subject. It was reserved for Laplace to detect the true origin of these anomalous phenomena in the mutual action of the two planets.

Perhaps a still more remarkable result, due to the same geometer, was the explanation of the secular inequality in the mean motion of the Moon. The records of certain eclipses of the Moon observed at Babylon about seven hundred years before the Christian era, when compared with observations of similar phenomena by the Arabian astronomers about the tenth

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* See Appendix IV.

century, seemed to indicate that the Moon's angular velocity round the Earth was subject to a slow acceleration. This fact was confirmed beyond all doubt by the observations of modern astronomers; but its existence seemed absolutely irreconcilable with the results to which geometers were conducted by their researches on the Theory of Gravitation. The physical cause of this acceleration continued to escape the analytical scrutinies of Euler, Lagrange, and Laplace, until at length the sagacity of the last-mentioned geometer led to its detection.

The sixth chapter is devoted to an account of the labours of geometers on the Figure of the Earth, the Precession of the Equinoxes, the Libration of the Moon, and other kindred subjects. By an ingenious application of his researches on the attraction of spheroids, Newton rigorously determined the ellipticity of the Earth, upon the supposition of its density being uniform, and of the figure of an oblate spheroid being compatible with the conditions of equilibrium of a fluid mass. The truth of the last-mentioned supposition was afterwards demonstrated by Maclaurin with all the elegance and rigour of the ancient geometry. With respect to the internal structure of the Earth, the ellipticity deduced from the measurement of arcs of the meridian was totally at variance with the supposition of its homogeneity. It was reserved for Clairaut to determine the ellipticity on the more probable hypothesis of the strata increasing in density towards the centre of the Earth.

The Precession of the Equinoxes is beyond doubt the most remarkable of all the perturbative effects by which the planetary system is characterised. Its original discovery as a sidereal phenomenon is due to the great astronomer Hipparchus. The explanation of its true character was first given by Copernicus, who shewed that it might arise from a conical motion of the Earth's axis. The question relative to the physical cause of this singular movement continued to be involved in impenetrable mystery, until at length Newton discovered its origin in the disturbing action of the Sun and Moon upon the redundant matter accumulated round the terrestrial equator. The subsequent discovery of the Nutation of the Earth's Axis by Bradley introduced a new cause of complication into the subject. The complete solution of the problem of the Earth's motion round its centre of gravity, by a rigorous application of the principles of Mechanical Science, was reserved for D'Alembert. The subject of the Libration of the Moon, which is noticed in the same chapter, exhibits another striking illustration of the comprehensive character of the Theory of Gravitation in assigning the physical explanation of the various phenomena relative to the movements of the celestial bodies. The researches of Newton on this subject were perfected by Lagrange, who succeeded in obtaining results which accorded in a most satisfactory manner with those deduced from observation.

The seventh and eighth chapters embrace a somewhat detailed history of the theory of Jupiter's satellites. In the seventh chapter I have given an account of the original discovery of these bodies by the illustrious

Galileo, and of the labours of subsequent astronomers in establishing the laws of their complicated movements. The eighth chapter exhibits a view of the researches of geometers, having for their object the explanation of these laws by the Theory of Gravitation. Some of the most curious effects of perturbation occur in this beautiful system. The results are mainly due to Lagrange and Laplace. The powerful character of the analysis which Laplace employed in these researches, is remarkably exhibited in the determination of the ellipticity of Jupiter by means of the derangements which the redundant matter accumulated round the equator of the planet occasions in the motions of the satellites. The illustrious geometer even boldly asserted, that the result thus derived from theory was entitled to greater reliance than that obtained by direct measurement with the micrometer!

The ninth chapter commences with a brief notice of the labours of geometers on some of the more hidden effects of perturbation. One of the most interesting of these is the gradual diminution of the obliquity of the ecliptic, occasioned by the disturbing action of the planets on the earth. The sublime results arrived at by Lagrange and Laplace, relative to the stability of the planetary system, assure us that such a diminution will not continue indefinitely, but that after a certain limit of obliquity has been attained, the angle contained between the planes of the ecliptic and the equator will then commence to open out. This process will continue until the obliquity attains a certain maximum value, when the increase will be converted again into a diminution, and thus the inclination of the two planes will continually oscillate between fixed limits prescribed by the intensity of the disturbing forces. It follows as a necessary consequence, that the climate of any particular country will not undergo an essential change from this cause, such as would inevitably ensue if the equator and ecliptic were ever to coincide, or to form with each other an angle of ninety degrees. Thus the more profoundly does analysis penetrate into the operations of nature, the more admirable is the harmony which appears to pervade her various arrangements.

The subject of Comets was one of the severest tests to which the Theory of Gravitation was submitted during the early period of its history. These bodies seemed to be so destitute of any coherent structure, and at the same time so capricious in their movements, that the attempt to make them the groundwork of strict investigation was long considered to be attended with insuperable difficulties. Newton, however, perceived, with characteristic sagacity, that, however evanescent might be the physical constitution of Comets, their material structure would subject them to the influence of the principle of Gravitation; and, in pursuance of this idea, he framed a theory of their movements, according to which they all revolved in orbits resembling one or other of the conic sections, having the sun in the common focus. The apparition of the great comet of 1680 furnished him with the means of obtaining a complete verification of his theory. By a rigorous discussion of its observed positions he demonstrated incontestably that the comet revolved in an orbit which sensibly coincided with a parabola, and that the line joining it and the sun described equal areas in equal times. Halley applied Newton's theory to a vast number of recorded observations of comets, and among the results to which he was led he arrived at the conclusion that the comet of 1682 would again pass through its perihelion in the year 1758 or 1759. The actual return of this celebrated comet, agreeably to the prediction of Halley, is familiar to every reader. The effects of planetary perturbation were calculated beforehand by Clairaut, who succeeded in fixing the time of return with remarkable precision. This was unquestionably one of the greatest triumphs which had yet been achieved in the developement of the Theory of Gravitation. The general theory of the Perturbations of Cometary Bodies was a few years afterwards simplified and improved by Lagrange.

The ninth chapter closes with a brief allusion to the *Mécanique Celeste*. The publication of that immortal work forms an important landmark in the history of Physical Astronomy. The Theory of Gravitation, after being subjected to a succession of severe ordeals, from each of which it emerged in triumph, finally assumes an attitude of imposing majesty, which repels all further question respecting the validity of its principles.

The tenth chapter introduces the second period in the history of the Theory of Gravitation. It commences with an account of the interesting results obtained by geometers, about the beginning of the present century, relative to the variations of the elements of the planetary orbits. The highly-refined method of investigation due originally to the genius of Euler, by which the perturbations of a planet are supposed to arise from a continuous variation of the elements of elliptic motion, was now carried to a state of unexampled perfection by Lagrange, and by the combined labours of that illustrious geometer and Poisson, was rendered applicable to all the great problems of the system of the world.

After a brief notice of some of the methods employed by modern geometers in their researches on planetary perturbation, the chapter closes with an account of the recent improvements in the lunar theory. The irregularities in the Moon's longitude, which, throughout the greater part of the nineteenth century, continued to occasion great embarrassment to astronomers and mathematicians, finally assumed a definite character, which rendered them a feasible subject of investigation with respect to their physical origin, when the vast mass of the Greenwich observations, extending from 1750 to 1830, were subjected to a comprehensive discussion by the present Astronomer Royal, and new corrections of the elements of the lunar orbit were deduced. Moreover, some hidden inequalities, which hitherto had totally escaped the notice of astronomers, and which seemed to be irreconcilable with theory, emerged from this important discussion. The explanation of the origin of these various anomalies by M. Hansen, forms an epoch of great importance in the history of Physical Astronomy. The complicated movements of the

Moon, which had occupied the attention of mankind from the earliest ages in the history of civilisation, upon which a long succession of illustrious astronomers and mathematicians had exerted their utmost powers of research, were at length completely analyzed, their laws clearly traced out, and the various resulting inequalities accounted for in strict accordance with the Theory of Gravitation. The consummation of this great achievement constitutes a new laurel in the wreath of the Royal Observatory of Greenwich, while it imperishably associates the already illustrious names of Airy and Hansen with the history of one of the most important departments of Astronomical Science.

The eleventh chapter is devoted to an account of the recent researches of geometers on the particular cases of perturbation which occur in the planetary system. Among the more important subjects which it embraces may be mentioned the discovery of the long inequality in the mean longitudes of the Earth and Venus, by Airy; the investigation of the perturbations of Halley's comet, on the occasion of its passage of the perihelion in 1835, by Rosenberger, Pontécoulant, and other geometers; the interesting researches of Le Verrier on various cases of cometary perturbation; the completion of Lagrange's labours on the Libration of the Moon, by Poisson; the determination of the ellipticity and mean density of the Earth, by Bessel and other enquirers; the final researches of Poisson on the motion of the Earth about its centre of gravity, and the invariability of the Sidereal Year; and the definitive detection of periodical oscillations in the Atmosphere depending on the perturbative influence of the Moon.

In the twelfth and thirteenth chapters I have endeavoured to give an account of the theoretical discovery of the planet Neptune, as it resulted from an investigation of the perturbations produced in the motion of Uranus. This may perhaps be regarded as the most astonishing conquest which the human mind has ever achieved in unfolding the arrangements of the material world. Nor does it tend to diminish our admiration of this great discovery, that it is due to the independent researches of two contemporary geometers, who, by methods totally dissimilar in their details, if not in their essential character, succeeded nearly about the same time in determining the position of the disturbing body. The brilliant researches of M. Le Verrier on this subject constitute the strongest title which he has yet earned to the admiration of the scientific world; while those of Mr. Adams, the other discoverer of the planet, may be justly regarded as the noblest tribute which he could offer to the memory of his illustrious countryman, the great founder of Physical Astronomy. Some remarks suggested by this discovery, which it would have been inconvenient to have inserted in the body of the work, will be found in an Appendix at the end.

The thirteenth chapter closes the history of Celestial Mechanics. Physical Astronomy, as usually understood, is confined to the researches of geometers on this subject; but in its more comprehensive sense it may be supposed to embrace the consideration of all the physical principles which are known to exercise an influence on celestial phenomena, as well as the study of those facts respecting the structure of the celestial bodies which admit of being explained by reference to established principles of physics. In accordance with this more enlarged signification, the subjects noticed in the greater portion of the remainder of the work ought to be considered as forming an essential part of Physical Astronomy.

The invention of the telescope about the beginning of the seventeenth century furnished the astronomer with an instrument of observation, the mighty efficacy of which can only be compared with the aid which the infinitesimal calculus affords to the geometer in his researches on the effects produced by the continuous agency of those forces which Nature employs in her operations. Armed with such an instrument, the sagacious Galileo was soon enabled to announce a multitude of discoveries in the heavens, of startling novelty and of the highest importance. Myriads of stars whose existence had eluded the scrutinies of the naked eye, were now seen to illumine the unfathomable regions of space. The investigation of the cosmical arrangement of the celestial bodies, and the study of their individual structure, were problems unexpectedly found to be within the reach of the human faculties. This department of astronomical science, no less remarkable for the sentiments which it is calculated to inspire respecting the grandeur of the material universe, than for the multitude of instructive and delightful views of the physics of the celestial regions which it unfolds, has been prosecuted with ardour by a succession of eminent astronomers from Galileo's time down to the present day.

The fourteenth chapter exhibits a view of the progress of researches on the physical constitution of the bodies of the solar system, and also includes an account of the various discoveries by which it has been enriched in modern times. The observations of the solar spots have suggested some highly interesting speculations respecting the great central body which forms the source of the light and heat of the system. The Moon, from her comparative proximity, has naturally given rise to much physical enquiry. The observations of the planets have disclosed a multitude of facts of a highly interesting character. Their rotatory movements round fixed axes with corresponding elliptical figures, and the diversified appearance of their surfaces, constitute striking points of analogy between them and the Earth. The remarkable phenomena visible in the polar regions of Mars, the belts of Jupiter and Saturn, and the wondrous rings of the latter planet, have all furnished abundant materials of observation and research. Nor are the satellites wanting in physical features of an important character. The relation of equality between their periods of rotation and revolution, which a variation of their brightness, in several instances, has served to establish, constitutes a striking point of analogy between them and the terrestrial satellite. The phenomena accompanying their transits and occultations are also suggestive of some interesting speculations.

This chapter introduces a name which occupies a prominent place in the remaining portion of the work—it is the immortal name of William Herschel. To the bulk of the intelligent class of readers this illustrious individual appears in the character of an astronomer distinguished by his skilful construction of huge telescopes, which he employed with marvellous success in exploring the heavens. To the student who has advanced within the precincts of astronomical science, he forms a more exalted object of admiration, as an observer of almost unrivalled acuteness and sagacity, whose exquisite faculty of discernment frequently enabled him to arrive at results far beyond the scope of the mere instrumental resources available to him; and as a philosopher of the highest order, who, by his originality of thought and capacity for comprehensive speculation, succeeded in establishing the principles of Sidereal Astronomy upon a broad and indestructible basis.

The fifteenth chapter contains an account of the progress of enquiry on the physical constitution of Comets. These mysterious bodies, beyond doubt, perform some important function in the economy of nature, which can only be ascertained by attentive observations of the phenomena which accompany their various apparitions.

The sixteenth chapter is devoted to those physical principles whose influence in disturbing the apparent positions of the celestial bodies, or in modifying the features of celestial phenomena, must necessarily be taken into account before astronomical observations can be rendered available as the groundwork of ulterior enquiry. It comprehends an account of the progress of researches on Precession, Refraction, Aberration, Nutation, Diffraction, and Irradiation. In the history of Refraction the mighty names of Newton and Laplace reappear with transcendant lustre. The correspondence between Newton and Flamsteed, published by the late Mr. Baily, has supplied some interesting materials connected with the researches of Newton on this subject. The uncertainty which so long existed respecting the construction of Newton's table of refractions, which Halley originally communicated to the Royal Society—whether it was based upon some physical theory of the subject, or whether it was calculated merely by an empiric process—has been effectually removed by the correspondence above referred to. It appears that Newton studied profoundly the theory of Astronomical Refraction, and succeeded in determining the results corresponding to various hypotheses respecting the physical constitution of the atmosphere. His suggestion to Flamsteed, recommending the practice of noting the indications of the barometer and thermometer, as a desirable accompaniment to astronomical observations, constitutes a striking illustration of the sagacity by which that great philosopher was distinguished above ordinary enquirers.

The subjects of Aberration and Nutation are introduced, with an account of the original discovery of these phenomena by the immortal

Bradley. The chapter closes with an account of the most important results which have been elicited by the labours of successive enquirers on the subject of the Irradiation of Light.

The seventeenth chapter is devoted to the history of the physical enquiries connected with eclipses of the Sun and Moon, the transits of the inferior planets, and other occurrences of a similar character. These phenomena are all influenced in so great a degree by Refraction, and the other affections of light, that it would have been inconvenient to have alluded to them at an earlier stage of the work. This chapter contains a somewhat detailed exposition of the most important facts which have been observed on the occasion of the various total eclipses of the Sun recorded in history, including an investigation of the conclusions which they are calculated to suggest respecting the physical constitution of the great central body of the planetary system. The subject of the transits of Venus has naturally suggested a brief notice of the life and labours of the lamented Horrocks. There is a deep interest associated with the fate of this youthful astronomer. Although dwelling in a remote district of Lancashire, in almost entire seclusion from the rest of the scientific world, he unquestionably arrived at a juster appreciation of Kepler's discoveries than any of the successors of that great astronomer had hitherto done; while the sagacity and originality of his views on various points relating to astronomy, his fertile and glowing imagination, and his ardent enthusiasm in the pursuit of science—all seemed to foreshadow a career of uncommon brilliancy, which a premature death unfortunately soon brought to a close. Even his brief labours, however, have assured to him a reputation which will live imperishably in the annals of science. By his own countrymen he cannot fail to be regarded with peculiar interest, as the morning star of a galaxy of men of genius, who continued for about a century to adorn these isles by their successful cultivation of the physico-mathematical sciences. The names of Horrocks, Gascoigne, Brouncker, Barrow, Wallis, Wren, Gregory, Hooke, NEWTON, Taylor, Bradley, Simpson, and Maclaurin, represent a constellation of scientific enquirers, which for splendour of genius and high intellectual endowments has never been surpassed, and rarely equalled, during a similar period in the history of any nation, whether of ancient or modern times.

As any work relating to astronomical science would be incomplete without some allusion to the important subject of observation, the eighteenth chapter has been devoted to a condensed account of the progress of Practical Astronomy, from the earliest ages down to the present time. The annals of physical science do not, perhaps, furnish a more interesting picture of gradual advancement towards perfection than that which exhibits the successive improvements effected in this department of astronomy—from the naked estimations of the Chaldean priests to the refined and complicated methods of observation practised by modern astronomers—from the gnomon and the clepsydra, in their most rudimentary forms, to the transit circle of the Greenwich Observatory, the pendulum clock of the most improved construction, and the electro-magnetic recording apparatus of the American astronomers.

In this chapter I have given a somewhat detailed account of the Royal Observatory of Greenwich, from its origin down to the present time. The observations which have emanated from that noble establishment have proved of incalculable service to astronomical science. No other similar institution, whether of ancient or modern times, can compare with it in this respect. Its history affords an instructive lesson regarding the advantage to be derived from applying the resources of an observatory to some definite object, and maintaining that object in view with unswerving constancy of purpose. An uninterrupted succession of eminent astronomers, who have directed the labours of this establishment, have contributed to render it the storehouse from which the materials for determining the elements of Astronomical Science have been mainly derived in modern times. With the triumphs of the Theory of Gravitation its history is inseparably associated. During the early period of its existence, it had the glory of supplying Newton with a series of observations, which served as a valuable guide to him while engaged in threading his way through the intricacies of the lunar theory, and it has continued ever since to furnish almost exclusively the astronomical facts, by an appeal to which the successors of that illustrious geometer have been enabled to establish the accuracy of their theoretical results. The recent reduction of the entire mass of the Greenwich Observations of the Moon and Planets, extending from 1750 to 1830, under the superintendence of the present Astronomer Royal, is an achievement which, while in respect of vastness it has few parallels in the annals of science, at the same time forms one of the most valuable acquisitions which Astronomy has received during the present century.

As a fitting sequel to the subject above-mentioned, the nineteenth chapter contains a brief account of the labours of astronomers in the construction of Catalogues of Stars. It is impossible to exaggerate the importance of this department of Astronomical Science. The places of the stars constitute so many fundamental facts, upon which depend all exact conclusions relative to the movements of the planetary bodies. The labours connected with their determination afford ample scope for talents of the highest order; but it must be acknowledged that they offer little to captivate either the imagination or the intellect, while at the same time they demand the most arduous exercise of the attention, and the most unflinching perseverance. Despite these disadvantages, there have not been wanting numerous examples of astronomers who, disregarding the *éclat* which usually attends discovery, have devoted the best portion of their lives to the construction of a Catalogue of Stars. Lacaille, Piazzi, and Groombridge will be especially remembered in the annals of astronomy, as individuals who sacrificed their days and nights with unwearied assiduity to this object, cheered only by the consciousness of the advantages which posterity would derive from their labours, and by the secret charm which a constant intercourse with nature never fails to yield.

The twentieth chapter contains the history of the Telescope. The researches of Van Swinden have recently contributed to throw much interesting light upon the original invention of that instrument.

The twenty-first chapter, which completes the work, is devoted to a condensed account of the progress of researches in Stellar Astronomy. The labours of modern enquirers in this department of Astronomical Science have led to some conclusions of a highly interesting and important nature. The existence of a sensible parallax in the fixed stars—a question which has occasioned much anxious investigation from the time of Copernicus down to the present day—has at length been definitively established in several instances by the labours of Bessel, Henderson, Struve, Peters, and Maclear. It is now ascertained beyond all doubt, that light, travelling at the rate of 192,000 miles in a second, would require three years and a half to traverse the space between one of the nearest of those luminaries and the earth! The motion of the solar system in space, is another of those sublime conclusions which have been established by the researches of modern astronomers. It appears from the labours of Sir William Herschel and his successors on this subject, that not only do the satellites move round the planets, and the planets round the Sun, but that the Sun, with his whole cortège of planets and satellites, is being constantly transported through space to a determinate point in the heavens, revolving, in all probability, round the centre of gravity of some vast system of suns, of which it forms one of the constituent members. Thus the farther the human mind is allowed to penetrate into the mechanism of the physical universe, the more overwhelming is the impression produced of the surpassing grandeur of its movements, and the more exalted is the conception formed of the Omnipotent Being who constantly presides over its countless arrangements.

To the student of Celestial Physics, the researches of astronomers on Double Stars offer a high degree of interest, inasmuch as they serve to demonstrate that the law of Gravitation, as announced by Newton, actually prevails in the mutual action of those remote bodies of the universe. The phenomena of Nebulæ excited little interest among astronomers until they attracted the attention of Sir William Herschel. The vast extent of that astronomer's observations of those objects, and the originality of his views on their physical constitution, had the effect of elevating them to a high degree of importance in sidereal astronomy. The subsequent labours of Sir John Herschel and Lord Rosse, in the same field of enquiry, have materially contributed to the advancement of our knowledge respecting those wonderful structures.

After a rapid view of the progress of research on the various subjects above mentioned, allusion is made to the labours of astronomers on the physical constitution of the Milky Way and the Distribution of the Stars in space. The chapter concludes with a brief account of the interesting speculations of M. Struve on the Extinction of Light in its passage through space.

In the prosecution of these labours I have generally endeavoured to elucidate the various facts of history by reference to the fundamental principles of astronomical science, adhering as closely as possible to the ordinary phraseology of language. Occasionally, however, the subject considered, does not naturally admit of concise elucidation, so that an adherence to this practice would have led to inconvenient—I had almost said interminable—digressions. On the other hand, to have omitted all allusion to such subjects would have been to sacrifice the principle of continuity which forms so essential an attribute of history, and to present the reader with an avowedly mutilated work. I have endeavoured to avoid these two extremes, by noticing every fact which seemed to constitute an essential link in the chain of historic exposition, but studiously aiming at conciseness in all those instances wherein explanation would be necessarily so prolix as to defeat its own object. This remark applies more particularly to the subjects relating to the Theory of Gravitation. With respect to the remaining portion of the work, it is to be hoped that no reader possessing an ordinary acquaintance with the elements of astronomical science can experience any difficulty in pursuing it through its details.

In a work demanding extensive research, and embracing a great variety of subjects, some of which are of a very abstruse nature, it is not pretended that imperfections may not be discovered. I may be permitted, however, to state, that it has not been without carefully consulting all the original authorities accesssible to me, and bestowing an attentive consideration upon each subject which it embraces, that I have ventured to submit this production to the judgment of the public. It is to be hoped that the accomplished reader will be enabled to discern in the following pages sufficient evidence of the justness of this statement, to induce him to regard with indulgence the shortcomings of the author in so far as his personal abilities are concerned.

It affords me sincere pleasure to have this opportunity of gratefully acknowledging my deep sense of obligation to Captain R. H. Manners, R.N., Secretary of the Royal Astronomical Society, whose kind encouragement and readiness in promoting my views I beneficially experienced on numerous occasions while engaged in preparing these sheets for the press.

R. GRANT.

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CHAPTER I.

Early notions of Physical Astronomy.—Newton.—His first Researches on the subject of Gravitation.—Cause of his failure.—Correspondence with Hooke.—Resumption of his previous Researches.—Law of the Areas.—Motion of a Body in an Elliptic Orbit, the force tending to the focus.—Picard.—His Measurement of an Arc of the Meridian.—Complete success of Newton's Investigation relative to the Action of the Earth upon the Moon.—His establishment of the principle of Gravitation in its widest generality.—Consequences he derived from it.—The Principia.—Account of the circumstances connected with its publication.—Halley, Hooke, Wren.—Synopsis of the subjects treated of in the Principia.—Laplace's opinion of its merits.

Attempts were made at an early period in the history of astronomy to account for the motions of the celestial bodies by means of some common principle. The Greeks, as might be expected, were the first people who invented a physical theory of the heavens; but the result of their speculations in this instance was totally unworthy of their high intellectual character. Conceiving that the constant succession of phenomena in the same order could only be effected by means of some material agency, they supposed each of the planets to be inclosed in a solid sphere of transparent structure, having the earth situate in the centre. The motion of the planet was then supposed to be accomplished by the revolution of the entire sphere in the direction of the planet's real motion, and with a velocity corresponding to its periodic time. In order to account for the various irregularities of its motion, each of the planets was provided with several spheres, which modified each other's effects; and at an immense distance beyond the planetary apparatus was situated the *primum mobile*, or sphere of the starry heavens, which revolved from east to west in twenty-four hours, carrying along with it all the fixed stars. It certainly affords a remarkable illustration of the proneness of the human mind to ascend from the phenomena of nature to some ulterior cause, that this monstrous theory should have commanded the assent of the learned world until the close of the sixteenth century. Aristotle introduced it into his system of philosophy, and by this means it came to be generally adopted as part of the ancient astronomy. We must not, however, confound this offspring of the imagination with the epicyclical theory of Hipparchus, which, although involving certain gratuitous principles, was notwithstanding framed in accordance with observation. The latter, in fact, was a pure mathematical theory, devised for the purpose of representing the motions of the planets, without reference to the physical cause of those motions; and, although incomplete in its structure, in so far as it took no cognizance of the distances of the planets, still, as it could be submitted to a rigorous calculus, it held out to astronomers the prospect of arriving at the true system of nature by means of a comparison of its results with those of observation. The history of the two theories presents us, indeed, with an instructive lesson of the value of an hypothesis which contains *some* elements of truth as contrasted with the inanity of a mere fiction of the mind. The mathematical theory, besides affording admirable scope for the inventive powers, had the advantage of enabling astronomers throughout a long course of ages to predict the places of the planets with tolerable accuracy; and, finally, was instrumental in conducting Kepler to a knowledge of their real motions: the physical theory, on the other hand, continued during an equal period to mislead men's minds, without possessing the redeeming merit of forming a subject of intellectual exercise; and, when it was at length overthrown by the invincible force of reasoning based upon facts, it disappeared without leaving a single trace of its existence behind.

It is difficult to ascertain what were the real opinions of Copernicus relative to the physical constitution of the heavens. While engaged, however, in establishing the Pythagorean system of the world, he was led to use a remark which may be said to contain the earliest notion of the principle of gravitation. The Aristotelians had asserted that heavy bodies, to use their own phraseology, naturally tend towards the centre of the universe, and as observation showed that a similar tendency existed towards the centre of the earth, they hence concluded that the earth must be placed immoveable in the centre of the universe. Copernicus, however, remarked that the parts of matter had a natural appetency to congregate together and unite in the form of spheres, and that the constant tendency of bodies towards the centre of the earth was merely a sensible manifestation of this inherent quality of matter.

Tycho Brahé was not endowed with qualities favourable to speculation, but he deserves to be mentioned in the history of physical astronomy, on account of the effect of his researches in leading to the overthrow of the ancient theory of solid orbs. By means of a series of careful observations on the comet of 1577, he discovered that it was at least three times more remote from the earth than the moon is; whence it followed, since comets traverse the celestial regions in all directions, that the heavens could not be composed of a solid mechanism such as the Aristotelians had imagined.

Gilbert, an English philosopher of great merit, who flourished towards the close of the sixteenth century, was one of the first persons who arrived at general notions on the subject of gravitation. His researches on magnetism, pursued in strict accordance with the principles of the inductive philosophy, were much esteemed by Kepler and Galileo, both of whom profess to have been greatly indebted to him for their views on that subject. In his treatise on the magnet, published in 1600, he explains the influence of the earth upon the moon by comparing the former to a great loadstone. He announces his opinions, however, much more explicitly in his posthumous work on the "New Philosophy,"* which first appeared about the middle of the seventeenth century. In this treatise, he asserts that the earth and moon act upon each other like two magnets; but he considers the influence of the earth to be greater than that of the moon, *on account of its superior mass.* It is important to note his explanation of the mode in which the two bodies affect each other. "It is not," says he, "so as to make the bodies unite like two magnets, but that they may go on in a continuous course." In another part of the same work, he ascribes the tides partly to the influence of the moon. "The moon," says he, "does not act on the seas by its rays or its light. How then? Certainly by the common effort of the bodies, and (to explain it by something similar) by their magnetic attraction." He seems to have been more perplexed in accounting for the ebb of the tide than for its flow. In order to explain this part of the phenomenon, he assumes that, besides the waters of the ocean, the earth contains subterranean humours and spirits, which are drawn out by the attraction of the moon; and, when that body has retired, are then absorbed again into the bowels of the earth. "The moon," says he, "attracts not so much the sea as the subterranean spirits and humours, and the interposed earth has no more power of resistance than a table or any other dense body has to resist the force of a magnet."

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* De Mundo Nostro Sublunari, Philosophia Nova, Amstelodami, 1651.

The preceding remarks of Gilbert contain unquestionably one of the earliest traces which is to be found among the writings of modern authors, of the notion of an attractive force acting between the bodies of the solar system. The moon's attractive influence upon the earth is naturally enough suggested by the phenomenon of the tides; but the influence of the earth upon the moon is mixed up with a great deal of error and confusion. It appears to him to be indicated not by the revolution of the moon in a curvilinear orbit round the earth, but by her accompanying that body in a continuous course round the sun. In fact the principle of terrestrial attraction is suggested by the notion of the earth *dragging* the moon along with her in her annual orbit. Finding himself utterly unable to account for the mutual attraction of the earth and moon, without the continual approach and ultimate union of the two bodies, he attempts to get rid of the difficulty by shifting his hypothesis, or, in other words, by asserting that the effects resulting from the mutual influence of the two bodies is not similar to the effects of magnetic attraction. Although Gilbert, therefore, deserves much credit for the sagacity with which he recognised, to a certain extent, the principle of gravitation, his ideas of it are so vague and inconsistent, that his speculations cannot be said to rise above the merit of mere conjectures.

Kepler, in the introduction to his "Astronomia Nova," published in 1609, announces the mutual gravitation of matter in very remarkable terms. He asserts, as Copernicus had already done, that bodies do not tend towards the centre of the earth, because it is the centre of the universe, but because it is the centre of a round body of the same nature with themselves. If two stones were situated in space beyond the influence of a third body, they would approach towards each other like two magnetic needles, and would meet in an intermediate point, each passing through a space proportional to the comparative mass of the other. If the moon and earth were not retained by their animal force, or some other equivalent, the earth would mount to the moon by a fifty-fourth part of their distance, and the moon would fall to the earth through the other fifty-three parts, and they would there meet. If the earth should cease to attract the waters to itself, all the waters of the sea would be raised, and would flow to the body of the moon.

These remarks are indeed very striking, and show how profoundly their illustrious author could penetrate into the secrets of nature; but we should not be justified in attaching to them all the importance due to a distinct recognition of the principle of gravitation. In his ideas and reasoning he coincides with Gilbert, except that he extends the principle of gravitation to the whole material universe. The difficulty which Gilbert experienced in accounting for the constant separation of the moon and earth, notwithstanding their mutual attraction, occurs with its full force to Kepler. The latter, however, gets over it not as Gilbert had done, by assuming a principle inconsistent with his previous ideas on the subject, but by supposing the terrestrial attraction to be neutralized by the animal force of the moon or some other equivalent. It is clearly possible to establish any principles whatever, if we are at liberty to have recourse to such assumptions in support of our reasoning. It will be remarked that Kepler does not seek to explain how the motion of the moon in her orbit is continually *kept up;* he doubtless assigned this task to the animal force which regulated the distance between the two bodies. The difficulty of accounting for the motion of a body in its orbit, by means of a centripetral force, occurs to him perpetually throughout the Astronomia Nova in course of his speculations on the physical cause of the planetary motions. In attempting to explain the phenomena of these motions by means of a force emanating from the sun, he is now compelled, like Gilbert, to introduce a principle totally at variance with his previous notions of gravitation; for he imagines that the planet requires to be continually impelled in its orbit by the solar force. To meet this view of the case, he supposes the sun to revolve from west to east, upon an axis perpendicular to the plane of the ecliptic, and to send forth continually magnetic rays, which attract the planet in a direction transverse to the line joining it and the sun.

It is hardly necessary to state that this opinion of the planets being kept revolving by a force continually whirling them round in their orbits is not only at direct variance with the character of a gravitating force, but is also inconsistent with the fundamental principles of motion. It must be admitted that there was more of truth in Ross's words than he could perhaps justly take credit for, when he asserted that "Kepler's opinion, that the planets are moved round by the sunne, and that this is done by sending forth a magnetic virtue, and that the sunbeames are like the teethe of a wheele taking hold of the planets, are senselesse crotchets fitter for a wheeler or a miller than a philosopher." *

Kepler might have formed more accurate ideas on the physical cause of the planetary motions, if the science of mechanics had been more advanced in his time; but it is surprising that, although he constantly strove throughout his researches on the planet Mars, as detailed by him in the Astronomia Nova, to connect the varying motion of the planet with a force emanating from the sun, he nowhere speculates so judiciously on that force as in the introduction to his work; and at the conclusion of his labours he inspires no more confidence in his reader respecting the reality of the force than he did at the commencement of them. In fact, it is to the extraordinary tenacity with which he clung to the idea of a solar force acting somehow on the planets, and his strong conviction that their motions were regulated by fixed laws, that we must ascribe the brilliant result of his researches, rather than to any clear perception either of the nature of the force or of its mode of operation.

It is difficult to say whether Gilbert or Kepler was first led to speculate on the physical theory of the celestial motions. Kepler's earliest notions on the subject are to be found in his "Mysterium Cosmographicum," which was published in 1596. Gilbert's "Treatise on the Magnet" appeared in 1600, and he died in 1603, leaving behind him his posthumous work, which was published only in 1651. It is clear from the nature of Gilbert's ideas, which turn entirely upon the magnet, that they could not have been suggested to him by Kepler's speculations. It is equally certain that the latter was not indebted to any person for his opinion relative to the existence of some physical principle directing the motions of the planets. When, however, he attempted at a subsequent period of his researches to devise a consistent theory of the solar force, he adopted the views of Gilbert by assuming it to be a modified form of magnetism. This appears from his great work, the "Astronomia Nova,"* wherein he cites the opinion of Gilbert while proceeding to frame his theory of a whirling force.

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* The New Planet no Planet, or the Earth no Wandering Star, 4to., London, 1646. See also Life of Kepler.—L. U.K.

Galileo, by means of his admirable researches on mechanics, contributed in a high degree towards the formation of more distinct ideas on the subject of curvilinear motion. The principle of mutual gravitation does not seem, however, to have found any favour with him, for he censures Kepler on account of his opinion relative to the attraction of the earth by the moon. He admitted the attraction of the moon by the earth, but he by no means formed a distinct conception of the mode in which the force of gravity in this case operates. "The parts of the earth,"† says he, "have such a propensity to its centre, that when it changes its place, although they may be very distant from the globe at the time of the change, yet must they follow. An example similar to this is the perpetual sequence of the Medicean stars, although always separated from Jupiter. The same may be said of the moon obliged to follow the earth."

The earth's attraction is here evidently inferred from the moon constantly attending her in her annual orbit round the sun. It might, however, be concluded from the same phenomenon, with equal shew of reason, that the moon attracts the earth; for the moon cannot be said to *follow* the earth any more than the earth can be said to follow the moon, since, in fact, both bodies, while revolving round the sun, revolve also continually round their common centre of gravity. The grand fact which leads to the establishment of the action of the earth upon the moon, consists in the revolution of the latter in a *curvilinear orbit which is concave with respect to the earth.* It has been sometimes said that Kepler only required a more complete knowledge of the laws of motion in order to have demonstrated the existence of the principle of gravitation. Here, however, we have a philosopher equal in sagacity to Kepler—who had successfully analyzed the phenomenon of curvilinear motion in one of its manifestations at least, and who moreover had access to the opinions of Kepler on the subject of gravitation; still, notwithstanding all these advantages, he failed to recognise the existence of an attractive force, either in the motion of the moon round the earth, or in the motions of the planets round the sun. This circumstance ought to render us cautious in attaching an undue value to mere sagacious surmises unsupported by legitimate proof, and in ascribing to individuals any credit for discoveries which are not the actual result of their own labours.

We do not propose to make any further allusion to Descartes' theory of vortices, beyond the few words we have already said respecting it in the introduction to this work. No doubt, we think, can exist that this celebrated fiction exercised a most pernicious influence in retarding the progress of sound mechanical ideas relative to celestial physics. Like the theory of solid orbs, it at length utterly disappeared before the advancing light of true science, after continuing for nearly a century to indulge its adherents with the miserable delusion that it revealed to them the whole secret of the mechanism of the universe.

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* Astronomia Nova, cap. xxxv., p. 176.

† Dialago sopra i due Massimi Sistemi del Mondo.—Firenze, 1632. See also Life of Galileo.—L. U. K.

Borelli, in his theory of the Medicean stars, published in 1666, appears to have speculated more judiciously on the physical theory of the planets than any of his predecessors. He remarks that the motions of the planets round the sun, and those of the satellites round their respective primaries, must doubtless depend in each case on some virtue residing in the central body. He seems to have arrived at pretty accurate notions of the motion of a body in a circular orbit. He remarks that bodies so revolving have a tendency to recede from their centre of revolution, as in the case of a wheel revolving on its axle, or a stone whirled by a sling. When this force is equal to the tendency of the body *to* the centre, a compensation of effects takes place, and the body will neither approach nor recede from the centre of force, but will continually revolve round it.

Here, for the first time, an attempt is made to account for the motion of a body in a circular orbit, by means of a force directed continually *to* the centre of the circle. It must be admitted, however, that Borelli's explanation is at once imperfect and indistinct. He does not analyze the phenomenon of curvilinear motion into its constituent elements, but merely seeks to establish the necessity of a constant central force by an appeal to experiment. He rightly asserts that the body tends continually to recede from the centre, but he gives no account of the origin of this centrifugal force: nor does he explain by what means the motion of the body in its orbit is continually *kept up*. His account of the last-mentioned part of the phenemenon is so obscure, that it is quite evident he had obtained only a very weak hold of the problem. After remarking that the compensatory effects of the two constant forces will maintain the body at a determinate distance from the centre, he then says, "therefore the planet will appear balanced and floating on the surface."*

Although Borelli's speculations possessed much merit, still they were not sufficiently clear to lead to any measurable results, and until a complete dynamical view of the problem of centripetal forces could be obtained, it was obviously hopeless to attempt its mathematical solution. Without stopping here to notice the partial researches of Hooke, Huygens, Wren, and Halley†, we shall at once proceed to give some account of the immortal discoveries of NEWTON. This illustrious philosopher was born in the year 1642, at Woolsthorpe, in the county of Lincoln. Before attaining the years of maturity he made a multitude of beautiful discoveries in Analysis, and was even in possession of the method of Fluxions when he was only twenty-four years of age. He was now about to enter upon a field of speculation which was destined to offer a magnificent theatre for displaying the resources of that powerful instrument of investigation. Pemberton states that Newton, having quitted Cambridge, for Woolsthorpe, in 1665, to avoid the plague, was sitting one day in his garden, when he was led to reflect on the principle which causes all bodies to tend towards the centre of the earth. As this tendency did not appear to suffer any sensible diminution on the tops of the highest buildings, or even on ascending the loftiest mountains, it occurred to him that it might possibly extend to the moon; and, if it did, might be the cause which retained that body in her orbit. Pursuing his meditations, he was led to imagine that a similar force directed continually towards the sun might retain the planets in their orbits. But a question naturally suggested by this generalization of his ideas was this—Did the solar force act with the same intensity on all the planets, or did it diminish with the distance from the centre, as the slower motion of the more remote planets seemed to indicate? His next step, therefore, was to determine, by a mathematical investigation, the magnitude of the force which retains a body in a circular orbit, the force being continually directed to the centre of the circle. The solution of this problem gave him an expression for the centripetal force in terms of the velocity of the body in its orbit and its distance from the centre, or, which amounts to the same thing, in terms of the periodic time and the distance. Hence, when the relation between these two elements was known, it was easy to express the force in terms of the distance alone, and by this means to ascertain the law according to which it varied. Now, Kepler had shown that the squares of the periodic times of the planets are proportional to the cubes of their distances from the sun; Newton hence inferred that the planets are retained in their orbits by a force directed towards the sun and varying inversely as the square of the distance from his centre.

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* "Ideoque planeta libratus apparebit et supernatans." Theoricæ Mediceorum Planetarum in causis physicis deductæ. Florentiæ, 1666.

† We shall have occasion to notice incidentally in the following pages the labours of these philosophers on the subject of centripetal forces. Newton commenced his researches at least as early as any of his contemporaries; nor does it appear, throughout all this career, that he was indebted to one or other of them for any of his ideas.

The result of Newton's investigation relative to the law of attraction was strengthened by the analogy which other natural emanations from centres offered: but it would manifestly have received a vast accession of support if it were found that the attraction exerted by the earth upon the moon, when compared with her attraction of objects at the surface, diminished also according to the same law of the distance. The solution of this question might; therefore, now be considered as the *experimentum crucis* which was to decide whether Newton had penetrated into the secret of the celestial motions, or whether he had been occupying his mind with speculations of a purely mathematical nature. Now, the force which determines the descent of a body at the surface of the earth is measured by the space through which it falls into a given small portion of time; and the force which retains the moon in her orbit is measured by the versed sine of the small arc described by her in the same time; for, if no force had acted, the moon would have proceeded in the direction of a tangent to her orbit, and the versed sine being the measure of deflection from the tangent, indicates, therefore, the intensity of the deflecting force. It is obvious that, in order to compare these two small spaces, they must both be expressed in terms of the same unit, as a foot for example. Now, the versed sine of the lunar arc is readily expressed in terms of the radius of the orbit, and again the latter is derivable from the earth's radius by means of the lunar parallax. The question relative to the comparison of the two forces is, therefore, finally reduced to the determination of the distance in feet, between the centre of the earth and the surface. This object may be very readily effected when once the length of a given arc of the meridian is known; but, at the time we are considering, this point was by no means accurately ascertained. Newton employed in his calculation the rough estimate of 60 miles to a degree, which was in use among geographers and navigators; whereas the real length of a degree is about 69½ miles. It may hence be readily inferred that the result obtained by him did not satisfy his expectations. Having determined the earth's force upon the moon by diminishing the gravity of bodies at the surface in the ratio of the square of the distance from the centre, and then compared the result with the force indicated by the motion of the moon in her orbit, he found that, instead of the two quantities being exactly equal, the former exceeded the latter by about one-sixth. Deeming this discordance too great to justify his bold surmise, he laid the investigation aside, doubtless with the intention of reconsidering it at some future time.

Newton's attention was again called to the subject of centripetal forces, by a letter he received from Hooke, in 1679, relative to the path described by a projectile, taking into account the effect of the earth's diurnal motion. Hooke was unquestionably endowed with a genius of a very high order; but, partly from the desultory character of his researches, and partly from his deficiency in mathematical skill, he has not achieved results by any means commensurate with his great acuteness and originality. As early as the year 1666 he had illustrated, by means of a beautiful experiment, the motion of a body revolving in an ellipse under the influence of a force directed continually to the centre; and, in his letter to Newton on the occasion above referred to, he declared that, if gravity decreased according to the reciprocal of the square of the distance, the path of a projectile would be an ellipse, having the centre of the earth in the focus. Although this assertion was unaccompanied by any proof, and consequently did not possess any merit beyond that of a sagacious conjecture, still it excited a strong interest in the mind of Newton, who had already devoted much attention to the subject of central forces. His researches had hitherto been confined to bodies revolving in circular orbits: he now proposed to investigate the vastly more difficult question of a body revolving in an orbit of variable curvature.

Considering generally the motion of a body projected in free space, and exposed to the incessant action of a force tending towards a fixed centre, he arrived at the remarkable conclusion, that an imaginary line joining the centre of force and the body would constantly sweep over equal areas in equal times. Now Kepler had found that the planets revolve round the sun precisely according to this law; it followed, then, that all these bodies were retained in their orbits by a force directed continually to the centre of the sun.

It still remained for Newton to investigate the law of the force corresponding to the variation of the distance in the same orbit. According to Kepler's first law, the planets move in ellipses, having the sun in one of the foci. The question, therefore, was to determine the law of the force by which a body is compelled to revolve in an elliptic orbit, the force being continually directed to one of the foci of the ellipse. This problem is of a much more complex character than the similar one relative to a circular orbit. In order to form some idea of the difference between the two cases, we may remark generally, that when a body has once received an impulse in any direction, it would persevere with a uniform motion in the direction of the impulse, if it were not exposed to the influence of any extraneous force. Now, when a body revolves in a curvilinear orbit, it is continually changing the direction of its motion; this is, therefore, a clear proof that it is acted upon by some force which continually deflects it from the tangent to the orbit in the direction of which it is every instant naturally endeavouring to move. Now, the force required to retain a body in a curvilinear orbit at any given point depends partly on the curvature of the orbit and partly on the velocity with which the body is moving; for, with the *same* velocity, but a *greater* amount of curvature, the body will require to be deflected in a *given* time through a greater space, and therefore the deflecting force must be more intense; and again, for the *same* amount of curvature, but a *greater* velocity, the body will be deflected in a *less* time through the same space, and therefore in this case also the force will be more intense. In order that the centripetal force may retain the body in its orbit without producing any other effect, it is necessary that it should constantly act at right angles to the tangent; for, if it act in an oblique direction, it will be partly expended in increasing or diminishing the tangential motion, according as the body is approaching to, or receding from, the centre of force. Now, when a body is compelled to revolve in a circular orbit by a force tending continually to the centre of the circle, the direction of the force is constantly perpendicular to the tangent; and therefore the force neither accelerates nor retards the body, but simply retains it in its orbit. The velocity of the body will, therefore, continue uniform, and, since the curvature of a circle is also uniform, it follows, from what we have already stated, that the centripetal force will have the same intensity for every point of the orbit.

But the question is much more complicated when we consider the motion of a body in an elliptic orbit. In this case, the force acts in an oblique direction with respect to the tangent at every point of the orbit except the two extremities of the major axis, and hence it is constantly expended, partly in deflecting the body into its orbit, and partly in accelerating or retarding the tangential motion. The velocity being therefore variable, and the same being true with respect to the curvature of the ellipse, it follows that the deflecting force which depends upon these two elements is also subject to continual variation. This force, however, which constantly acts at right angles to the tangent, can only be increased or diminished by means of a corresponding change in the intensity of the centripetal force, of which it forms one of the resolved parts. It follows, therefore, that the centripetal force varies not only from being more or less effectual in retaining the body in its orbit, but also because the elements upon which the effective part depends are also in a state of continual variation *.

The preceding remarks may serve in some degree to show the peculiar difficulties of the problem which now suggested itself to Newton. Enveloped as it was in complications and obscurities, his inventive genius devised the means of its solution, and he found that the centripetal force varied inversely as the square of the distance from the focus of the ellipse. This result accorded in a most satisfactory manner with the conclusion to which he was conducted by his previous researches, founded on the supposition of the planets revolving in circular orbits. Assuming the solar force to extend to the remotest planets, and to vary everywhere according to the inverse square of the distance from the sun, he demonstrated that the squares of the periodic times of the planets would be proportional to the cubes of their mean distances. This was the third of Kepler's famous laws of the planetary motions. It followed, therefore, that the law of the inverse square of the distance was true, not only when the distances related to the same orbit, but even when they were compared in different orbits. He had already arrived at this conclusion, by assuming the orbits to be circular, and now he found it to be demonstrable for the more rigorous case of elliptic orbits.

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* The resistance offered by a body to move in a curvilinear orbit has been termed its centrifugal force; it is therefore equal, and opposite to, the resolved part of the centripetal force, which acts perpendicularly to the tangent. Hence, when a body revolves in a circular orbit by means of a force directed to the centre of the circle, the centripetal and centrifugal forces will be equal; but, in every other case, the latter of these forces will exceed the former, and will tend not to the centre of force, but to the centre of the circle of curvature, corresponding to the infinitely small arc of the orbit in which the body is moving at the given instant. It is obvious that the centrifugal force has no positive existence; it merely arises from the resistance offered by the inertia of the body, in virtue of which the latter tends constantly to persevere in a straight line.

Notwithstanding the satisfactory nature of Newton's researches relative to the planets, the law of gravitation appeared to his cautious mind to be imperfectly established, so long as the serious discordance offered by the moon remained unexplained. A circumstance, however, had recently occurred, which induced him to suspect that the cause of this discordance lay in assuming an erroneous value for a degree of the meridian. We have mentioned that, in computing the earth's semi-diameter, he used the commonly received estimate of 60 miles to a degree. Picard, the French astronomer, however, having in the intermediate period measured an arc of the meridian with great care, and obtained a result considerably different, he resolved to repeat his previous calculation by means of it. To his unspeakable delight he now obtained a result which completely harmonized with his researches on the planets. Assuming that the semi-diameter of the lunar orbit was equal to 60 semi-diameters of the earth, he found that the space by which the moon is deflected from the tangent to her orbit in one minute is exactly equal to the space through which bodies at the earth's surface fall in one second. In order to appreciate the conclusiveness of this result, we may remark that, when a body is acted upon by a continuous force during a small portion of time, the space described by it in consequence varies in the direct ratio of the force and the square of the time. Hence if the force be supposed to vary in the inverse ratio of the square of the distance, the space will vary as the square of the time directly and the square of the distance inversely. It is clear, then, that when two bodies are placed at unequal distances from the centre of force, the minute spaces through which they are drawn by the force can only be equal, when the time, during which the more remote body is under the influence of the force, exceeds the corresponding time of the nearer body, in the same ratio in which its distance from the centre exceeds the corresponding distance of the other. Conversely, if two bodies fall through equal spaces in times which are to each other in the direct ratio of the distances from the centre of force, we may conclude that the force varies in the inverse ratio of the square of the distance *. Now, Newton assumed in his calculation that the moon is 60 times more distant from the centre of the earth than objects at the surface; and he found that the time occu-

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* Let f f' be the force of gravity at the earth's surface and at the moon, d d' the corresponding distances from the earth's centre, s s' the minute spaces through which bodies would fall at those distances in the times t t'; then, as mentioned in the text, we have $s=a\,f\,t^2$, $s'=a\,f'\,t'^2$ a being a constant quantity. Now, if we assume with Newton, that $s=s'$, we have $f\,t^2=f'\,t'^2$; hence $f : f' :: \frac{1}{t^2} : \frac{1}{t'^2}$. But Newton found that $t : t' :: d : d'$; therefore $\frac{1}{t^2} : \frac{1}{t'^2} :: \frac{1}{d^2} : \frac{1}{d'^2}$, and consequently, $f : f' :: \frac{1}{d^2} : \frac{1}{d'^2}$.

pied by her in falling through a given space * was exactly 60 times greater than that occupied by a body at the earth's surface in falling through an equal space. It thus appeared that the force which retained the moon in her orbit, as deduced from her actual motion, was less than the force of gravity at the earth's surface, in the exact ratio of the inverse square of the distance from the centre of the earth †.

When Newton had thus satisfied himself by indisputable evidence that he had discovered the true law of gravitation, he proceeded to investigate more profoundly its real character. He had found that the planets gravitate towards the centre of the sun, and the satellites towards the centres of their respective primaries, but it did not escape his sagacity that these points could not of themselves exert any physical influence; and that the attractive force was directed towards them solely in consequence of the mass of material particles which in each case surround them. He was thus led to regard the principle of attraction as residing in the constituent particles of the attracting body, and to conclude that the tendency of the force to the centre was no other than the resultant of all the molecular forces acting with unequal intensities and in different directions. In order to establish this important fact, it was necessary for him to investigate the nature of the attraction exercised by a mass of particles agglomerated in the form of a sphere; for observation shewed that all the heavenly bodies were spherical, or very nearly so. In the course of these researches he was conducted to the remarkable conclusion that, if the sphere were of uniform density, or even if it consisted of concentric strata of uniform density throughout each stratum, but differing in density from one stratum to another, the combined effect of the attraction of all the molecules would be the same, both in intensity and direction, as if the whole mass had been collected at the centre. This result afforded a most satisfactory explanation of the fact that, in accounting for the motion of the planets by a solar force, varying according to the inverse square of the distance, it was in all cases found necessary to measure the distance from the centre of the sun; and the same explanation applied to the motions of the satellites round their respective primaries.

Having thus assured himself that the tendency towards the central body was due to a quality inherent in the constituent particles, and not to any virtue residing in the centre, he naturally was led to suppose that this tendency must be mutual for all the parts of matter, and that as the sun attracts the planets, and the planets the satellites, so, in like manner, the planets attract the sun, and the satellites the planets, and even objects at the surface of the earth attract the earth. The equality of action and reaction, which was strikingly illustrated in all the other relations of the material world, rendered this proposition self-evident; nor did his sagacity fail to discover sensible manifestations of this principle in the irregular movements of the celestial bodies, especially in those of the moon ‡. He

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* The force which retains the moon in her orbit is here supposed to act in the same direction during a very short space of time. This supposition is not strictly true, but for a very small arc of the lunar orbit it cannot sensibly affect the final result.

† It is said that Newton became so much agitated as soon as he began to suspect the probable result of his calculation, that he was compelled to assign to a friend the task of bringing it to a conclusion.

‡ Cotes, in his admirable preface to the second edition of the Principia, demonstrates in the following simple and convincing manner that the action of gravity is equal on both sides:—" Let the mass of the earth be divided into any two parts whatever, either equal or anyhow unequal; now, if the weights of the parts towards each other were not mutually

therefore finally arrived at the conclusion, that *every particle of matter in the universe attracts every other particle, with a force varying inversely as the square of their mutual distances, and directly as the mass of the attracting particle.*

When Newton had thus ascended to the principle of gravitation in its most comprehensive form, he devoted the whole energies of his vast intellect to the unfolding of its consequences; and, with a sagacity and power of investigation unexampled in ancient or modern times, he succeeded in tracing all the grand phenomena of the universe to its agency. Considering generally a body projected in free space, and exposed to the action of a central force, varying according to the inverse square, of the distance, he demonstrated, by means of a beautiful geometry which he had specially invented for such researches, that the body would revolve in a curvilinear orbit which would be some one of the conic sections. It *might* be a circle, an ellipse, a parabola, or an hyperbola, but it *must* necessarily be one of them—the question as to the particular species of curve depending entirely on the primitive position of the body, and the velocity of the impulse. He showed that, when once the initial distance and the velocity and direction of the impulse were given, not only the conic section in which the body would move was readily assignable, but also the magnitude, position, and form of the orbit. Applying these principles to the motions of comets, he discovered that these bodies, like the planets, are retained in their orbits by the attraction of the sun; and he invented a method for determining the elements of a comet's orbit, by means of three distinct observations.

He perceived that, while the planets and satellites are mainly influenced by the attraction of the central bodies round which they revolve, they are also liable to be disturbed in their motions by their mutual attraction. Considering the moon as disturbed by the sun in her orbit round the earth, he found that the action of that body would account for the numerous inequalities which astronomers had from time to time detected in her motion. He demonstrated that the mean effect of such a disturbing force would be to cause the apsides to *advance* in the direction of the moon's motion, and the nodes to *regress* in the opposite direction, both of which results are conformable to observation; nor did he stop here, but actually computed the exact quantity of many of the most important of the lunar inequalities. He discovered that the mutual gravitation of the molecules composing the earth's mass, combined with the centrifugal force generated by her motion round her axis, would cause her to be flattened at the poles. Assuming the actual figure to be an oblate spheroid, he assigned the ratio between the polar and equatorial axes, and determined the law of gravity at the surface. With a sagacity almost divine, he perceived that the action of the sun and moon upon the redundant matter accumulated at the equator, would produce the slow conical motion of the earth's axis which occasions the Precession of the Equinoxes, and he indicated the quantity of the motion due to each of the two disturbing bodies. He shewed, also, that the attraction of the sun and moon, by elevating the waters of the ocean, would continually disturb their equilibrium, and would thereby give rise to the phenomenon of the Tides. Finally, what is

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equal, the lesser weight would give way to the greater, and the two parts joined together would continue moving in a right line *ad infinitum,* towards the part to which the greater weight tends; a result which is entirely contrary to experience."

perhaps the most astonishing of all the results to which he was conducted by his theory, he found that the quantities of matter contained in the heavenly bodies might be ascertained by observing the effects of their mutual attraction. By means of this principle, he was enabled to compare the mass of the sun with the masses of those planets that are accompanied by satellites, and also to compare the mass of the moon with that of the earth*.

Newton has given a full exposition of these sublime discoveries in his immortal work, the Principia. As the appearance of this work was destined to introduce a new era in science, it may not be uninteresting to mention briefly the circumstances connected with its publication. Newton does not appear to have contemplated communicating to the world his researches on the subject of gravitation until the occasion of a visit paid him by Dr. Halley in 1684. About the beginning of that year, Halley had discovered, by means of Kepler's third law, that the centripetal force for circular orbits varied according to the inverse square of the distance. This result gave him the law of the solar force from one orbit to another, on the supposition that the planets move in circles, with the sun in the centre; but, as in reality, they move in elliptic orbits, with the sun in the focus, the distance, in the same orbit, was subject to continual variation; and hence it became necessary to ascertain the corresponding variation of the force. Finding his mathematical powers inadequate to the task of successfully grappling with this more difficult problem of dynamics, he applied to Wren and Hooke, in hopes of receiving from either of them a solution of it. Wren, according to Newton's statement, had deduced the law of the inverse square of the distance (for circular orbits) several years previous to Halley's present communication with him. When Halley proposed to him the problem of the law of the force in an elliptic orbit, he replied, that he had bestowed much thought on it, but was compelled to give it up from inability to make any impression on it. Hooke asserted that he had solved it, and had found that the force varied according to the inverse square of the distance. When pressed to produce his solution, he refused to do so, declaring that he would conceal it, until others trying and failing, might know how to value it when he should make it public. It is quite clear, however, that he was unable to support his assertion by any mathematical proof, for if such had been the case he would have given it forth to the world as the surest means of vindicating his claims, when he attempted, a year or two afterwards, to appropriate to himself the credit of Newton's discoveries.

Unable to obtain a solution of this interesting problem from any of his acquaintance in London, Halley proceeded to Cambridge, in the month of August, 1684, for the express purpose of conferring with Newton on the subject. To his inexpressible delight, he learned the good news that his friend had already brought the demonstration to perfection. So little was Newton's mind occupied at this time with such researches, that he was unable to lay his hand on the papers relating to them when Halley visited him, but he promised to send them to him soon after his return to London. It appears that Newton subsequently worked out the propositions afresh, and transmitted them to Halley, in the month of November of the same year. Halley immediately set out upon a second visit to

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* For a concise but very luminous exposition of the mode by which Newton established the principle of gravitation, see the "History of Astronomy," Library of Useful Knowledge, p. 83, et seq.

Cambridge, to procure more information, and to encourage Newton to pursue his researches. In December, of the same year, we learn the progress of Newton's labours, from Halley's announcement to the Society, on the 10th of that month, "that he had lately seen Mr. Newton, at Cambridge, and that he had shown him a curious treatise 'de Motu,' which, upon his desire, he said was promised to be sent to the Society, to be entered upon their register."* In fulfilment of his promise, Newton transmitted to the Society, about the middle of February, 1685, a paper containing his early researches on centripetal forces. This communication consisted of eleven propositions, the greater number of which were similar to those which subsequently formed the second and third sections of the Principia. Newton, in acknowledging the registration of his paper by the Society, thus writes to Mr. Aston, the secretary, on the 23rd of February. "I thank you for entering in your register my notions about motion. I designed them for you before now; but the examining several things has taken greater part of my time than I expected, and a great deal of it to no purpose. And now I am to go into Lincolnshire for a month or six weeks. Afterwards I intend to finish it as soon as I can conveniently."† It is quite clear from the above letter that, although Newton was already in possession of the groundwork of all his discoveries in Physical Astronomy, he had not at this time developed his thoughts beyond the substance of the brief essay transmitted to the Society. Indeed, he can hardly be said to have entered seriously upon the composition of the Principia until his return to Cambridge, in April, 1685. Mr. Rigaud has justly remarked, in reference to this fact, that the Principia was not a protracted compilation from memoranda which might have been written down under the impression of different trains of thought. It had the incalculable advantage of being composed by one continued effort, during which the mutual bearing of all the several parts was vividly presented to the author's mind ‡.

On the 21st of April, 1686, Halley read before the Royal Society a paper on gravity; in which, after alluding to the labours of Galileo, Toricelli, and Huygens, he mentions the truths "now lately discovered by our worthy countryman, Mr. Isaac Newton, who has an incomparable 'Treatise of Motion' almost ready for the press." § The prospect held out by Halley was very soon realised; for, on the 25th of the same month, Dr. Vincent presented to the Society a manuscript treatise of Mr. Isaac Newton's, entitled, "Philosophiæ Naturalis Principia Mathematica." This was the first book of the Principia. The Society directed that a letter of thanks should be addressed to the author: they also referred the question of printing it to the consideration of the Council, and the drawing up of a report on it to Dr. Halley. On the 19th of May, the Society ordered that the book should be printed forthwith: whence an impression has been generally formed that the Principia was printed at the expense of that body. This conclusion, however, is not borne out by the words on the title page of the work, which are, "Jussu Societatis Regiæ," not "Jussu et Sumptibus," as was usual in those cases where the expense of printing was defrayed out of the funds of the Society. But a decisive refutation of the current opinion is furnished by a resolution passed at the meeting of the Council on the 2nd of June, to the effect that Mr. Newton's book be printed, and "that E. Halley shall undertake the business of looking after it and printing it at his own charge, which he engaged to do." The fact is, that when the Council, which took cognizance of all the pecuniary affairs of this Society, came to consider the resolution adopted at the general meeting of May the 19th, they found that the state of their finances could not admit of their carrying it into effect. A work, "De Historia Piscium," by Fr. Willughby, had been published in 1686, "Jussu et Sumptibus," and the outlay incurred by this publication appears to have completely exhausted the funds of the Society. To such extremities, indeed, were they reduced by this act of imprudent liberality, that they were compelled to pay their officers in copies of this work on fishes, in consequence of their inability to procure purchasers for it.

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* Journal Book of the Royal Society; see also Rigaud's Historical Essay on the first publication of the Principia. Oxford, 1838.

† Letter Book of the Royal Society, vol. x. p. 28. See also Rigaud's Essay. Appendix, page 24. The original letter has not been discovered.

‡ Rigaud's Essay, page 25.

§ Phil. Trans., vol. xvi. p. 6.

Meanwhile a violent reclamation was raised by Hooke relative to the discovery of the law of gravitation. This individual, who would be well entitled by his genius to occupy a high place in the history of physical science, if he had displayed more uprightness and moderation in his relations with contemporary philosophers, had no sooner heard of the manuscript which Dr. Vincent had presented to the society in Newton's name, than he asserted that it was he who first communicated to the author the law of the inverse square of the distance, as well as various other discoveries announced in the manuscript. We have mentioned that, as early as 1666, Hooke had arrived at very accurate notions on the subject of centripetal forces. In 1674 he published a work, entitled "An Attempt to prove the Motion of the Earth from Observations," in which he describes the general nature of gravitation with remarkable clearness and accuracy. Although, however, he remarked that the attractive forces acting between bodies "are more powerful as the distances from the centres are less," it is quite clear that the idea of computing by a mathematical investigation the intensity of the force in any case at different distances from the centre, and thereby ascertaining the law of its variation, did not at all occur to him; for, after referring to the varying intensity of the force, he then goes on to say: "now what these several degrees are I have not yet *experimentally* verified." It would appear, however, that, guided by the analogy of other emanations from centres, he had subsequently adopted the inverse square of the distance as the law of the force which retains the planets in their orbits; and then, extending the same law to the earth, he concluded, by an inversion of the question, that the path of a projectile was an ellipse, with the centre of the earth in the focus. We have mentioned already that Hooke was unable to produce a demonstration of the law of the inverse square of the distance, although he boasted repeatedly that he had arrived by legitimate reasoning at that result. The fact is that, although a man of extraordinary acuteness in physical matters, he had no talents for mathematical science; and this defect constituted an effectual bar towards his establishing, upon a satisfactory basis, any of the great truths relating to the theory of gravitation.

But although Hooke's powers were inadequate to the complete investigation of the problem of centripetal forces, there was much merit in the clearness with which he pointed out the mode in which a body is retained in a curvilinear orbit by a force continually directed towards a fixed centre. His views on this subject were in strict accordance with mechanical principles, and it must be admitted that they formed an important step towards a rigorous solution of the problem.

When Halley learned the extreme pretensions of Hooke, he deemed it his duty to acquaint Newton with the charge preferred against him. This called forth a long and interesting letter from Newton, dated June 26th, 1686, in which he mentions a variety of particulars connected with the progress of his researches. He asserts that he had discovered the law of the inverse square of the distance (for circular orbits) even previous to the publication of Huygen's treatise "De Horologio Oscillatorio."* He admits that he was led to consider the law of the force in an elliptic orbit by Hooke's letter to him in 1679, but he positively denies being indebted to him in any other way for the results at which he arrived. This letter contains some interesting information relative to the progress of his labours in composing his great work. "I designed," says he, "the whole to consist of three books; the second was finished last summer, being short, and only wants transcribing, and drawing the cuts fairly. Some new propositions I have since thought of, which I can as well let alone. The third wants the theory of comets." Thus it appears that, about fifteen months after he returned from Lincolnshire to Cambridge, he had almost completed the three books of the Principia. This fully corroborates the statement of Pemberton, that Newton was engaged only about eighteen months in the composition of his immortal work. When we contemplate, in connexion with this fact, the prodigious mass of original discoveries announced in the Principia, the mind is lost in amazement at the power of thought which could have reared into existence so stupendous a monument in such a brief space of time.

Newton seems to have been so much disgusted with Hooke's violent conduct, that, in the letter above referred to, he intimated his resolution to suppress the third book altogether, containing the application of his dynamical discoveries to the system of the world. On the occasion of announcing his splendid discoveries in Optics at an earlier period, he had experienced much annoyance from the ignorance and jealousy of rival claimants, and he now feared that his peace of mind might be disturbed again by a similar cause. "Philosophy," says he, "is such an impertinently litigious lady, that a man had as good be engaged in lawsuits, as have to do with her. I found it so formerly, and now I am no sooner come near her again but she gives me warning. The two first books without the third will not bear so well the title of 'Philosophiæ Naturalis Principia Mathematica;' and therefore I had altered it to this, 'De Motu Corporum libri duo;' but upon second thoughts I retain the former title, 'twill help the sale of the book, which I ought not to diminish now 'tis yours." Halley wrote a soothing reply to Newton, declaring his belief in the groundlessness of Hooke's charges, and imploring him not to persevere in his resolution of suppressing the third book of his work. Newton seems to have listened favourably to the advice of his friend, and he gave a proof of his conciliatory disposition by adding a scholium to the fourth proposition of the first book, in which he mentions that Wren, Hooke, and Halley, had all found, by means of the relation between the periodic times and the distances, that the force which retains the celestial bodies in their orbits (supposed circular) varies according to the inverse square of the distance.

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* In a subsequent letter to Halley, dated July 14th, 1686, he mentions having arrived at the law of the inverse square of the distance, by means of Kepler's theorem, about twenty years previously. This would carry back his original speculations to about the time assigned to them by Pemberton. The original of this letter is in the guard-book of the Royal Society.

It is impossible too much to admire the conduct of Halley in regard to the part he took in the publication of the Principia. Indeed we may reasonably doubt whether that immortal work would ever have been written at all, if it had not been for his enlightened zeal in the cause of science; for Newton himself appears to have been imbued much more strongly with the love of pondering in secret over his discoveries, than he was urged by the equally natural feeling of communicating them to others. This disposition of mind was fostered by a lively recollection of the annoyance he had suffered from the publication of his researches in Optics, and the consequent dread he entertained of having his tranquillity again disturbed by a controversy with envious rivals. Halley, therefore, besides discovering the only individual living who could unfold the physical theory of the celestial motions, is entitled to the credit of having persuaded him to communicate his discoveries to the world. Nor was this all; for, as has been already hinted, he defrayed the expense of publishing[*] the Principia, at a time too when his finances could ill afford such an outlay[†]; and also undertook the revision of it in its progress through the press. Posterity has retained a grateful recollection of those princes who at different periods of history have distinguished their reign by a munificent patronage of learning and science; but, among all those who have thus contributed indirectly to the progress of knowledge, there is none who exhibits such a bright example of disinterestedness and self-sacrificing zeal as the illustrious superintendent of the first edition of the Principia. It is pleasing to reflect that Halley received such a noble reward for his exertions in the splendid discovery with which his name is immortally associated, and to which he was mainly conducted by Newton's researches on comets.

The Principia was published in 1687, and was dedicated to the Royal Society. At the beginning of it was inserted a Latin poem in hexameter verse by Halley, in honour of Newton's discoveries. The concluding line runs thus:—

> "Nec fas est propius mortali attingere divos;"[‡]

"an eulogium," says the severe Delambre, "which no one has charged with exaggeration."[§]

The whole work is divided into three books. The first book treats of motion in free space; the second is occupied chiefly with questions relating to resisted motion; the third is upon the system of the world.

The first book is divided into fourteen sections, and contains ninety-eight propositions, besides a number of corollaries, lemmas, and scholia. In the first section, Newton explains the geometry which he employs in his subsequent investigations. It is termed by him the method of prime and ultimate ratios, and is essentially the same as the differential calculus. In the second section he enters upon the subject of centripetal forces, demonstrating Kepler's theorem of areas, and investigating the law of the force in various curves. In the third section, he considers the motion of a body compelled to revolve in any of the conic sections by a force directed continually to the focus. The fourth and fifth sections are purely geometrical, relating to methods of drawing conic sections through given points and touching given straight lines. The sixth section treats of the motion of a body in a given orbit. The seventh treats of the motion of a body ascending or descending in a straight line relative to the centre of force. The eighth contains the investigation of the orbit described by a body when the law of the centripetal force is given. The ninth relates to the motion of bodies in moveable orbits. This section contains the famous investigation of the motion of the apsides. The tenth treats of bodies moving on given surfaces, and of the motion of pendulums.

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[*] It must be understood that Halley was subsequently reimbursed for the expenses connected with the publication of the Principia by the sale of the copies of the work.

[†] He was brought up in affluent circumstances, but in 1684 his father died, after completely wasting his fortune.

[‡] Nor is it lawful for mortals to approach nearer the Deity.

[§] Histoire de l'Astronomie de Dixhuitième Siècle, p. 2.

Hitherto Newton has been considering only the motion of material points. In the eleventh section he investigates the motion of bodies exposed to their mutual attraction. The twelfth treats of the attraction of spheres. The thirteenth of the attraction of bodies not spherical. The fourteenth relates to the motion of small particles passing from one medium into another.

The second book is divided into nine sections, and contains fifty-three propositions. It treats of bodies moving in resisting media upon different hypotheses of the resistance; and, whether moving in straight lines, or curves, or vibrating like pendulums. It also takes cognizance of the more recondite parts of several other branches of the Physico-mathematical sciences. The second lemma to the eighth proposition contains an exposition of the method of Fluxions, which is rendered necessary in most of the investigations of this and the following book.

The third book contains forty-two propositions. From the first to the eighteenth inclusive, Newton demonstrates various general theorems relative to the attraction of the sun, moon, and planets. In the nineteenth and twentieth he investigates the ratio of the earth's axes, and compares the weights of bodies at the surface in different latitudes. In the four following propositions, he shows that the precession of the equinoxes, the irregularities of the moon and the other satellites, and the phenomena of the tides, are all explicable by the principle of gravitation. From the twenty-fifth to the thirty-fifth inclusive, he computes the various inequalities of the moon's motion. The thirty-sixth and thirty-seventh treat of the tides. The thirty-eighth, of the figure of the moon. The thirty-ninth, of the precession of the equinoxes. The remaining three propositions are devoted to the theory of comets. At the conclusion is a scholium to the whole work, containing general reflections on the constitution of the material universe, and on the eternal and omnipotent Being who presides over it *.

The publication of the Principia marks by far the most important epoch in the history of physical science. Previous to its appearance the researches of philosophers may be said to have resembled the voyages of the early navigators, who continued creeping timidly along the coasts, without daring to launch their barks into the boundless ocean. Newton, like another Columbus, disdained to confine himself within the common-

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* Besides the original edition of the Principia, two others were published during the life of the author. The second edition was published at Cambridge in 1713, under the superintendence of Cotes. The third edition was published at London in 1726, by Pemberton.

place conventionalities of ordinary minds; and, guided by the eagle eye of genius, explored the secret springs which animate a whole system of worlds. We cannot convey to the general reader a more adequate idea of the merits of the incomparable work just mentioned, than by citing the judgment pronounced upon it by the most illustrious of Newton's followers. Laplace, after enumerating the various astronomical discoveries first announced in the Principia, concludes in the following terms:—"The imperfection of the Infinitesimal Calculus, when first discovered, did not allow Newton to resolve completely the difficult problems which the system of the world offers, and he was often compelled to give mere hints, which are always uncertain until they are confirmed by a rigorous analysis. Notwithstanding these unavoidable defects, the number and generality of his discoveries relative to this system, and many of the most interesting points of the Physico-mathematical sciences, the multitude of original and profound views, which have been the germ of the most brilliant theories of the geometers of the last century, all of which were presented with much elegance, will assure to the Principia a pre-eminence above all the other productions of the human intellect." *

CHAPTER II.

Newton's Intellectual Character considered in connexion with his Scientific Researches.—His Inductive Ascent to the Principle of Gravitation.—Motion of a Body in an Orbit of Variable Curvature.—Attraction of a Spherical Mass of Particles.—Developement of the Theory of Gravitation.—General Effects of Perturbation.—Inequalities of the Moon computed.—Aid afforded by the Infinitesimal Calculus.—Figure of the Earth.—Attraction of Spheroids.—Precession of the Equinoxes.—General accuracy of Newton's Results.—Anecdotes illustrative of his Natural Disposition.—His Death and Interment.

NEWTON was singularly endowed with all those qualities which enable the mind to unfold the laws of the material world. He could detect with a glance the distinctive features of natural phenomena, and with marvellous sagacity divine the principles on which they depended. With these valuable qualities he combined a proneness to generalization, which constantly led him to connect together the facts he was contemplating, and advance from them to more comprehensive views of the operations of nature. He possessed also powers of mathematical invention adequate on all occasions to surmount the difficulties he might encounter, either in ascending by induction to general laws, or in subsequently redescending from them to the explanation of their various consequences. When we consider, moreover, that he was imbued with an extreme love of truth, which induced him to reject all speculations, however ingenious and beautiful, that were not reconcileable with facts—that his whole soul was wrapped up in the study of nature and her works, and that he possessed in an extraordinary degree the power of concentrating the whole energies of his intellect upon the object of his researches, we may form some conception of the advantages under which he approached the examination of physical questions. It is, in fact, in consequence of his possession of

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* Exposition du Système du Monde, liv. v. chap. v.

all these qualities in so high a degree, that he stands without a rival among ancient or modern philosophers. His discovery of Universal Gravitation, beyond all comparison the greatest achievement that the human mind can boast of, affords abundant illustration of the truth of this remark. Throughout the magnificent train of investigations which that discovery suggested to his mind, we see him constantly uniting the sagacious and comprehensive views of the genuine interrogator of nature with the fertility of invention, the skilful research, the profundity and elegance, of the consummate mathematician. We have, in fact, presented to us the unexampled combination in one individual of all those attributes of genius which ennoble the human intellect, and which have thrown the halo of immortality around the names of Kepler and Leibnitz—of Galileo and Descartes—of Bradley and Laplace.

The transcendent powers of Newton's intellect are equally discernible in his inductive ascent to the principle of gravitation, and in his subsequent developement of its numberless consequences. Notwithstanding the sagacity he exhibited in connecting the fall of a stone at the surface of the earth with the motion of the moon in her orbit, and both of these phenomena with the motions of the planets round the sun, he would inevitably have failed in establishing this sublime conception as a physical truth, if he had not also possessed sufficient mathematical genius to solve the problem of central forces for an orbit of variable curvature. To those who are acquainted with the state of mechanical science in Newton's time it would be superfluous to mention that the highest powers of invention were indispensable for this purpose. When we reflect on the fact that Kepler spent a considerable part of his life in vain efforts to establish a connexion between the motions of the planets and the continual agency of some physical principle, that the question entirely escaped the sagacity of Galileo, and that Huygens, although in complete possession of the laws of motion, was unable to advance in its solution beyond the case of a circular orbit, we may well imagine the obscurity in which it was enveloped, and the mathematical difficulties which the investigation must have offered. Even when Newton had succeeded in this research, he merely established the mutual gravitation of the planets, according to the law of the inverse square of the distance, but he was not also enabled to extend the same principle to the ultimate particles of which the masses of the planets are composed. In order to effect this object, and thereby to establish the law of gravitation in its widest generality, he was compelled to determine the effect of the attraction of a spherical agglomeration of particles. This problem is of a totally opposite nature to the one already referred to; for here we have an infinite number of particles in juxtaposition, all attracting the body with unequal intensities and in different directions. Its intricacy is manifest at first sight; nor was this circumstance compensated by any preliminary hints calculated to facilitate its solution, for the mere conception of such a problem had not yet occurred to any mathematician. Newton, however, again triumphed over opposing difficulties, and thus succeeded in riveting, with the bonds of demonstrative reasoning, all the links of his magnificent generalization.

In redescending from the principle of universal gravitation, and pursuing it into its remoter consequences, he displays even more astonishing force of genius than he does in the course of his inductive ascent. It might be supposed that when once the highest step of generalization was attained, the functions of the natural philosopher would cease, and the task of tracing the derivative truths of a principle so essentially conversant with the abstractions of space and time as that of gravitation, would devolve entirely on the mathematician. This is indeed true to a great extent in our own day, when, from a few differential equations, involving the general law of gravitation, all the phenomena of the planetary motions may be derived by a process of pure symbolical reasoning. But in Newton's time such a method of investigation was utterly impracticable, for the groundwork of it could not be said to exist. The science of mechanics was not sufficiently advanced to admit of the immediate translation of the conditions of a problem into an analytical form *; and even if such a step had been already possible, no further progress could have been made in the same direction without a more powerful calculus than Newton was in possession of. The theory of differential equations was yet a mere germ, and the arithmetic of angular functions †, which tends so much to condense and simplify the processes of analysis, and thereby to increase its efficiency, was utterly unknown. It was therefore solely upon the innate resources of his genius as a philosopher and a mathematician that Newton had to rely in pursuing the consequences of the theory of gravitation. By a profound study of the mode in which forces operate, aided by his admirable sagacity in referring phenomena to their true physical causes, he was enabled to trace with astonishing accuracy the various consequences resulting from the mutual gravitation of the bodies of the solar system. It would be difficult for any mathematician of the present day, armed with all the resources of mechanical science, to expound more fully and more clearly the *general* effects of perturbation than Newton has done in the sixty-sixth proposition, and its corollaries, of the first book of the Principia. When he proceeded to investigate the actual values of these effects, with the view of submitting his theory to a rigorous comparison with observation, he found his path beset with mathematical difficulties infinitely more formidable than any he had hitherto encountered, in consequence of the excessive complication occasioned by the perturbing forces. Nor was the geometry he employed in these researches calculated to facilitate his labours. He loved, on all possible occasions, to adhere to the synthetic method of the ancient geometers; but this course entailed upon him a vast expenditure of thought,—for not only was it with the utmost difficulty that the ancient geometry could be wielded in such delicate inquiries, but as it could not furnish any general method of investigation, he was compelled to devise a fresh mode of attack for each successive problem, and thus his inventive powers were constantly called into severe exercise. Notwithstanding the rude and unmanageable character of the instruments he had to deal with, he applied them with amazing dexterity to the computation of some of the most complex effects of perturbation, such as the irregularities of the moon's motion, the figure of the earth, and the precession of the equinoxes. The difficulty of treating such subjects by the ancient geometry may be imagined from the fact, that no one of his successors has been enabled by its aid to advance a single step beyond the point at which he arrived [*]; and, in order to proceed with the further developement of the theory of gravitation, it has been found necessary to have recourse to the more easy and comprehensive methods of analysis.

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* We allude here more especially to the investigations connected with the figures of the heavenly bodies, their motions around their centres of gravity, and the oscillations of the fluids on their surfaces.

† Although the use of trigonometrical formulæ in analytical processes was not introduced among mathematicians until half a century after the publication of the Principia, it would perhaps be unsafe to pronounce a positive opinion on this point with respect to Newton himself, for his investigations show him to have been at least in complete possession of the algebraic character of angular functions. Thus, in tracing the horary motion of the nodes (Prin., book iii. prop. xxx.) by means of the triple product—sin T P I. sin P T N. sin S T N,—or the moon's distance from quadratures, her distance from the nodes, and the distance of the nodes from the sun, he describes the effect upon the formula of the varying magnitudes of the several angles with as much apparent ease as the most expert analyst of the present day could do. The illustrious Euler may, however, be considered as the real originator of this valuable extension of analysis, since it was he who first introduced it generally to the knowledge of mathematicians. This he did in his memoir on the inequalities of Jupiter and Saturn, which obtained for him the prize of the Academy of Sciences of Paris for the year 1748. After deriving the analytical expressions for the perturbing forces, he then proceeds in the following terms:—"La plupart du calcul roulera donc sur les angles, *que j'introduirai eux-mêmes dans le calcul*, en marquant leur sinus, cosinus, tangentes, cotangentes par les caractères sin cos. tang. et cot. mises devant les lettres qui expriment les angles. Cela abregera très considérablement le calcul surtout dans les integrations et differentiations."—Récherches des Inegalités de Jupiter et de Saturne, p. 15.

Nor are the results to which he was conducted such rude approximations as one would be apt to suppose from the unsuitableness of synthesis for such intricate subjects. His researches on the lunar theory are especially remarkable for their ingenuity and elegance, and for the general accordance of the results with observation. He computed the inequality termed the variation, and fixed its mean value at 36′ 10″ [†]; Mayer, in his celebrated tables of the moon, made it 35′ 47″. Laplace considers the method pursued by Newton in investigating this inequality as forming one of the most remarkable portions of the Principia, and he has shewn that, by viewing it through the medium of analysis, it conducts to the usual differential equations of the moon's motion [‡]. He computed the mean motion of the nodes with still greater accuracy. He obtained 19° 18′ 1″.23 for the regression in a sidereal year [§]; the astronomical tables assigned 19° 21′ 22″.50 as the real value. The difference was therefore less than $\frac{1}{300}$th part of the whole motion. He obtained equally satisfactory results for the horary motion of the nodes, and for the variation of the inclination corresponding to different positions of the moon and her nodes. He also computed several other inequalities of a more hidden nature, but contented himself with merely announcing their greatest values [||]. Among these were included the annual equation, which he fixed at 11′ 51″, assuming the eccentricity of the earth's orbit to be .016916. Mayer's tables give 11′ 14″ for the coefficient of this equation. He also assigned the values of the inequalities in the mean motions of the apogee and nodes, depending on the motion of the earth in her orbit. The inequality of the apogee was fixed by him at 19′ 43″, and that of the nodes at 9′ 24″. According to the modern tables these inequalities are equal to 22′ 17″ and 9′ 0″; they had entirely escaped the notice of astronomers until Newton derived them from his theory. It was while engaged in these profound researches, that the infinitesimal calculus, the brilliant discovery of his earlier years, came so opportunely to his aid, by enabling him to sum up the effects of minute forces, varying every instant in intensity and direction. It is true, that the infant powers of this noble calculus were yet comparatively feeble; but still, without its aid, the problems relating to the perturbing action of the heavenly bodies would have been utterly unassailable. The success with which Newton investigated the lunar theory is astonishing, when the intricacy of the subject is considered. We may form some idea of the complicated character of the moon's motion from the fact that it is only in our own day that all her irregularities have finally yielded to the scrutinies of a most refined analysis [*].

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[*] Maclaurin's beautiful speculations on the attraction of elliptic spheroids may be considered as forming the only exception to this remark.

[†] Principia, b. iii. prop. 29.

[‡] Mécanique Céleste, liv. xvi. chap. ii.

[§] Principia, book iii. prop. 32.

[||] Ibid., book iii. prop. 35. Scholium.

In one remarkable instance Newton failed to derive from his theory a result agreeing with observation. He had shown, by a method of uncommon ingenuity and subtlety, that a small disturbing force of the same nature with that exerted by the sun upon the moon would not sensibly alter the elliptic form of the disturbed body's orbit, but would, on the whole, cause the line of apsids to advance continually in the direction in which the body was moving [†]. When he applied this result to the theory of the moon, by calculating the mean motion of the lunar apogee, he obtained 1° 31′ 28″ for the monthly progression. The value, however, assigned by observation, amounted to 3° 4′, a quantity nearly double the result obtained by Newton. We shall have occasion in the next chapter to mention the origin of this discordance.

The same commanding powers of investigation marked his progress as he penetrated into still more recondite parts of his theory. His solution of the problem of the figure of the earth is a remarkable instance of his success in accomplishing a great result by very small means. He perceived that the mutual gravitation of the particles, combined with the effect of their diurnal rotation, would occasion a flattening of the earth at the poles; but the question was to ascertain its real form, and the ratio between the equatorial and polar axes. Proceeding upon the supposition that the earth was originally in a fluid state, and that its density was homogeneous, he concluded that the forces acting upon the particles would cause it to assume the form of an oblate spheroid. This solution of a difficult question of hydrostatics was nothing more than a sagacious conjecture; yet, strange to say, it was afterwards confirmed by a rigorous investigation, founded on the conditions of equilibrium of a homogeneous mass. In order to determine the ratio of the axes, he conceived two columns of the fluid to extend from the centre of the earth to the surface;—one to the equator, and the other to one of the poles. Since these two columns were in equilibrium, they would press each other with equal intensities, and hence the ratio of their lengths would be found by equating their weights. Now the weight of the equatorial column depends partly on the gravitation of the particles, and partly on their centrifugal force; but as the polar column is not affected by the diurnal rotation, its weight will depend simply on the gravitation of the particles. The centrifugal force of a particle is very easily ascertained by means of its angular motion and its distance from the centre, but its gravitation, resulting from the combined attraction of the surrounding particles, can be determined only by a profound mathematical investigation. Newton, by a method of great elegance, had previously found the gravitation of a particle within a spherical mass; but the result he obtained on that occasion was useless in the present case, since the question now referred to a spheroid and not a sphere. He was thus led to consider a series of problems relating to the attraction of spheroids, all of which he solved with great elegance by means of the ancient geometry*. Applying these results to the investigation in question, he then found, by an indirect but most ingenious process, that the polar axis of the earth was to the equatorial axis as 229 to 230 †. The ellipticity of the earth is considerably greater, whence it may be inferred that the density is not homogeneous. It is remarkable, however, that Newton's solution of the problem on the supposition of homogeneity is quite correct; for when geometers subsequently applied to it all the resources of analysis and mechanics, they were conducted to exactly the same result.

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[*] We allude to the result of M. Hansen's recent researches relative to the irregularities in the moon's epoch. We shall endeavour to give some account of this important discovery in its proper place.

[†] Principia, book i. sec. ix. prop. xlv. cor. 2.

He also shewed that the spheroidal figure of the earth, combined with its diurnal motion, would cause the weights of bodies at the surface to vary in different latitudes; and this result of pure theory explained the singular fact first noticed by Richer, the French astronomer, who found that a clock regulated to mean time of Paris lost 2′.28″ daily at Cayenne in Africa ‡.

His explanation of the precession of the equinoxes is one of the most beautiful illustrations of his genius. Conceiving a satellite to revolve round the earth in the plane of the equator, he had already found that the effect of a disturbing body exterior to it would be to cause the nodes of the satellite to regress on the orbit of the disturbing body. Imagining, then, a ring of such satellites to encompass the earth, the instantaneous effect produced on the ring by the disturbing body would manifestly be similar to that produced on any one satellite in course of a complete revolution. The nodes of the ring would therefore constantly regress on the plane of the disturbing body's orbit, and if the ring actually adhered to the earth the nodes would still regress, but with a much smaller velocity, in consequence of the enormous mass of the earth participating in the regression while the moving force retained the same value. This is precisely the real case of nature, the equatorial matter forming the circumambient ring, and the sun or moon representing the disturbing body. Thus, after the lapse of nearly two thousand years since its discovery by Hipparchus, the precession of the equinoxes was finally traced to its physical origin. This grand phenomenon had in all ages appeared utterly inexplicable to astronomers; even Kepler, notwithstanding his unrivalled aptitude in the formation of hypotheses, was unable to account for it by any physical principle. Newton's explanation was so natural that it could not fail to carry with it instant conviction. Mr. Airy has well remarked that, "if at this time we might presume to select the part of the Principia which probably astonished and delighted and satisfied its readers more than any other, we should fix, without hesitation, on the explanation of the precession of the equinoxes." §

The sagacity which Newton displayed in the discovery of the true cause of the conical motion of the earth, can only be equalled by his boldness in making it the subject of a mathematical investigation; for the theory of the motion of a rigid body around its centre of gravity was yet totally undeveloped. By means of several ingenious suppositions he succeeded in bringing the problem within the reach of his geometry, and computed the quantity of precession due to each of the two disturbing bodies *. The imperfect state of mechanical science, combined with the intricacy of the subject, happened indeed in this instance to betray him into a mistake; but his solution of this great problem was on the whole sound, and Laplace, who has critically examined it, has not failed to point out its excellent merits †.

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* Principia, book i. prop. 91, and book iii. prop. 19.

† Ibid., book iii. prop. 19.

‡ Ibid, book iii. prop. 20.

§ Encyc. Metrop., art. Figure of the Earth.

In pursuing his way through these abstruse researches, Newton seems to have compensated by the innate resources of his genius for the defective state of his methods. The accuracy of his results, in many cases in which a rigorous course of investigation was impracticable, is one of the most inexplicable facts in the annals of science. His clear insight into the operation of physical principles and his fine discriminating judgment, qualities which contributed so effectually to enhance the value of his delicate researches in Optics, appear to have been equally favourable to him while engaged in considering the less tangible and less familiar relations of the system of the world. It is this wonderful facility of seizing truth as it were with a single bound, without pursuing the long avenue of sequences by which ordinary inquirers are conducted to it, which has led Delambre to remark that the words of Fontenelle, in relation to Cassini, might be much more appropriately applied to the English philosopher — "Un Astronome si subtil est presque un devin; on dirait qu'il pretend à la gloire d'un astrologue." ‡

It is much to be regretted that Newton should have persevered so generally in expounding his discoveries by the synthetic methods of the ancient geometers, for it can hardly be doubted that he was in most cases conducted to them by analysis. He probably feared that the infinitesimal calculus would not be considered as imparting to his researches that character of severe reasoning by which the synthetic mode of demonstration is peculiarly distinguished. His apprehension will appear by no means unreasonable, when we consider that the analytical instrument of investigation was then in its infancy, and that very few persons were acquainted with its true principles. By his practice, however, of presenting his researches in a synthetic form, he deprived himself of the honour attached to many important discoveries in analysis, which his results indicate him to have been in possession of. The famous problem of the solid of least resistance affords a striking illustration of this remark. In the scholium to the 34th proposition of the second book of the Principia, he gives the construction of this solid, but does not accompany it with any demonstration. This is the first of a peculiar class of problems that was ever solved, and it is clear, from Newton's construction, that he must have been acquainted with those principles of the infinitesimal analysis which form the basis of the Calculus of Variations §.

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* Book iii. prop. 39. † Méc. Cél., liv. xiv. chap. 1.

‡ Histoire de l'Astronomie Moderne, tome ii. p. 739, and Histoire au Dixhuitième Siècle, p. 630.

§ It is quite conceivable, when we consider Newton's powers of generalization, that, if he had devoted much attention to problems of this nature, he might have been conducted to the Calculus of Variations. We have no reason however to conclude, from his solution of the problem cited in the text, that he was in possession of the general method of Lagrange

This illustrious philosopher, who contributed more than any other mortal ever did towards enlarging the domain of human knowledge, appears to have been quite unconscious of any difference between himself and ordinary inquirers of nature. Alluding to his discoveries in a letter to Dr. Bentley, he says, " If I have done the public any service this way, it is due to nothing but industry and patient thought." In fact, it was only by the most strenuous contention of mind, and the sternest subjection of the will, that even Newton was enabled to penetrate into the more recondite parts of the system of the world. One of his biographers has remarked * that, during the two years he was engaged in preparing the Principia, he lived only to calculate and think. Oftentimes lost in the contemplation of those grand objects to which it relates, he acted unconsciously, his thoughts appearing to take no cognizance of the ordinary concerns of life. Frequently, when rising in the morning, he would be arrested by some new conception, and would remain for hours seated on his bedside in a state of complete abstraction. He would even have neglected to take sufficient nourishment if he had not been reminded by others of the time of his meals. Speaking of the mode by which he arrived at his discoveries, he said, " I keep the subject constantly before me, and wait till the first dawnings open slowly by little and little into a full and clear light." On another occasion, when some of his friends were complimenting him on the great results he had achieved, he replied : " I know not what the world will think of my labours, but to myself it seems to me that I have been but as a child playing on the sea-shore ; now finding some pebble rather more polished, and now some shell rather more agreeably variegated than another, while the immense ocean of truth extended itself unexplored before me." What a lesson of humility is here conveyed to those explorers of nature who cannot congratulate themselves on the discovery even of such shells and pebbles as those which adorn the cabinet of the Principia.

Newton died on the 20th March, 1727, at the advanced age of eighty-five years. Unlike some of his illustrious predecessors, he continued throughout his long career to receive the honours due to his exalted genius, and his death was deplored as a national calamity. His funeral obsequies were performed with the ceremonies usually confined to persons of royal birth. His body lay in state in the Jerusalem Chamber, and was subsequently interred in Westminster Abbey, his pall having been borne by six peers. A monument was erected over his remains, the inscription upon which concludes with the following suitable words : " Sibi gratulentur mortales, tale tantumque extitisse humani generis decus."†

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for this purpose, any more than we should be warranted in inferring from Fermat's theory of Maxima and Minima, or Barrow's Method of Tangents, that either of these mathematicians had discovered the Differential Calculus. The probability is, that in this, as in many other instances, Newton solved the problem merely *en passant*, attending less to the means than the end to be obtained by them.

* Biot. Biographie Universelle.—See also Life of Newton, L. U. K.

† Let mortals congratulate themselves that so great an ornament of the human race has existed.

CHAPTER III.

Circumstances which impeded the early progress of the Newtonian Theory.—Its reception in England.—Reception on the Continent.—Huygens, Leibnitz.—Researches in Analysis and Mechanics.—Their influence on Physical Astronomy.—Problem of Three Bodies.—Motion of the Lunar Apogee.—Clairaut.—Lunar Tables.—Mayer.

NOTWITHSTANDING the multitude of sublime discoveries by which the theory of gravitation was first announced to the world, no attempt was made to develope the views of its immortal founder, during the first half century that elapsed after the publication of the Principia. The seductive speculations of Descartes had already taken a firm hold of men's minds, and had been introduced as a branch of scientific study into the principal universities of Europe. Independently of this circumstance, the profound and intricate reasoning, which Newton was compelled to adopt in the Principia, formed a serious impediment to the early dissemination of his doctrines. As the questions considered in that immortal work were generally of the kind which required the aid of the higher geometry for their complete investigation, only a very small number of mathematicians were qualified to appreciate the evidence upon which the conclusions of the author were founded. The methods also which he employed in expounding his discoveries were almost wholly the creation of his own genius, and it was necessary to study them with deep attention in order to become familiar with their real character. Hence it is easy to understand why the severe doctrines of the Principia continued long to be neglected, while the more accommodating principles of the Cartesian theory met with universal favour.

The country which gave birth to Newton may in some degree be considered an exception to these remarks. The Principia, upon its first appearance, was read with admiration by the most eminent mathematicians of the day; and the sublime truths announced in it were enthusiastically embraced by the more intelligent classes of the community. The university of St. Andrews, in Scotland, has the honour of being the first Academic Institution which admitted the Newtonian theory as a subject of study. In 1690, James Gregory, the celebrated mathematician who was then professor of philosophy in that university, published a thesis containing twenty-five positions, twenty-two of which are said to have formed a compendium of the Principia. The same principles were introduced into the university of Cambridge under the auspices of Dr. Samuel Clarke, the personal friend of Newton. Whiston first expounded them from the chair, in the year 1699. They were also taught at Oxford by Keil, as early as the year 1704.

On the continent, all the great mathematicians were unanimous in their hostility to the Newtonian theory. Huygens, although he generally speaks of Newton in terms of profound admiration, was so strongly impressed with his own peculiar notions of gravity, that he failed to appreciate the force of the reasoning by which the doctrines of his contemporary were supported. He admitted the mutual gravitation of the planets and satellites according to the law of the inverse square of the distance; but he could not be persuaded to extend the same principle to the material molecules of which the several bodies are composed. He had adopted

Descartes' notion of a vortex, to explain the descent of bodies at the earth's surface; but in order to account for their invariable tendency to the centre of the earth, and not to the axis, he supposed the ethereal medium composing the vortex to circulate round the earth in all directions. In accordance with these views, he considered the force of gravity to be equally intense at all equal distances from the centre of the earth; and his investigation of the figure of the latter was founded simply on the statical relation connecting the absolute value of gravity with the centrifugal force generated by the diurnal motion. Alluding in one of his works to Newton's researches relative to the figure of the earth, he says that they are based upon a principle which appears to him inadmissible, inasmuch as it supposes that all the particles of matter attract each other; but this he contends to be an unfounded assumption, which cannot be reconciled with the established laws of mechanics. On another occasion his language, though more cautious, is decidedly hostile to the doctrines of the English philosopher. "Newton," says he, "believes that the space between the celestial bodies is void; or at least that the fluid pervading it is so rare as not to affect the motions of the planets; but, if this were true, my explanation of light and gravity would be entirely overthrown." It is interesting to remark the sound views by which this distinguished philosopher was guided when his mind was not wholly under the influence of his own favourite notions. In course of some allusions to the Cartesian theory, he thus expresses his deliberate opinion respecting the merits of that celebrated fiction. "The entire system of Descartes, concerning comets, planets, and the origin of the world, rests upon so weak a foundation, that I wonder how the author of it took the trouble of arranging so many reveries. We should have achieved a great step if we succeeded in forming a clear idea of what really exists in nature, but we are still very far from having attained that end."*

Leibnitz and John Bernouilli were equally conspicuous in their opposition to the Newtonian theory. In 1689 Leibnitz published a physical dissertation in the Leipsic acts, in which he explained the motions of the planets by means of an ethereal fluid, somewhat after the manner of Descartes. By the aid of several arbitrary assumptions, he succeeded in shewing the possibility of an elliptic motion in a vortex, and hence deduced the law of the inverse square of the distance; but it is remarkable that, although he was indebted to Newton for the suggestion of this law, he merely incidentally mentions the name of the English philosopher in connexion with it; and appears to be totally ignorant of the Principia, although two years had passed since it was published. "I see," says he "that this law has been already deduced by the celebrated geometer, Isaac Newton, as appears from an account of it given in the Leipsic acts, but I am unacquainted with the mode by which he arrived at it."

In France, the Cartesian philosophy, as may naturally be supposed, was for a long time even more popular than in any other country. Cassini, and Maraldi, persisted till their deaths in rejecting the theory of gravitation; and their example was generally followed by contemporary astronomers. The earliest historical recognition of Newton's principles in France, is contained in a memoir by Louville, which appeared in the volume of the Academy of Sciences for the year 1720. The motion of a body in an elliptic orbit is there explained by means of two forces—the one a momentary impulse directed along the tangent; the other a continuous force tending towards the focus of the ellipse. Maupertius was the first astronomer of France who undertook a critical defence of the theory of gravitation. In his treatise on the figures of the celestial bodies, which appeared in the year 1732, he compared together the theories of Descartes and Newton, and concluded by expressing a strong opinion in favour of that of the latter philosopher. The person, however, who contributed most to the general diffusion of the doctrines of gravitation in France, was unquestionably Voltaire. In 1738 that celebrated writer published a brief but very luminous exposition of Newton's most important discoveries in optics and astronomy. Being written in a popular style, this little work soon found its way into all ranks of society; and from the time of its first appearance we may date the triumph of Newton's principles over those of his once redoubtable rival.

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* Kosmotheoros sive de Terris Celestibus earumque natura conjecturæ. 4to, Hagæ, 1698.

Although Physical Astronomy may be considered as almost stationary during the period we have been considering, there were causes in silent operation which contributed powerfully to its future developement. Since the time of its invention, by Newton and Leibnitz, the infinitesimal analysis continued to be assiduously cultivated by the most eminent mathematicians of Europe, and was rapidly advancing to a high state of perfection. Without the aid of this powerful instrument of research, it would have been impossible to determine with precision the minute irregularities which take place in the motions of the planets in virtue of their mutual attraction. Newton, in his investigations, had applied the ancient geometry with almost superhuman address; but he appeared to have utterly exhausted its resources, and no other course remained for his successors than to devise other methods of greater fertility and more easy application. Leibnitz, and the two Bernouillis, by means of their brilliant researches in the new calculus, were unconsciously promoting this desirable end. These eminent analysts little imagined, while sneering at the theory of gravitation, that their own labours were destined to become subservient in reconciling its most minute consequences with the observed motions of the celestial bodies, and thereby in placing it for ever beyond the reach of cavil. The researches in mechanics, which engaged the attention of geometers during this period, also exercised a favourable influence in preparing men's minds for the consideration of the great questions relating to the system of the world. This branch of science appeared to offer an unlimited field of original speculation, until D'Alembert*, in 1740, discovered a general principle by means of which every question of motion was immediately reducible to a corresponding one of equilibrium. The statical equations being easily formed, the difficulties attending all such researches henceforth assumed a purely analytical character. It is not improbable that this important generalization had the effect of directing the attention of geometers to physical astronomy, which now presented the most inviting field of study.

The success which attended Newton's efforts to explain the phenomena of the system of the world, by the principle of universal gravitation, was well calculated to encourage his followers to engage in similar researches. Not only did he give a complete theory of the motion of two bodies revolving under the influence of their mutual attraction, but, with unrivalled sagacity, he also traced the various disturbing effects produced by the action of a third body upon either of them, and even actually computed several of the more important inequalities in the moon's motion. He did not attempt to investigate the effects of the mutual attraction of the planets, but he clearly perceived that the elliptic motion of each would in consequence be more or less deranged; and he especially remarked that the action of Jupiter on Saturn, when these two planets were in conjunction, attained such a magnitude that it could not be overlooked *. In one important instance Newton signally failed in reconciling his theory with observation. We allude to his attempt to determine the motion of the lunar apogee, on which occasion he obtained a result equal only to half the quantity which observation assigned. This discordance was naturally considered as offering a serious objection to the Newtonian theory; for the evection, which is the largest inequality in the moon's longitude, after the elliptic inequality, depends, to a certain extent, on the motion of the apogee, and therefore it still remained inexplicable by the principle of gravitation.

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* Born at Paris, 1717; died in 1783.

Euler appears to have been the first geometer who attempted the developement of physical astronomy beyond the point at which the founder of it had left it. In 1745 he investigated the perturbations of the moon, and in the following year he constructed new lunar tables based upon his researches; but, as he employed few observations in determining the maximum values of the inequalities, his tables did not present a marked superiority over those in actual use. About the same time Clairaut † and D'Alembert, two of the first geometers of France, undertook the investigation of the lunar perturbations without any knowledge of each other's intentions.

The Academy of Sciences of Paris having offered their prize of 1748, for an investigation of the irregularities of Jupiter and Saturn, Euler ‡ composed a memoir on the subject, which he transmitted to the Academy in the month of July, 1747. The two geometers above mentioned, naturally imagining that their eminent contemporary might anticipate them in their researches, took the precaution of communicating the result of their labours to the Academy before the time appointed for the award of the prize. Clairaut lodged his memoir in the hands of the Secretary on the 9th of November, 1747, and D'Alembert on the 15th of the same month. In all the three memoirs, the perturbing action of the celestial bodies was investigated by an analytical process. Clairaut mentions that he first endeavoured to calculate the lunar inequalites after the manner of Newton; but, having been soon stopped by insuperable difficulties, he decided upon having recourse to analysis alone in all his researches.

The subject, even when so treated, is one of astonishing intricacy; but,

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* Newton remarked that when Jupiter and Saturn are in conjunction, the action of Jupiter upon Saturn is to the action of the sun upon the same planet, as 1 to 211: "whence," says he, "there arises, in each conjunction with Jupiter, a derangement of Saturn's orbit, which is so sensible, as to be the cause of embarrassment to astronomers." Princip., b. iii. prop 13. Euler, however, discovered by analysis that the corresponding derangement of Jupiter is about six times greater, although the action of Saturn upon that planet is to the action of the sun only as 1 to 500. "This remark of Euler's," says Laplace, "shows us that we ought not to adopt, but with extreme reserve, the most plausible appearances so long as they are not verified by decisive proofs." Méc. Cél., tome v. p. 302.

† Born at Paris, 1713; died, 1765.

‡ Born at Basle, 1707; died at St. Petersburg, 1783.

fortunately, the planetary system is so constituted as to favour the researches of the mathematician. The problem of a planet's motion, when considered in its most general sense, requires that we should include in one common investigation the attractive forces exerted upon the planet by the various bodies composing the solar system. The sun, however, exercises such a preponderating influence, on account of his enormous mass, that we may regard each of the planets as revolving round him in an orbit, approaching very closely to an ellipse; while the other planets may be considered as so many perturbing bodies, producing continual irregularities in the elliptic motion. These perturbations being very minute, the action of each planet may be investigated in succession, without taking into account the simultaneous action of the others; and the aggregate of the results so obtained, when applied to the elliptic motion, will determine the true place of the planet in its orbit. The whole question is, therefore, reduced to the investigation of the motion of one body revolving round another, and continually disturbed by the attraction of a third body. Thus originated the famous Problem of Three Bodies, which has formed the basis of so much profound research in physical astronomy. The rigorous solution of this problem has been found to surpass the powers of the understanding, notwithstanding the many improvements which have been effected in the infinitesimal analysis; but the same considerations, which limit the investigation to the mutual attraction of three bodies, conduct also to other important simplifications. The masses of the planets being in fact very small, compared with the sun's mass, and the eccentricities as well as the inclinations of the planetary orbits being also very inconsiderable, a number of terms involving these elements in the general solution of the problem become, in consequence, so small as to admit of being rejected; and the geometer is thereby enabled to bring the subject within the reach of his analysis. Notwithstanding these obvious advantages, the utmost resources of a profound calculus, combined with the most consummate analytical skill, are indispensably required, in order to effect a solution of this difficult problem; and even then the object can only be attained by a process of successive approximation. In the lunar theory, the principal disturbing body is the sun; for the planets are either too small or too remote to exercise much influence. It might naturally be supposed that the sun, on account of his enormous mass, would very much derange the moon's motion; but in reality the effect of his attractive power is greatly diminished by the immense distance at which he is placed compared with the earth, which is in this case the central body. Still the inequalities of the moon's motion are much more considerable than the perturbations which take place in the motions of the planets; and, on this account, they were justly considered to afford the most favourable means of testing the theory of gravitation. We have already alluded to the failure which attended Newton's attempt to determine the motion of the lunar apogee. Singular enough, when Clairaut and the other two geometers above mentioned deduced the motion of the apogee from their respective analytical solutions of the problem of the lunar perturbations, they found, like Newton, that the result was equal only to half the observed motion. This anomalous fact excited great surprise in the scientific world, and many persons began to entertain a strong suspicion that the law of gravitation, as announced by Newton, was erroneous. Clairaut, despairing of being able to reconcile the ordinary law with the results of observation, proposed that, instead of representing the force by a term depending on the inverse square of the distance, it should be expressed by two terms, one composed of the inverse square, and the other of the inverse of the fourth power of the distance. Buffon adduced metaphysical arguments against this law; and the question continued to excite a deep interest among men of science. At length Clairaut discovered that, when the lunar perturbations were rightly computed, according to the Newtonian law, the motion of the apogee, when so computed, was exactly conformable to the observed motion.

He found, in fact, by repeating the approximation and taking into account certain small terms which he had previously neglected, that the value obtained by him in the first instance was now exactly doubled. D'Alembert and Euler, upon a revisal of their labours, arrived at the same conclusion: and thus a circumstance, which at one time threatened to subvert the whole structure of the Newtonian theory, resulted in becoming one of its strongest confirmations.

It is right, also, to mention that Thomas Simpson arrived at the correct motion of the apogee before he learned the successful result of Clairaut's labours. This eminent analyst might have done much to sustain the reputation of his country in the researches of physical astronomy if he had lived under more auspicious circumstances.

The method of lunar distances, which offers such advantages in finding the longitude at sea, rendered an accurate knowledge of the moon's motion peculiarly desirable. In 1754, Clairaut and D'Alembert published lunar tables based upon their respective theories. Those of Clairaut obtained considerable credit for accuracy; but D'Alembert's efforts were less fortunate, chiefly in consequence of having paid too little attention to observation in the evaluation of his coefficients. In 1755 Euler published his researches in the lunar theory, accompanied with new tables, greatly superior in accuracy to those of 1746. In his analysis he resolved the forces acting upon the moon along three rectangular co-ordinates, after the example of Maclaurin, who, a few years previously, had first employed this method in his elegant geometrical investigations connected with the question of the Tides. In 1772 he published a third set of tables, based upon a most elaborate analysis of the moon's motion; but, notwithstanding the amount of thought expended on them, they proved inferior in point of accuracy to those of Mayer, chiefly in consequence of his having placed too much reliance on theory in fixing the maximum values of the equations. Mayer, to whom allusion has been already made, was the first person who constructed lunar tables of sufficient accuracy for the great practical purpose of finding the longitude at sea. This he did in 1755, by means of Euler's theory and a skilful discussion of observations. These tables were found to come within the limit of accuracy fixed by the Board of Longitude of this country; and a recompense of £3000 was in consequence awarded to the widow of Mayer, the astronomer himself having died some years before this decision was come to. Bradley, who was appointed to compare the tables with observation, states, in his report of them, that in no case did the error exceed $1\frac{1}{4}'$. They were first printed in the year 1770.

CHAPTER IV.

Perturbations of the Planets.—Inequality of Long Period in the Mean Motions of Jupiter and Saturn.—Researches of Euler.—Perturbations of the Earth.—Clairaut.—Perturbations of Venus.—Lagrange.—His Investigation of the Problem of Three Bodies.—Secular Variations of the Planets.—Laplace.—His Researches on the Theory of Jupiter and Saturn.—Invariability of the Mean Distances of the Planets.—Oscillations of the Eccentricities and Inclinations.—Stability of the Planetary System.

The planets, while revolving round the sun, continually disturb each other by their mutual attraction, and hence arise numerous inequalities in their motions, similar to those which take place in the motion of the moon round the earth. Although these disturbing forces form a class of relations as complicated as the mind can perhaps imagine, the study of their effects is on many accounts peculiarly attractive to the thoughtful enquirer. The fundamental ideas are clear and well defined; the principles are firmly established, the methods of research are derivable wholly from the resources of the intellect, and the subject is both vast in extent and varied in character. The magnificent prizes which the theory of gravitation offers prospectively to the mathematician, as the rewards of his labours, are also calculated to allure his researches, while its extreme intricacy serves only to redouble his energies, and stimulate his inventive powers. Hence Physical Astronomy is characterized by a multitude of conceptions at once ingenious, subtle, and profound, while its investigations are pursued, throughout their long and intricate windings, with a coherence and beauty of ratiocination unequalled in any other branch of Natural Philosophy.

Apart from those more obvious questions which impart an interest to the study of Celestial Mechanics, others of the highest moment, with respect to the stability of the system of the world, are also involved in the subject. These questions naturally offered themselves to mathematicians, while engaged in researches connected with the actual motions of the planets, and continued for some time to form the subject of profound study. Their complete solution will ever be ranked among the most brilliant triumphs recorded in the annals of science, while it has shed an imperishable lustre on the names of those eminent individuals by whose labours it has been achieved.

The masses of the planets being small compared with the mass of the central body, the derangements occasioned by their mutual attraction do not in any case attain a magnitude comparable to that of the lunar inequalities. Indeed their existence has generally been established only by a comparison of distant observations, conducted with all the refinements of practical astronomy. In many instances theory has preceded observation, and has pointed out inequalities which, on account of their extreme minuteness, might otherwise have for ever escaped detection. The planets Jupiter and Saturn, being favourably placed in the system for the exertion of their mutual attraction, and their masses being also considerable, it might be expected that the inequalities of their motions would be more readily appreciable than those of the other planets. In fact, as early as 1625, Kepler remarked that the observed places of these planets could not be reconciled with the usually admitted values of their mean motions, The errors of both planets were found to increase continually in the same direction, with this difference, that the tables made the mean motion of Jupiter too slow, and that of Saturn too quick. Lemonnier found that, by adopting the mean motion of Saturn, as determined by a comparison of ancient with modern observations, the planet had fallen behind its computed place to the extent of 2′ in 1598, 20½′ in 1657, and 36½′ in 1716.

Halley first suspected that the anomalous irregularities of the two planets were due to their mutual attraction. He also attempted to determine the magnitude of the inequality for each planet. He concluded from his researches that in 2000 years the acceleration of Jupiter amounted to 3° 49′, and the retardation of Saturn to 9° 16′. In his tables of the planets he represented the errors by two secular equations increasing as the square of the time, the one being additive to the mean motion of Jupiter, and the other subtractive from the mean motion of Saturn.

The Academy of Sciences of Paris, desirous of obtaining an explanation of these inequalities, in accordance with the theory of gravitation, offered its prize of 1748 for their complete investigation. Euler was induced to compose a memoir on the subject, which was crowned by the Academy; but, although his researches contain a valuable exposition of the analytical theory of planetary perturbation, he was unable to throw any light on the main object of the inquiry. He found a series of inequalities in the mean motions of both planets, but they were all such as completed the cycles of their values every time that the planets returned to the same configurations. He concluded, therefore, that the observed irregularities must be attributed to some extraneous cause, and not to the mutual attraction of the two planets.

Euler in this memoir resolved the differential equation, relative to the latitude of the disturbed planet, into two differential equations of the first order, one of them expressing the differential of the inclination, and the other that of the planet's distance from the node. This may be considered as the germ of the famous method of the variation of arbitrary constants.

The theory of Jupiter and Saturn offers some difficulties of a peculiar kind, which did not occur in the investigation of the lunar inequalities. The disturbing action of one body upon another may be expressed by a series of terms involving the ratio of the mean distances of both from the central body. In the lunar theory this fraction is very small, on account of the great distance of the sun, which is the disturbing body; hence the terms converge with great rapidity, and an approximate value of the series is readily obtained. When the question, however, refers to the mutual action of Jupiter and Saturn, the same fraction rises to a considerable magnitude, and the terms of the series converge in consequence with such extreme slowness, as to render impracticable the usual method of computation. Euler's genius was eminently conspicuous in devising the means of vanquishing this difficulty, which would effectually have obstructed a mind gifted with less fertile powers of invention.

The explanation of the motion of the lunar apogee by Clairaut in 1749, having inspired renewed confidence in the principle of gravitation, as adequate to account for all the phenomena of the planetary motions, the Academy of Sciences was again induced to propose the theory of Jupiter and Saturn as the subject of their prize for 1752. Euler was on this occasion also the successful competitor, but he now actually discovered secular equations in the mean motions of both planets, depending on the angular distance between the aphelia of their orbits. Contrary to observation, however, he found that the two equations were equal in magnitude, and were in both cases additive to the mean motion. He fixed the inequality at 2′ 24″ for the first century, counting from 1700. Nothwithstanding the analytical skill which this geometer displayed in his researches, he signally failed in his efforts to account for the irregularities of the two planets by the Newtonian theory; and their physical origin, therefore, still continued to be involved in profound mystery.

The attention of geometers was now directed to the perturbations in the earth's motion occasioned by the other planets. Euler investigated this subject in an elaborate memoir, which was crowned by the Academy of Sciences in the year 1756. It was on this occasion that he explained and partially developed the theory of the variation of arbitrary constants. In considering the motion of a planet in an elliptic orbit, there are six constants or elements, which by their independent variations would severally modify the motion. These are—1°, the major axis of the orbit, or the mean distance; 2° the eccentricity; 3°, the position of the line of apsides; 4°, the inclination of the orbit with respect to a fixed plane; 5°, the position of the line of nodes; 6°, the longitude of the planet at any assigned instant, or the longitude of the epoch, as it is called. Now if the planet were exposed only to the action of the sun these elements would remain invariable, and the planet would continually revolve in the same ellipse. Its place, corresponding to any given time, might therefore be readily computed, by means of Kepler's law of the areas, when once these six elements were known. As, however, it is continually disturbed in its motion by the action of the other planets, the theory of a constant ellipse will no longer be applicable to the question. Still, as its aberrations from an elliptic orbit are very small, its place may be computed by assuming it to move in a mean ellipse, and then ascertaining the minute irregularities occasioned by the perturbing forces. This is the course which geometers had hitherto pursued in all researches connected with the problem of three bodies. Euler, however, proposed to compute the motion wholly by the elliptic theory, upon the supposition that the planet continually revolved in an ellipse, the elements of which varied every instant from the action of the other planets. By these means the whole effect of perturbation was thrown upon the elements of the orbit, and when these were ascertained for any given instant, it was easy to calculate the corresponding place of the planet by the elliptic theory alone. As this refined conception has not unfrequently been ascribed to Lagrange, it may be proper to cite Euler's own words in reference to it. After obtaining the differential expressions of the elements, he then proceeds in the following terms to point out their advantages: "These formulæ appear to be peculiarly commodious in computing the deviations of the motion from Kepler's laws; since they have reference to motion in an ellipse, which varies continually, as well in respect to the parameter as to the eccentricity and the position of the apsides. For, during an indefinitely small portion of time, the motion of the planet may be conceived as taking place in an ellipse, according to the laws of Kepler; and, if the elements of this ellipse be computed for any given time, by means of the formulæ just found, the true place of the planet, relative to an assumed plane, may be also assigned."* This investigation of Euler's, like the two previous ones, displays abundant proofs of the amazing fertility of his inventive powers, and his great command of analysis; but in regard to the final results obtained by him he was not equally fortunate. Grave errors of calculation prevented him, on this as well as on several other occasions, from duly appreciating the importance of his own methods.

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* Prix de l'Académie, tome viii. Investigatio Motuum Planetarum, p. 20

Clairaut, about the same time, investigated the Earth's perturbations in a memoir distinguished by great perspicuity and skill *. By a comparison of his theory with the observations of Lacaille, he fixed the lunar equation at 8″.7. This result gives for the moon's mass $\frac{1}{67}$ of the earth's, a quantity which differs considerably from the value assigned to it by Newton. In order to compute the actual perturbations occasioned by the other planets, it was necessary to possess a knowledge of their masses. He skilfully determined the mass of Venus by means of observations made on the sun when the moon was in that part of her orbit wherein she produced no effect on the earth's motion. The mass of Jupiter, the only other planet that he conceived would occasion a sensible derangement of the Earth, was easily derivable from the elongations of his satellites, and had already been determined by Newton. Combining together the perturbations of Jupiter, Venus, and the Moon, he found that when they conspired in the same direction the error of the Earth in longitude might rise to 1′.

D'Alembert also investigated the subject of the planetary perturbations in the year 1754 †; but his researches did not add anything new to the subject. Lalande applied Clairaut's theory to the perturbations of Mars, by Jupiter and the Earth, and found that the derangement might rise to 2′. Mayer, about the same time, arrived at a similar conclusion by means of Euler's theory. Lalande also computed the perturbations of Venus, and obtained 15″ for the maximum value; a result which was confirmed by similar researches of Father Walmsley in England.

Meanwhile another geometer, gifted with powers of the highest order, was about to commence his brilliant career. In the volume of the Turin Memoirs for 1763, Lagrange ‡ gave a new solution of the Problem of Three Bodies, which he applied to the theory of Jupiter and Saturn. He obtained for Saturn a secular equation equal to 14″.221, and subtractive from the mean motion; and for Jupiter a similar equation equal to 2″.740, and additive to the mean motion. This result agreed better with observation than that of Euler, who made the equations both additive and equal in magnitude; but it by no means assigned a complete explanation of the irregularities in the mean motions of the two bodies.

Attention was now directed to the secular inequalities of the planets. A comparison of distant observations had shown that the elliptic elements of each planet were subject to a slow variation, which proceeded continually in the same direction, and apparently to an indefinite extent. These variations have been denominated secular because they require an immense number of ages for their complete developement; while, on the other hand, those that are termed periodic complete the cycles of their values with all similar configurations of the planets. It is obvious that this shifting of the elements, however slow, would ultimately render useless the tables of the planets, constructed for any given epoch, unless due account were constantly taken of the altered value of each element.

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* Mém. Acad. des Sciences, 1754.

† Recherches sur differens points du Systême du Monde, tome 1 and 2.

‡ Born in 1736 at Turin; died at Paris in 1813.

It is indispensable then to investigate these variations, and to compute their numerical values, with the view of applying them as corrections to the fundamental elements of the tables. But, apart from all considerations connected with the requirements of practical astronomy, the study of the secular variations is pregnant with the deepest interest to the physical inquirer; for it is manifest that their indefinite continuance in the same direction would result in the complete destruction of the stability of the planetary system.

The illustrious Euler led the way in these sublime researches. In his memoirs of 1748 and 1752, he determined the secular variations of the elements of Jupiter and Saturn; but he was prevented by the intricate nature of the subject, and the immense calculations which it entailed upon him, from arriving at very accurate conclusions. He found that the aphelia of both planets had a progressive motion, as in the case of the lunar apogee. He fixed the annual progression of Jupiter's aphelion at 9″.5, and that of Saturn's at 13″.

But the most important element was the mean motion. We have already mentioned that Euler found a secular inequality in that element, equal and additive for both planets, and that Lagrange was conducted to a result which accorded better with the observed irregularities, but still was inadequate to their complete explanation. It was at this stage of the planetary researches that Laplace*, for the first time, appeared as the rival of Lagrange. Struck with the discordant conclusions to which geometers had been conducted, he resolved to institute a searching investigation into the subject. Euler and Lagrange had neglected all terms which exceeded the second powers of the eccentricities and inclinations; Laplace, besides carefully repeating the calculations of these geometers, carried his approximation to the terms of the third order. When he came to apply his formula to the mean motion of Saturn, he was surprised to find that all the terms affecting that element destroyed each other. He obtained a similar result, when, by means of the same formula, he computed the effect of Saturn's disturbing action upon the mean motion of Jupiter. Justly suspecting that these results had no connexion with the particular values of the elements of Jupiter and Saturn, he investigated the subject by a general analysis, applicable to any two planets of the system, and he now again found that the sum of the terms affecting the mean motion was identically equal to zero. He remarked, that if any such terms were contained among those involving the fourth or higher powers of the eccentricities and inclinations, they would be so small that their effects would not become sensible until an immense number of ages had elapsed. He therefore arrived at the important conclusion that, from the time of the earliest astronomical observations which history records down to the existing epoch, the mean motions of the planets has not been sensibly altered by their mutual attraction, and hence he inferred that the irregularities of Jupiter and Saturn must be attributed to some disturbing cause independent of that principle†.

Although the result to which Laplace was conducted by his researches was decisive in so far as the question relative to the irregularities of Jupiter and Saturn was concerned, it still remained uncertain whether an inequality of this kind might not exist among the terms involving the higher powers of the eccentricities and inclinations. Such an inequality, however minute, would ultimately become considerable by continual accumulation, and might affect the mean motions and the mean distances of the planets to so great an extent as to occasion the total derangement of the planetary system. In 1776 Lagrange investigated this important question * on the supposition that the planets move in ellipses; the elements of which continually vary in consequence of their mutual perturbations. The conclusion at which he arrived is one of a very remarkable character. He discovered, by a very simple analysis, that the mean distances are not subject to any secular variations whatever; but are merely affected by a series of inequalities, which compensate themselves in short periods depending on the mutual configurations of the different planets. Thus, amid all the changes that are incessantly taking place in the other elements of the celestial orbits, the conservation of the mean distances stands out in striking contrast to this law of mutation. The eccentricities and inclinations will perpetually vary in magnitude; the apsides and nodes will similarly vary in position; but, throughout an indefinite lapse of ages, the mean motions of the planets will remain unaltered by their mutual attraction. This result offers to our contemplation a sublime example of the order which reigns among the vast bodies of the universe, and of the unerring character of the laws by which they are controlled in their courses. When viewed in relation to its effects upon the invariability of the solar year, and the stability of the planetary system, it is justly regarded as one of the most valuable truths in the whole range of physical science.

Lagrange and Laplace were now zealously engaged in investigating generally the secular variations of the planets. A knowledge of the variations of all the elements is indispensable for the purposes of practical astronomy; but, in so far as the stability of the system is concerned, it is manifest that the variations of the apsides and nodes cannot produce any effect. In the researches of physical astronomy the eccentricities and the apsides are generally considered together by a common analysis, and the same remark holds good in regard to the nodes and inclinations. In 1774 Lagrange investigated the secular variations of the nodes and inclinations of the planets in an elaborate memoir, which appeared in the volume of the Academy of Sciences for the same year. Having obtained the differential expressions of these elements, he computed their annual variations by assuming each element to vary at a uniform rate during a limited number of years. This supposition is rendered allowable by the excessive slowness with which the elements vary, and the method of computation founded on it is sufficient for all purposes connected with the actual state of astronomy. But in order to ascertain the real nature of these variations, so as to be enabled to predict the condition of the elements throughout an indefinite number of ages, it is absolutely necessary to integrate the differential equations relative to them. These appeared at first to offer insuperable difficulties; but Lagrange, by a happy transformation of the variables, reduced them to linear differential equations of the first order; whence, by integration, he obtained finite expressions for both elements. Examining, then, the mutual action of two planets, he found that the inclinations would perpetually oscillate about mean values from which they would deviate only by very small quantities. Applying this principle to the planets Jupiter and Saturn, which he considered as form-

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* Mém. Acad. des Sciences de Berlin, 1776.

ing with the sun a system apart in the heavens, he found that the greatest inclination of Jupiter's orbit to the ecliptic would be 2° 2′ 18″, and his least 1° 17′ 15″; and that the greatest inclination of Saturn's orbit would be 2° 32′ 41″ and his least 46′ 49″. Thus the whole variation of Jupiter's inclination amounted to 45′ 3″, and that of Saturn's to 1° 45′ 51″. When the number of planets whose mutual action is considered exceeds two, the question offers greater difficulties; and Lagrange, in consequence, did not at this period of his researches attempt to inquire whether the inclinations of the four planets nearest the sun oscillated round mean values, like those of Jupiter and Saturn, or whether they increased continually in the same direction, attaining in succession all degrees of magnitude with respect to a fixed plane.

From the analytical expressions of the inclinations, he derived an elegant method of determining geometrically the positions of the orbits at the end of any given time, and of representing their several motions relatively to each other.

Laplace successfully applied Lagrange's method of integration to his own differential expressions of the nodes and inclinations. He also extended the same method to the equations of the eccentricities and the aphelia, and obtained finite expressions for those elements similar in form to those which Lagrange had already found for the nodes and inclinations[*].

The method of successive approximation which Clairaut and his contemporaries employed in the problem of the Perturbations was attended with the inconvenience of introducing into the expression of the radius vector a series of terms depending on arcs of circles which increased continually with the time. Such terms, it is clear, would have the effect of causing the planet's distance from the central body to increase to an indefinite extent; but, as this conclusion was at variance with observation, it necessarily followed that the method of approximation was defective. The same inconvenience, indeed, is found to arise when the method is employed in less complicated researches than those relating to the planetary perturbations; and wherein it is demonstrable that the rigorous integrals do not contain any terms susceptible of indefinite increase[†]. When this difficulty first presented itself to the geometers engaged with the lunar theory, they very soon discovered that it arose from the motion of the apogee, and they found the means of getting rid of the embarrassing terms by assuming the motion of that element in the outset; and afterwards computing its value by the method of indeterminate coefficients.

In the theory of the planets, the inconvenience of such terms is similarly found to arise from the motion of the aphelia; but the mode of obviating it is much more difficult in this case, on account of the irregularity of the motion, and the mutual dependence of the aphelia of both planets. Lagrange vanquished this difficulty by the invention of a new method of integration, which he first employed in an investigation of the motions of Jupiter's satellites[‡]. In an elaborate memoir which Laplace communicated a few years afterwards to the Academy, he shewed that the ordinary method of integration might be freed from the inconvenience of circular arcs by means of the variation of the arbitrary constants in the approximate integrals*. Proceeding upon this supposition, he obtained very simple differential expressions of the secular variations; and by transforming them into linear equations, after the manner of Lagrange, he was enabled to integrate them without any difficulty. He then considered the mutual action of two planets, and arrived at the same conclusion relative to their eccentricities, as that to which Lagrange had been conducted relative to their inclinations. In other words, he established the important fact that the eccentricities of the two planets would not increase to an indefinite extent in virtue of their mutual attraction, but would be confined within certain fixed limits between which they would perpetually oscillate. Applying his formula to the theory of Jupiter and Saturn, he found that the eccentricity of Jupiter's orbit would va[illegible] between 0.061776 and 0.024006, and that of Saturn's between 0.083094 and 0.011478. The period of these variations he found to be the same for both planets, and equal to about 35,000 years.

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[*] Mém. Acad. des Sciences, 1772, part i. This memoir was not written until 1776, although it was inserted in the volume of the Academy for 1772.

[†] Lagrange has given an example of this kind in a memoir which appears in the volume of the Berlin Academy for 1783.

[‡] The memoir which contained this investigation was crowned by the Academy of Sciences in the year 1766. See Prix de l'Académie, tome ix.

In 1782 Lagrange investigated the perturbations of the planets by a method which embraced both the secular and periodic inequalities in one common analysis†. This method consists in supposing *all* the derangements of each planet to be occasioned by a continual variation of the elliptic elements. We have seen that Euler originally employed this refined method of investigation, which offers peculiar advantages in computing the effects of perturbing forces of small value. Lagrange having obtained the differential expressions of the elements, decomposed them into two parts, one depending on the configuration of the planets, the other on the masses and the elements themselves. The former class of terms gave him the periodic variations; the latter gave him the secular. Applying himself first to the secular variations, he considered the mutual action of Jupiter and Saturn, and obtained results similar to those which Laplace and himself had already derived from their researches. He next considered the system formed by Mars, the Earth, Venus, and Mercury, taking into account the action of Jupiter and Saturn upon each of those bodies. We have remarked on a previous occasion that this case is much more difficult than that in which only two planets are concerned. The genius of Lagrange, however, was triumphant in these researches, and he succeeded in demonstrating, as in the case of the larger planets, that the eccentricities and inclinations would always be confined within very narrow limits. He found that the ecliptic would not be displaced to a greater extent than 5° 23′ by the action of the planets upon the earth, and that all the planetary orbits would be perpetually comprised within a zone of the heavens whose breadth was 7° 58′. He therefore announced, as the final result of his researches, that the secular variations of the elements were in all cases such as would for ever assure the stability of the planetary system.

The same illustrious geometer extended his researches to the periodic inequalities, which he investigated in two elaborate memoirs communicated by him to the Academy of Sciences of Berlin [*]. He derived the analytical expressions of these inequalities from the periodic variations of the elements, and then computed their numerical values for each planet.

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* Mém. Acad. des Sciences, 1772, part ii. Laplace had given a brief outline of this method in a note at the end of the preceding volume. He continued to improve it in two successive memoirs, which appeared in the volumes of the Academy for 1777 and 1789. In the last memoir the method assumed the same form as in the Mécanique Céleste. Lagrange has very much simplified this mode of obtaining the secular variations in a memoir which appears in the volume of the Berlin Academy for the year 1783.

† Mém. Acad. Berlin, 1781, 82, 83.

The interesting results obtained by Lagrange relative to the stability of the system were founded upon a knowledge of the masses of the several planets. The computation of the masses of those planets that are accompanied by satellites is not a difficult problem, but it is quite different when the question refers to the other planets of the system. Theoretically speaking, the masses of all the planets may be ascertained by observing the effects of their mutual perturbations, but these effects are generally so very minute that they are almost entirely lost in the errors of observation. Lagrange determined the masses of the planets that have no satellites by combining their volumes with their densities, assuming the latter to vary in the inverse ratio of the planet's distance from the sun. This principle, although naturally suggested by the relative densities of the Earth, Jupiter, and Saturn, was, notwithstanding, gratuitously assumed, and therefore the consequences derived from it could not be altogether free of uncertainty. Lagrange, indeed, shewed the improbability of any minute alteration in the values of the masses affecting essentially the conclusions at which he arrived; but still it was desirable that such valuable truths should be established by an analysis divested of all considerations of a hypothetic character. This important step was made by Laplace [†]. In 1784 he demonstrated that, no matter what might be the relative masses of the planets, the eccentricities and inclinations if once inconsiderable would always continue so, provided the planets were subject to this one condition—*that they all revolved round the sun in the same direction.* This remarkable truth is embodied in two elegant theorems, which the great geometer just mentioned was the first to announce to the world. The theorem relative to the oscillations in the form of the orbits may be thus stated: *If the mass of each planet be multiplied by the square of the eccentricity, and this product by the square root of the mean distance, the sum of these quantities will always retain the same magnitude.* Now when this sum is determined for any given epoch, it is found to be small; by the preceding theorem, then, it will always continue so; it follows, therefore, *à fortiori*, that each quantity will continue small, and, consequently, the eccentricity cannot in any case become considerable. The theorem relative to the positions of the orbits is equally elegant. It may be expressed in the following terms: *If the mass of each planet be multiplied by the square of the tangent of the orbit's inclination to a fixed plane, and this product by the square root of the mean distance, the sum of such quantities will continue invariable.* Considerations similar to those we employed in the previous instance enable us to conclude from this theorem that the orbits of the planets will suffer only a very inconsiderable displacement from their mutual attraction [‡].

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[*] Mém. Acad. Berlin, 1783-4.

[†] Mém. Acad. des Sciences, 1784. This memoir of Laplace's is remarkable for containing the first announcement of three of the most important discoveries in Physical Astronomy. These were—1st, the explanation of the long inequality of Jupiter and Saturn; 2nd, the investigation of the origin of the curious relations which connect the epochs and mean motions of the three interior satellites of Jupiter; 3rd, the results mentioned in the text.

[‡] The value of Laplace's researches on the present occasion does not rest merely on the discovery of the two theorems announced in the text. The fact is, that the investigation

The laws which thus regulate the eccentricities and inclinations of the planetary orbits, combined with the invariability of the mean distances, secure the permanence of the solar system throughout an indefinite lapse of ages, and offer to us an impressive indication of the Supreme Intelligence which presides over nature, and perpetuates her beneficent arrangements. When contemplated merely as speculative truths, they are unquestionably the most important which the transcendental analysis has disclosed to the researches of the geometer, and their complete establishment would suffice to immortalize the names of Lagrange and Laplace, even although these great geniuses possessed no other claims to the recollection of posterity.

It cannot fail to have occurred to the reader that in these sublime researches the two mighty rivals pressed forward always at an equal pace, insomuch that it would be hardly possible for the most discerning judgment to assign the palm of superiority to either of them. Their investigations of the secular variations were in both cases equally original, and equally entitled to admiration. Laplace's method might be more simple; Lagrange's was more luminous, and had the advantage of being direct. In his researches connected with the mean motion, Laplace displayed a practical sagacity which rarely characterized the speculations of Euler or Lagrange, and, perhaps, this quality was more valuable to him throughout his career than an unexampled command of analysis was to his great rival. In the integration of the differential equations relative to the secular variations of the planets, the genius of Lagrange was eminently conspicuous. Laplace admits that he was compelled to abandon the design of integrating his own equations on account of the difficulties they offered, and was only induced to resume the subject on becoming acquainted with the ingenious method devised for that purpose by his illustrious contemporary *.

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the resolution of an algebraic equation, equal in degree to the number of planets whose mutual action is considered, and involving their masses in indeterminate forms. Lagrange shewed that if any of the roots of this equation should be equal or imaginary, the corresponding element (whether the eccentricity or the inclination) would increase to an indefinite extent, but if the roots should be all real and unequal, the same elements would perpetually oscillate between fixed limits. Having ascertained the masses of the planets by the methods mentioned in the text, he substituted them in each equation, and then, by the method of successive approximation, he obtained the values of the several roots. These he found to be all real and unequal, whether the equation referred to the eccentricities or the inclinations, whence he concluded that these elements would perpetually oscillate. The peculiar merit of Laplace's researches consisted in shewing that the roots were all real and unequal, without having recourse to the actual solution of the equations, and, consequently, without the necessity of employing any determinate values of the masses.

* "Je m'etais proposé depuis long temps de les integrer mais le peu d'utilité de ce calcul pour les besoins de l'Astronomie joints aux difficultés qu'il présentait m'avait fait abandonner cette idée et j'avoue que je ne l'aurais pas reprise sans la lecture d'un excellent mémoire, sur les inegalités seculaires du mouvement des nœuds et de l'inclinaison des orbites des Planetes que M. De Lagrange vient d'envoyer à l'Académie." Mém. Acad. des Sciences, Année 1772, part i. p. 371.

CHAPTER V.

Irregularities of Jupiter and Saturn.—Researches of Lambert.—Lagrange.—Circumstances which determine the Secular Inequalities in the Mean Longitude.—Laplace's Investigation of the Theory of Jupiter and Saturn.—His Discovery of the physical cause of the Long Inequality in their Mean Motions.—Acceleration of the Moon's Mean Motion.—Halley.—Dunthorne.—Failure of Euler and Lagrange to account for the Phenomenon.—Its explanation by Laplace.—Secular Inequalities in the Moon's Perigee and Nodes.—Inequalities depending on the Spheriodal Figure of the Earth.—Parallactic Inequality.

ALTHOUGH the principle of gravitation was shewn to be admirably calculated for maintaining the stability of the solar system, the strange irregularities in the mean motions of Jupiter and Saturn still continued to perplex astronomers, and in some degree to tarnish the lustre of the Newtonian theory. In 1773 Lambert published an interesting essay on this subject, in which he attempted to represent the inequalities of the planets by means of empiric equations. The researches of this astronomer contributed to throw some light upon the real character of the phenomenon. It had been hitherto supposed that the mean motion of Jupiter was continually accelerated, and that of Saturn similarly retarded. These results were derived from a comparison of the observations cited by Ptolemy in the Syntaxis, and those of the earlier astronomers of Europe, with the observations of modern times. Lambert, however, found, on comparing the observations of Hevelius with those of the following century, that the mean motion of Jupiter was retarded, while that of Saturn was accelerated. This important fact indicated that the inequalities did not increase indefinitely in the same direction, but were merely periodic, like those depending on the configurations of the planets. The researches of Lagrange, in 1776, tended to strengthen this conclusion; but it is important to remark, that the result he obtained relative to the invariability of the mean distance does not *necessarily* exclude the existence of secular inequalities in the mean motion. When, indeed, we consider a single planet revolving round the sun in an undisturbed orbit, the mean motion will depend solely on the mean distance, and will not in any manner be affected by the elements which prescribe the form and position of the orbit. Thus we may make the eccentricity and the other elements vary in any manner we please, but so long as the mean distance retains the same magnitude the mean motion will continue unalterable. The relation which connects these two elements forms the third of Kepler's famous laws, and when one of them is known the other is readily deducible from it by means of that relation. But the case will be quite different when we suppose the planet to be perpetually disturbed in its orbit by the action of another planet. The two elements will no longer be connected together by Kepler's law, for the perturbing forces will now introduce into the expression of the mean motion a class of terms depending upon the eccentricities and the other elements of both planets. These elements, in virtue of their secular variations, might produce an effect on the planet's longitude which would ultimately become sensible, and hence might arise a secular inequality in the mean motion, notwithstanding the invariability of the mean distance. It became, therefore, an object of the highest importance to ascertain whether the mean motions of the planets were affected in this manner, and if so, to determine, in the case of Jupiter and Saturn, whether the effects were of such a magnitude as to account for the observed irregularities of the two planets.

In 1783 Lagrange investigated this interesting question. He had previously found that if all the terms exceeding the first powers of the eccentricities and inclinations were neglected, the perturbing forces would not in any manner whatever affect the mean motion. On the present occasion he extended his inquiries to the terms involving the squares of the eccentricities, and he now actually discovered among them a secular equation affecting the mean motion. Applying his formula to the theory of Jupiter and Saturn, he found that the equation was utterly insensible in both planets; for in neither case did it exceed the thousandth part of a second, even when it reached its maximum value *. "This result," says Lagrange, "will allow us to dispense with a similar examination of the secular inequalities in the mean motions of the other planets, as we originally proposed to do, for it is easy to predict that the values of the equations will be even less than those we have just found. We may, then, henceforth consider it as a truth rigorously demonstrated, that the mutual attraction of the principal planets cannot produce any sensible alteration in their mean motions."†

This result is one of the most interesting in physical astronomy, and we have seen that the merit of establishing it is almost wholly due to Lagrange. At this stage of the planetary researches it had the effect of narrowing the question relative to the irregularities of Jupiter and Saturn; for it shewed that if these irregularities resulted from the mutual action of the two planets, their explanation must be sought for among the *periodic terms*, and not among those depending on the secular variations of the elements. It is clear, then, that, apart from its intrinsic value, this result must be considered as forming a most important step in the developement of the theory of gravitation.

It appears from Lagrange's words, as quoted above, that he did not consider himself warranted in concluding from his researches that the mean motions of the secondary planets might not be affected with secular inequalities of sensible magnitudes. Unfortunately for his fame, it did not occur to him to apply his formulæ to the moon, although the secular inequality which astronomers had actually detected in the mean motion of that satellite might have suggested such a step to a mind of much less sagacity than his. By this inadvertence he missed one of the noblest discoveries in physical astronomy, and it happened to him, as on several other occasions, that, while he allowed his brilliant researches to remain comparatively fruitless in his hands, he had the mortification of seeing the prize carried off by his more persevering and ambitious rival.

Geometers being now assured that the mutual attraction of Jupiter and Saturn could not produce an inequality of a secular character in their mean motions, it only remained for them to inquire whether the anomalous irregularities of the two planets might not be explicable by some periodic inequality of long duration. This was the form which the question assumed when Laplace applied the energies of his powerful mind to a rigorous examination of all the circumstances calculated to affect it. He first proceeded to inquire whether the inequalities were connected together by relations similar to those which would ensue on the supposition that they were produced by the mutual action of the two planets. By a very simple analysis he found that the mean motion of Jupiter would be accelerated, while that of Saturn was retarded, and *vice versâ.* He also discovered that, if we only regard inequalities the periods of which are very long, the corresponding derangements of the two planets would always be to each other as the products formed by multiplying the mass of each planet into the square root of its mean distance. He hence easily concluded that the derangement of Jupiter at any time would be to the simultaneous derangement of Saturn very nearly as 3 to 7. Now, by assuming, according to Halley, that the retardation of Saturn in 2000 years amounted to 9° 16′, this relation gave him 3° 58′ for the corresponding acceleration of Jupiter, a quantity which differs only by 9′ from the result obtained by Halley.

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* The mean longitude of a planet depends upon two elements:—1st, the mean motion; 2nd, the mean longitude corresponding to any given epoch, or, more simply, the longitude of the epoch. As the mean motion is supposed to be derivable from the mean distance by Kepler's law, it cannot affect the mean longitude with a secular inequality, in consequence of the invariability of the element upon which it depends. Hence, in the theory of the variation of arbitrary constants, the secular inequality in the planet's motion

Being thus furnished with a strong indication that the irregularities of the two planets were due to their mutual attraction, he entered upon a searching inquiry into their real source. This he finally discovered in the near commensurability of the mean motions of the two planets. Five times the mean motion of Saturn is very nearly equal to twice the mean motion of Jupiter. In fact, if n, n' represent the mean motions of the two planets, $5n-2n'$ is equal only to about $\frac{1}{74}$th of the mean motion of Jupiter. Now, Laplace found that certain terms, involving this quantity in the differential equations of the longitude, would receive, by double integration, the square of the same quantity as divisors, and in consequence would rise very much in value. Terms of this class are, indeed, generally very minute, being only of the order of the cubes of the eccentricities and inclinations; but Laplace, with characteristic sagacity, suspected that the small divisors they acquired might render them sensible, and that they might possibly explain the irregularities of the two planets. The result of actual calculation entirely confirmed his suspicion. He found that the terms assigned to Saturn an inequality equal to 48′ 44″, and to Jupiter a contrary inequality equal to 20′ 49″. The periods of the two inequalities were equal, and amounted to 929 years. They reached their maximum values in the year 1560. The apparent mean motions of the two planets henceforth continually approximated towards their true mean motions, and finally coincided with them in the year 1790. This is the reason why Halley, on comparing ancient with modern observations, found the mean motion of Jupiter to be quicker, and that of Saturn slower, while Lambert, on the other hand, from a comparison of modern observations with each other, arrived at a diametrically opposite conclusion.

Laplace found that his equations accounted in a most satisfactory manner for the irregularities of the two planets. Among forty-three oppositions of Saturn which he compared with theory, the error in no case exceeded 2′, and generally it fell very far short of that quantity. At a sub-best tables of Saturn exceeded 20′*. By this capital discovery Laplace banished empiricism from the tables of Jupiter and Saturn, and extricated the Newtonian theory from one of its gravest perils. "The irregularities of the two planets," says that illustrious geometer, "appeared formerly to be inexplicable by the law of universal gravitation—they now form one of its most striking proofs. Such has been the fate of this brilliant discovery, that each difficulty which has arisen has become for it a new subject of triumph, a circumstance which is the surest characteristic of the true system of nature."†

We shall now give a brief account of the circumstances connected with a remarkable inequality in the moon's motion, which continued to form the subject of toilsome research until its true physical cause was at length discovered. From an extensive comparison of ancient with modern observations, it was established beyond doubt by the astronomers of the last century, that the mean motion of the moon has been becoming continually more rapid ever since the epoch of the earliest recorded observations. Halley was the first person who suspected this important fact. We may remark that, if the moon's mean motion be more rapid now than it was in ancient times, the place of that satellite, when computed for any remote epoch by means of the modern tables, will be less advanced than her actual place, and hence the time of an eclipse, when calculated in this manner, will appear to happen earlier than the recorded time. It is also obvious that, if we make a similar computation for any intermediate epoch, the moon in this case too will be thrown back in her orbit, though not to such an extent as in the previous case, and it is manifest that the error will diminish continually as we descend towards the epoch of the tables. Now this was the character of the results which Halley obtained from an examination of some ancient eclipses recorded by Ptolemy and the Arabian astronomers, and which in consequence induced him to suppose that the moon's mean motion was subject to a continual acceleration. He first alluded to this phenomenon in 1693, but no attempt was made to confirm his suspicion until the year 1749, when Dunthorne communicated a memoir to the Royal Society, in which he discussed all the observations calculated to throw light upon the subject. He computed by the modern tables an eclipse of the moon observed at Babylon in the year 721 A.C.; another, observed at Alexandria in the year 201 A.C.; a solar eclipse, observed by Theon in the year 364 A.D., and two similar phenomena, observed by Ibyn Jounis, at Cairo, in Egypt, towards the close of the tenth century. In all these cases the computed time of the phenomenon was earlier than the observed time; and the error generally was greater as the eclipse was more ancient. He therefore concluded that the several observations could only be reconciled with the tables by assuming that the mean motion was continually accelerated agreeably to the remark of Halley, and, from a comparison between the observed and computed times

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[*] Laplace first explained these inequalities in the volume of the Academy of Sciences for the year 1784. In the volumes for the two following years he gave a complete analysis of the theory of Jupiter and Saturn, and shewed its accordance with the ancient and modern observations. An admirable exposition of the origin of the famous inequality mentioned in the text is contained in Airy's Treatise on Gravitation; a little work which should be in the hands of every person (whether a mathematician or not) who desires to obtain clear ideas of the various modes in which the planets disturb each other by their mutual attraction.

of a number of eclipses, he was induced to fix the amount of the acceleration at 10″ in a century, counting from the year 1700.

A similar discussion conducted the celebrated astronomer Mayer to a secular acceleration of the mean motion. In his lunar tables, published in 1753, he fixed it at 7″ in a century; but in those published at London, in 1770, it was raised to 9″. Lalande also investigated the question in the year 1757, and deduced from his researches a secular equation of 9″.886, which he ultimately fixed at 10″.

Astronomers having thus demonstrated by incontestable evidence that the moon's mean motion was becoming continually more rapid, it henceforth became an interesting question to discover the physical cause of this phenomenon. The Academy of Sciences at Paris, always actuated by a zealous desire to promote the cause of science, offered their prize of 1770 for an investigation, which should have for its object to ascertain whether the theory of gravitation could render a satisfactory account of this secular inequality in the moon's motion. The prize was carried off by Euler; but that illustrious geometer was unable to discover any equations in the mean motion of a secular character. Towards the conclusion of his memoir, he uses the following remarkable words:—"There is not one of the equations about which any uncertainty prevails, and now it appears to be established by indisputable evidence, that the secular inequality in the moon's mean motion cannot be produced by the forces of gravitation."* The future history of this inequality should teach us to accept with the utmost caution the dictum of any authority, however high, when it tends to impugn the generality of a principle supported, as in the present instance, by a multitude of phenomena of the most unequivocal character.

Anxious to obtain a solution of this difficult question, the Academy of Sciences again proposed it for their prize of 1772. Euler and Lagrange were declared the successful competitors and shared the prize between them. Euler concluded his memoir by repeating the assertion he had made on the previous occasion, adding that no doubt henceforth could exist that the inequality arose from the resistance of an ethereal fluid pervading the celestial regions†. Lagrange, in his memoir, gave a new solution of the Problem of Three Bodies, which he applied to the moon ‡, but he reserved for a future occasion a rigorous inquiry into the cause of the acceleration. Meanwhile, some persons began to entertain a suspicion that the spheroidal figures of the earth and moon, by disturbing the law of their mutual attraction, might occasion the inequality. This induced the Academy again to propose their prize of 1774 for an investigation of the subject. Lagrange was declared the successful competitor. He examined the effects of the moon's figure upon her motion, by a very skilful analysis, but he could find no equation of a secular character. By a simple process of reasoning, he extended the same conclusion to the earth, and he assured himself with equal confidence that the attraction of the planets and satellites could not be the cause of the phenomenon. He then entered upon a critical discussion of the observations upon which the alleged acceleration of the mean motion was founded, and his final conclusion was, that in general the data were of a doubtful character, and that perhaps the best course would be to reject the inequality altogether §.

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* Prix de l'Académie, tome ix.

† Ibid., tome ix.

‡ Ibid., tome ix.

§ Mémoires des Savans Étrangers, tome vii.

Laplace, about the same time, investigated this interesting subject. Having carefully examined the ancient observations, he was induced to consider it as fully established, that the moon's mean motion was becoming more rapid in modern times. Some persons had endeavoured to explain the phenomenon by means of a continual retardation of the earth's diurnal motion. If this supposition were true, an acceleration ought to have manifested itself in the mean motions of the planets, as well as in that of the moon, but this was not borne out by observation. But, besides, no sufficient cause could be assigned why the rotatory motion of the earth should be continually retarded. It was indeed alleged, that this effect might be produced by the continual blowing of the easterly winds, generated by the heats of the torrid zone, against the great mountain chains which run from north to south in both hemispheres. Laplace, however, mentions that he examined this point with attention, and arrived at the conclusion that no retardation of the diurnal motion could possibly arise from such a cause. He considered another solution of the problem, founded on the supposition that the regions of space are occupied by an ethereal fluid, which continually resists the motions of the celestial bodies. He admits that such an hypothesis suffices to explain the phenomenon, but he contends that we have no independent proof of the existence of an ethereal fluid, and until we are assured beyond all possibility of doubt that the theory of gravitation cannot account for the moon's acceleration, we ought not to have recourse to any extraneous source of explanation *. His views on this subject are unquestionably more sagacious and philosophical than those of Euler or Lagrange

Unable to discover a secular inequality in the disturbing action of the sun, and yet reluctant to derive this result from any foreign principle, he was led to consider what effect might be produced by adopting a different conception of gravity. It had been always assumed that the effects of this principle were propagated instantaneously from bodies. Laplace, however, considered, that some time might be required for this purpose, and he readily perceived that such a supposition would have the effect of modifying the intensity of the force exerted on the moving body. He therefore computed what ought to be the velocity of gravity, in order that the gradual transmission of that principle should occasion the observed acceleration of the moon's mean motion, and he arrived at the remarkable conclusion that it must exceed the velocity of light eight millions of times. He remarked that if a satisfactory account of the origin of the phenomenon be adduced, without having recourse to this hypothesis, it would follow that the effects of the successive transmission of gravity would be insensible, and therefore the velocity must be at least fifty millions of times greater than the velocity of light!

No further progress was made in this question until the close of the year 1787, when Laplace finally announced that he had discovered the cause of the phenomenon in the gradual diminution of the mean action of the sun, arising in consequence of the secular variation of the eccentricity of the terrestrial orbit †. The mean action of the sun upon the moon tends to diminish the moon's gravity to the earth, and thereby causes a diminution of her angular velocity. This diminution being once supposed to occur, the angular velocity would afterwards remain constant, provided the mean solar action always retained the same value. This,

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* Mém. des Savans Étrangers, tome vii.

† Mém. Acad. des Sciences, 1786.

however, is not the case, for it depends to a certain extent on the eccentricity of the terrestrial orbit, an element which we know to be in a state of continual though inconceivably slow variation, from the action of the planets on the earth. This variation of the earth's eccentricity will, therefore, produce a corresponding variation in the mean action of the sun; and the earth, in consequence, having more or less power over the moon, will either quicken or retard her angular velocity, whence will ensue a secular inequality in the mean motion conformably to observation. Now, the eccentricity of the earth's orbit has been continually diminishing from the date of the earliest recorded observations down to the present time; hence the sun's mean action must also have been diminishing, and consequently the moon's mean motion must have been continually increasing. This acceleration will continue as long as the earth's orbit is approaching towards a circular form, but as soon as this process ceases, and the orbit again begins to open out, the sun's mean action will increase, and the acceleration of the moon's mean motion will be converted into a continual retardation *.

Laplace computed the acceleration, and found it to amount to $10''.1816213t^2$, t denoting the number of centuries before or after the year 1801. This result agrees as nearly as possible with that which astronomers have derived from a comparison of ancient with modern observations.

If the inequality were rigorously determinable by the preceding formula, it would obviously continue for ever to increase in the same direction, a conclusion which would be totally at variance with the explanation we have just given of its physical cause. The fact is, however, that the complete analytical expression of it is a periodic function of the time, and the quantity $10''.1816213t^2$ is merely the second term in the developement of it †, the others being so small as to admit of being rejected, when the computation does not extend to more than about 2000 years. Laplace, indeed, found that when the moon's place was calculated for the time of the Chaldean observations, it would be necessary to take into account the term depending on the cube of t. The inequality would then be expressed thus: $10''.1816213t^2 + 0''.01853844t^3$.

The variation of the earth's eccentricity, upon which the inequality in the moon's mean motion depends, cannot be calculated from theory without a knowledge of the masses of the planets. When it is considered what uncertainty prevails respecting the masses of Mars and Venus, it is surprising how close the agreement is between theory and observation. Fortunately, the planet which exercises by far the greatest influence on the eccentricity is Jupiter, whose mass is easily derived from the elongations of his satellites. It is remarkable that the action of the planets on the moon, when transmitted to her indirectly through the medium of the sun, should be more considerable than their direct action upon her.

The moon, in the present day, is about two hours later in coming to the

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* It does not *necessarily* follow that because an inequality is secular it should increase continually in the same direction. Lagrange found that the secular inequalities in the mean motions of Jupiter and Saturn were of a recurring character, although their duration extended to the immense period of 70414 years! The secular inequality in the moon's mean motion, being a more complicated phenomenon, has a much longer period than this.

† The first term, being proportional to the time, is absorbed in the mean motion, and therefore cannot form part of the inequality as *determined by observation*.

meridian than she would have been if she had retained the same mean motion as in the time of the earliest Chaldean observations. It is a wonderful fact in the history of science, that those rude notes of the priests of Babylon should escape the ruin of successive empires, and, finally, after the lapse of nearly three thousand years, should become subservient in establishing a phenomenon of so refined and complicated a character as the inequality we have just been considering.

Laplace also discovered that the lunar perigee and nodes were subject to secular inequalities from the same cause. He found that the inequality in the perigee was to the corresponding inequality in the mean motion as 33 to 10, and was subtractive from the mean longitude. He also discovered that the secular inequality of the nodes amounted to seven-tenths of that of the mean motion, and was additive to the mean longitude. Thus it appeared that, while the mean motion was continually accelerated, the perigee and nodes were continually retarded, the three inequalities being as the numbers 1, 3, .7 *. It hence also followed that the moon's motions, with respect to the sun, her perigee, and her nodes, continually increased in the ratios of 1, 4, 0.265. The existence and magnitude of these inequalities were confirmed in a most satisfactory manner by Bouvard, who for this purpose instituted an extensive comparison between the ancient and modern observations.

These great inequalities all depend on the secular variation of the earth's eccentricity. They will continually become more perceptible as ages roll on, but a vast number of years will elapse before they will have passed through all their values †. They will one day affect the secular motion of the moon to the extent of at least the fortieth part of the circumference, and that of the perigee to the extent of the thirteenth part ‡.

It might be imagined that the secular variation in the position of the ecliptic would have the effect of modifying the sun's action on the moon, and would in consequence disturb the mean inclination of the lunar orbit. Laplace, however, found that the moon was constantly maintained by the sun at the same inclination towards the moveable plane of the ecliptic, so that her declinations were subject to the same secular changes as those of the sun, and were due solely to the continued diminution of the obliquity of the ecliptic.

It was not until Laplace announced his discovery of the cause of the moon's acceleration, that Lagrange became aware of the oversight he had committed, while engaged in similar researches in 1783, by neglecting to apply his analysis to the moon. He now made the required substitutions, and, computing the numerical value of the inequality, he obtained a result which almost coincided with that of Laplace §.

We shall conclude this historical notice of the secular inequalities of

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* Since the mean motion of the lunar perigee is *direct,* the effect of an inequality, which is subtractive from the mean longitude, will manifestly be to retard the perigee behind its true place. On the other hand, since the motion of the nodes is *retrograde,* a retardation can only take place when the inequality is additive to the mean longitude.

† Leverrier has found that the eccentricity of the terrestrial orbit will continue to diminish during the period of 23,980 years. It will then attain a minimum value equal to 0.003314. Mémoire sur les variations séculaires des élémens des orbites pour les sept planètes principales. See also Connaissance des Temps, 1843.

‡ Exposition du Système du Monde, tome ii., liv. iv. chap. v.

§ Mém. Acad. Berlin, 1792-3.

the moon, with a brief account of two other remarkable results, which Laplace derived from his researches in the lunar theory. The spheroidal figure of the earth occasions a sensible perturbation of the moon's motion both in longitude and latitude. The inequality in longitude was discovered by Mayer, who was ignorant of the physical cause of it, but represented it in his tables by an empiric equation. Laplace derived the equation from theory, and found it to depend on the longitude of the moon's node. Burg, by a comparison of numerous observations, was led to estimate the greatest value of the coefficient at 6″.7. This result gives $\frac{1}{305.05}$ for the earth's ellipticity. The inequality in latitude was discovered by Laplace to vary with the sine of the moon's true longitude. Its value was derived by Burg and Burckhardt, from the combined observations of Bradley and Maskelyne, and was fixed by them at — 8″, a quantity which implies an ellipticity equal to $\frac{1}{304.5}$.

The agreement between the results derivable from these two distinct equations is very interesting. If the earth were homogeneous, it is demonstrable that the ellipticity would be equal to $\frac{1}{230}$; it follows, then, that the density must increase towards the centre—a fact which we know to be true from other sources.

A comparison of arcs of the meridian, measured in different parts of the world, presents a series of anomalous results, which lead us to conclude that the figure of the earth is not that of an exact spheroid. It is remarkable, however, that when two arcs are compared, the distance between which is so great as to obviate the effects of any minute inequalities in the spheroidal figure, they indicate an ellipticity almost equal to that derived from the lunar inequalities. Thus, the result of a comparison between a meridional arc at the equator and one measured in France, gives $\frac{1}{308}$ for the earth's ellipticity.

Another striking result which Laplace derived from his researches was the value of the solar parallax. Among the equations in longitude, he found one involving that element, and varying with the angular distance between the sun and moon. The coefficient of this equation, when compared with observation, was found to give 8″.6 for the mean value of the solar parallax. This result agrees with the mean of those obtained by astronomers from observations on the transit of Venus in 1769. "It is very remarkable," says Laplace, "that an astronomer, without leaving his observatory, by merely comparing his observations with analysis, has been enabled to determine with accuracy the magnitude and figure of the earth, and its distance from the sun and moon, elements, the knowledge of which has been the fruit of long and troublesome voyages in both hemispheres." *

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* Exposition du Système du Monde, tome ii. p. 91.

CHAPTER VI.

Theory of the Figure of the Earth.—Newton.—Huygens.—Maclaurin.—Clairaut.—Attraction of Spheroids.—D'Alembert.—Legendre.—Theory of Laplace.—Motion of the Earth about its Centre of Gravity.—Nutation.—Bradley.—Investigation of Precession and Nutation, by D'Alembert.—The Tides.—Equilibrium Theory.—Researches of Laplace.—Stability of the Ocean.—Libration of the Moon.—Galileo.—Hevelius.—Newton.—Cassini.—Newton's Explanation of the Moon's Physical Libration.—Researches of Lagrange.—Combination of the Principle of virtual Velocities with D'Alembert's Principle.—Laplace investigates the Effect of the secular Inequalities of the mean Motion upon the Libration in Longitude.—His Theory of Saturn's Rings.

THE Figure of the Earth was the first of the subjects treated of in the Principia, which engaged the attention of geometers. In 1690 Huygens published his treatise "De Causa Gravitatis," in which he investigated the ratio of the earth's axis in accordance with his own views of gravity. Assuming the density to be homogeneous, he imagined, like Newton, two fluid columns, reaching from the centre of the earth to the surface; one in the plane of the equator and the other along the polar axis. The particles of the equatorial column were acted upon by gravity and by the centrifugal force arising from their rotation; those of the polar column were acted upon by gravity alone. The equatorial particles being, therefore, severally lighter than the polar, and the two columns being also in equilibrium, it was necessary that the equatorial column should compensate, by its superior length, for the diminished pressure of its particles. Huygens assumed that gravity urged the particles to the centre of the earth with a force varying according to the inverse square of the distance. This supposition was inconsistent with the theory of gravitation, for Newton had found that, in consequence of the attraction of the surrounding particles, the tendency of each particle to the centre would vary in the direct ratio of the distance. We have already remarked, however, that Huygens rejected the mutual attraction of the particles of matter, and admitted only their gravity towards a central point. Having computed the lengths of the two columns on the supposition that they were in equilibrium, he found that the equatorial column would exceed the polar, assumed equal to unity, by half the ratio of the equatorial centrifugal force, to the equatorial gravity, or by $\frac{1}{2} \times \frac{1}{289} = \frac{1}{578}$. Hence the ratio of the two axes would be as 579 to 578. He also found that the increase of gravity at the surface, from the equator to the pole, would vary in the proportion of the square of the sine of the latitude, and that the total increase, supposing the equatorial gravity equal to unity, would be equal to twice the ratio of the equatorial centrifugal force to the equatorial gravity, or $2 \times \frac{1}{289}$. Thus the fraction expressing the ellipticity* was to that expressing the total increase of gravity as $\frac{1}{2}$ to 2. Newton's theory, on the other hand, gave the same fraction in the one case as in the other; both being measured by $\frac{5}{4}$ths the ratio of the equatorial centrifugal force to the equatorial gravity. It is remarkable, however, that the sum of the fractions is the same in both theories for the same values of the last-mentioned ratio. Thus, in Newton's theory, the two fractions being both equal to $\frac{5}{4} \times \frac{1}{289}$, their sum is equal to $\frac{5}{2} \times \frac{1}{289}$; in Huygens' theory, the same sum is equal to $\frac{1}{2} \times \frac{1}{289} + 2 \times \frac{1}{289} = \frac{5}{2} \times \frac{1}{289}$. These are only particular cases of a general theorem discovered by Clairaut, connecting the ellipticity of spheroids with the variation of gravity at their surfaces. This theorem, indeed, supposes the mutual gravitation of the particles of matter, which Huygens refused to admit; but the investigation of that philosopher may be considered as founded on the same principle, by imagining the spheroid to be composed of strata of different densities; the exterior stratum being infinitely rare, and the density thence increasing to the centre, where it is infinite.

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* The ratio of the excess of the equatorial over the polar axis to the latter axis is termed the ellipticity of the spheroid. Hence, if the polar axis be assumed equal to unity, the ellipticity will be represented simply by the excess of the equatorial axis over it.

The theories of Newton and Huygens involve the two extreme cases of density, and therefore assign the limits of ellipticity for a heterogeneous spheroid revolving round a fixed axis. Hence, since there is strong reason to believe that the density of the earth increases towards the centre, it might naturally be expected that the ellipticity would be comprised between these limits. This conclusion has been verified in the most satisfactory manner by the researches of astronomers, who have found that the ellipticity, whether as determined by the measurement of meridional arcs, by experiments with the pendulum, or by observations on the motion of the moon, lies between $\frac{1}{230}$ and $\frac{1}{578}$, the values assigned by the two extreme cases of the problem.

Neither Newton nor Huygens demonstrated *à priori* that the earth might possibly assume the form of an oblate spheroid. This important step was reserved for Maclaurin *. In his prize memoir on the Tides, which appeared in 1740, this distinguished mathematician proved, by a beautiful application of the ancient geometry, that an oblate spheroid would satisfy the conditions of equilibrium of a homogeneous fluid mass, differing little from a sphere, and endued with a rotatory motion round a fixed axis. He also demonstrated that the increase of gravity from the equator to the pole would vary as the square of the sine of the latitude, and that the ratio of the total increase to the gravity at the equator would be expressed by the fraction representing the ellipticity, or, in other words, by $\frac{5}{4}$ths the ratio of the equatorial centrifugal force to the equatorial gravity. These results confirmed the assumptions of Newton; but, as they were founded on the supposition of a homogeneous fluid, they were not applicable to the earth, which evidently increased in density towards the centre. They formed, however, an important advance towards a more correct theory of the earth's figure, and on this account deserve to be considered as a valuable contribution to Physical Astronomy. The investigations by means of which Maclaurin arrived at these results have been universally admired for their ingenuity and elegance, and are justly considered as rivalling, in these respects, the most finished models of the ancient geometry.

In 1743 Clairaut published his valuable treatise on the Figure of the Earth. In this work the general equations of the equilibrium of fluids, independently of any hypothesis with respect to the density or the law of the attraction, are for the first time given. By means of these equations Clairaut investigated the figure of the earth on the supposition of the density being heterogeneous; and he found, that in this case also an elliptic spheroid would satisfy the conditions of equilibrium, provided the mass was disposed in concentric strata of similar forms and homogeneous density. The ellipticities of the successive strata will obviously depend on the law of the density, and the other conditions of the problem; but Clairaut discovered that the following theorem is generally true:—*the sum of the fractions expressing the ellipticity and the increase of gravity at the pole is equal to two and a half times the fraction expressing the centrifugal force at the equator*[*]. This theorem, combined with that relating to the variation of gravity at the surface, enables us to determine the ellipticity of the earth, by means of observations on the force of gravity, in two different latitudes. Its peculiar value consists in being independent of any hypothesis with respect to the internal constitution of the earth. We have seen that the results obtained by Newton and Huygens offer particular illustrations of this important theorem, which is generally designated by the name of its inventor. Little real progress has been made in the theory of the Figure of the Earth beyond the results to which Clairaut was conducted by his admirable researches on this occasion.

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* Born in 1698, at Kilmoddan, in Argyllshire; died at Edinburgh, in 1746.

The actual ellipticity of the earth may be determined by three distinct methods. The simplest of these in principle depends on the measurement of two arcs of the meridian lying in different latitudes. The other two methods are derived from the theory of gravitation. One of these is suggested by Clairaut's theorem, and requires a knowledge of the force of gravity in two different latitudes. These data may be found by means of experiments with the pendulum. The other method, assigned by theory, depends on the effect of the earth's ellipticity in disturbing the moon's motion. It may not be uninteresting to compare the results obtained by these three methods; and for this purpose we shall select the examples given by Mr. Airy in his treatise on the Figure of the Earth[†].

With respect to the first method, Lambton measured an arc of the meridian in India, comprised between lat. 8° 9′ 38″ .4, and lat. 10° 59′ 48″ .9, and found its length to be 1029100.5 feet. Swanberg, on the other hand, measured a similar arc in Sweden from lat. 65° 31′ 32″ .2, to lat. 67° 8′ 49″ .8, and found its length 593277.5 feet. These measures assign $\frac{1}{306.3}$ as the earth's ellipticity.

Again, at Madras, in latitude 13° 4′ 9″, the length of the seconds pendulum has been found to be equal to 39.0234 inches; at Melville Island, in latitude 74° 47′ 12″, the corresponding length is 39.2070 inches. These results give $\frac{1}{300}$ for the ellipticity.

Lastly, the coefficient of the inequality in the moon's latitude, depending on the spheroidal figure of the earth, is found by observation to be equal to $-8''$. This result indicates an ellipticity equal to $\frac{1}{297}$[‡].

The near agreement of these values of the ellipticity, determined by methods so very dissimilar, constitutes a powerful argument in favour of the theory of gravitation.

It is obvious that the question relative to the figure of the earth, and the variation of gravity at the surface, is intimately connected with the theory of the attraction of spheroids. In 1773 Lagrange demonstrated by analysis the results to which Maclaurin was conducted by his researches on this subject, and extended them to the general form of the ellipsoid. Maclaurin had limited his investigation to the attraction of particles either contiguous to the surface of the spheroid, or situated in its interior. It was desirable, however, to complete the theory of the subject, by determining the attraction of a point situated anywhere without the spheroid. D'Alembert first gave a theorem, by means of which the attraction in this case might be found, when the particle was situated in the prolongation of one of the axes, and Legendre afterwards discovered a similar theorem applicable to an exterior point, situated anywhere whatever when the attracting body was an ellipsoid of revolution. The problem for the general case of the ellipsoid presented analytical difficulties, which continued for some time to elude the researches of the most profound analysts. In 1784 Laplace finally succeeded in effecting its solution, but his method was embarrassed with series, and did not by any means possess the elegance and perfection which distinguished the other parts of the theory.

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[*] In this enunciation of Clairaut's theorem, the unit of force is represented by the equatorial gravity.

[†] Mathematical Tracts on the Lunar and Planetary Perturbations.

[‡] This result does not exactly coincide with Laplace's, on account of a slight difference in the data.

In 1782 Laplace explained a general theory of the attraction of ellipsoids. His researches were based wholly upon a partial differential equation of the second order of a very remarkable character, which has been subsequently employed with great success in many important investigations connected with the Physico-mathematical sciences. By simple differentiation, he determined the figure assumed by a heterogeneous mass of fluid, differing only in a small degree from a sphere, and by a similar process he also obtained the law of attraction at the external stratum. His results coincided with those to which Clairaut had been already conducted by a less direct analysis. The investigation of this great geometer is indeed more remarkable, for the method by which he derives the theorems of his predecessors, than for any new light he throws on the difficult subject to which it relates. The calculus he employs in it is described by one of the most eminent mathematicians of the present age, as the most singular in its character, and the most powerful in its application, which has ever been devised *.

The motion of the earth about its centre of gravity was one of those great problems of the system of the world, which demanded for its solution the most advanced principles of mechanical science. Newton's researches on this subject have been admired as one of the most remarkable triumphs of his genius, but a more complete and systematic investigation was rendered desirable by the improved state of analytical mechanics. Further researches were also called for by Bradley's discovery of Nutation. It did not escape the sagacity of Newton, that besides the motion which occasions the Precession of the Equinoxes, the earth's axis would be affected by an oscillatory motion, arising from the variable position of the plane of the equator with respect to the direction of the sun's disturbing force †. In fact, if we suppose the earth to be situated in the vernal equinox, the sun's disturbing force will pass through the plane of the ring formed by the redundant matter at the equator, and, therefore, it can produce no effect on the position of that plane. As the earth proceeds in her course, the sun's force becoming inclined to the ring, will tend to disturb its position, and this disturbance will continually increase to the solstice, where the inclination reaches its maximum. From this point the tendency of the sun to disturb the ring continually

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* Airy, Encycl. Metrop. Art. Figure of the Earth.

† Principia, book i. prop. 66, cor. 20.

diminishes, until it finally vanishes once more upon the earth's arrival in the autumnal equinox, when the disturbing force passes through the plane of the ring. The same succession of changes will manifestly take place from the autumnal to the vernal equinox. This constant variation of the influence of the sun's disturbing force upon the position of the equator will give rise to an oscillatory movement of the latter plane, which will pass through its values in the course of half a year, and it will be accompanied by a corresponding nutation of the earth's axis with respect to the plane of the ecliptic. Newton announced the period of this inequality, but did not compute its value, deeming it too inconsiderable to be detected by observation *. In fact, it does not amount to half a second at its maximum. A similar nutation arises from the action of the moon on the terrestrial spheroid. It completes its period in half a month, but, like the solar nutation, it is quite insensible.

But, if the plane in which either of the disturbing bodies moves should vary in position with respect to the plane of the equator, it is clear that a nutation of the earth's axis will arise, independent of that which we have just been considering. In this case the effect of the disturbing force will increase or diminish with the increase or diminution of the inclination between the two planes, and it will give rise to an inequality, the period of which will be equal to that comprised between the least and greatest angles of inclination. Since the plane of the ecliptic constantly preserves the same inclination with respect to the equator, at least, if we neglect its secular displacement, no nutation of this kind can arise from the action of the sun. But the case is quite different when the disturbing force of the moon is considered. The lunar orbit is inclined to the ecliptic at an invariable angle, but, as its nodes have a retrograde motion upon that plane, its inclination to the equator will continually vary. The inclination of the lunar orbit to the ecliptic is about 5° 8′, and the nodes perform a complete revolution in somewhat more than 18 years. The inclination to the equator will therefore pass from its maximum to its minimum value in about 9 years, varying to the extent of 10° 16′, and in the succeeding 9 years it will return to its original state. Hence arises an inequality in the motion of the earth's axis, which completes its period in a little more than 18 years. This inequality had escaped the notice of geometers, until Bradley detected it by observation. That great astronomer discovered irregularities in the places of the stars, which could not be reconciled either with the phenomenon of aberration, or with the annual motion of precession. Having prosecuted his observations during a number of years, he found that the irregularity of each star passed through all its values in course of a complete revolution of the moon's nodes. Thus, for example, he found that, during the nine years comprised between 1727 and 1736, the star γ Draconis moved 10″ to the north, while during the nine following years it continued to move southwards, until it finally arrived in its original position. He explained these irregularities by an oscillatory movement of the earth's axis, the period of which extended to a revolution of the moon's nodes. In virtue of this nutation, the earth's axis alternately approaches to, and recedes from, the plane of the ecliptic, and the equinoctial points alternately advance and recede upon the same plane. Both of these effects may be accounted for by imagining the pole of the equator to describe a small

* Principia, book iii. prop. 21.

ellipse in the heavens round its mean place, the major and minor axes of the ellipse being 18″ and 13″, and the former of these coinciding with the circle of latitude passing through the mean pole of the equator.

Bradley, besides determining the period and maximum value of Nutation, had the sagacity also to discover its true physical cause. It now remained for geometers to compute the inequality by theory. In 1749 D'Alembert published his important work on the Precession of the Equinoxes, which contained a rigorous investigation of the motion of the earth about its centre of gravity. The quantities of precession and nutation, when computed by theory, were found to accord in the most satisfactory manner with observation. Solutions of the same great problem, differing more or less from each other, were soon afterwards given by Euler, Frisi, Thomas Simpson, and several other geometers. Laplace at a later period examined the effect which might be produced on the motion of the earth's axis by the fluid state of the ocean, and he was conducted to the following remarkable conclusion:—*the motion of the earth's axis is the same as if the whole sea formed a solid mass adhering to its surface.*

About the time that geometers resumed the consideration of the figure of the earth, their attention was also directed to the theory of the Tides. The Academy of Sciences of Paris having proposed it as the subject of their prize of 1740, four individuals were considered to possess just claims to distinction, and the prize was shared among them. Three of these, Euler, Maclaurin, and Daniel Bernouilli, adopted the principle of gravitation as the basis of their respective investigations; the fourth, Father Cavalleri, endeavoured to explain the phenomena by the system of vortices. This was the last honour paid to the Cartesian theory, which soon afterwards sank into total oblivion. The three geometers first mentioned supposed that the action of the sun or moon upon the ocean drew the earth every instant into the form of an aqueous spheroid, which would be maintained in equilibrium if the forces continued to operate with the same intensity and in the same direction. This was termed the equilibrium theory, and it is manifest from its fundamental principle that the researches suggested by it essentially coincided with those relating to the figure of the earth. In reality, however, the continual change in the positions of the sun and moon with respect to the earth does not allow the waters of the ocean to attain a state of equilibrium, and it is by the mutual blending of the oscillations hence arising that the different phenomena of the tides are occasioned The question is therefore one of dynamics, and not of statics. Laplace first considered the subject in its proper light, its investigation having been recently very much facilitated by the researches of D'Alembert on the motion of fluids, and by his invention of the calculus of partial differences. The theory of Laplace, although generally allowed to be a signal effort of mathematical genius, is based upon two suppositions, which cannot be reconciled with the real condition of the earth. These are—1st, that the whole exterior stratum of the earth is covered with an aqueous fluid; 2nd, that the depth of the ocean is uniform under the same parallel of latitude. Discovering the impossibility of adapting his results to the actual state of the Tides, on account of the influence of a multitude of circumstances which could not be ascertained with precision, and even if they were so ascertained could not be introduced into his theory, he was compelled to assume as a general principle that the oscillations of the waters of the ocean are periodical, like the forces which produce them, but that they are not necessarily proportional to the magnitudes of those forces, nor are the times of their maxima and minima necessarily coincident with the times of the maxima and minima of the forces. In other words, he assumed that, if the disturbing forces of the sun or moon be expressed by a series of cosines of angles, the oscillations of the sea will be expressed by a corresponding series of cosines, the arguments being the same in both cases, but the epochs and coefficients being different. This assumption has been justly regarded as tantamount to an evasion of the difficulties of the problem, rather than a real conquest of them. Indeed, it is now generally admitted, that a long course of observations, conducted with great skill, and under a variety of different circumstances, can alone lead to a theory of this subject which shall be of any service in the construction of accurate Tide tables.

An interesting question which Laplace considered in connexion with that of the Tides was the stability of the Ocean. It is important to know whether the condition of the ocean is such that any disturbance which it might suffer would produce only temporary oscillations, in course of which the waters would gradually relapse into their former position, or whether the condition is so unstable that the communication of a small quantity of motion would cause the waters to leave their ordinary bed and overwhelm the whole surface of the earth. Laplace found that the equilibrium would be stable provided the density of the sea was less than the mean density of the earth. Now, the various experiments made on the attraction of mountains, and those executed for a similar purpose with the torsion balance of Cavendish, all concur in showing that the mean density of the earth is about five times the density of the sea. We are therefore assured that, under the present constitution of the material universe, the face of the earth will not be liable to any overwhelming inundation of the waters of the ocean, a conclusion which beautifully illustrates the language of Scripture—"hitherto shalt thou come, and no further."*

Another important subject which soon afterwards engaged the attention of geometers was the libration of the moon. It had been remarked, from the most ancient times, that the moon turns the same side towards the earth throughout the entire course of every revolution. A curious consequence of this fact is, that the motion of that body round the earth is equal to her motion round her axis. It is singular, however, that this conclusion does not appear to have suggested itself to astronomers until the revival of science in modern times. Galileo first discovered that the appearance of the moon's surface is subject to a slight variation, depending on her altitude above the horizon. This fact he rightly explained by the variable position of a spectator at the earth's surface on account of the diurnal motion. In fact, unless the moon be in the zenith, a spectator always views her in a direction different more or less from that in which he would view her if he were placed at the centre of the earth. This phenomenon, in consequence of passing through all its phases in twenty-four hours, has been designated the diurnal libration of the moon. Hevelius discovered a similar libration in longitude, which he ascribed to the displacement of the centre of the orbit from the centre of motion; but Newton first gave a clear explanation of this phenomenon in a letter addressed to Mercator, which appeared in a work on astronomy, published by that individual in the year 1676. He showed that it arose from the inequalities of the moon in longitude causing the angle described by her round the earth sometimes to exceed in magnitude the angle described by her round her axis, and other times to fall short of the same angle; whence it happened, that the great circle formed by the intersection of the lunar surface with a plane passing through the moon's centre, and perpendicular to the line joining the earth and moon (which great circle determines the lunar hemisphere visible to the spectator), does not maintain a fixed position, but oscillates continually round a mean state. Hence the moon will appear to librate continually to and fro in the plane of her motion. In the same letter Newton explained the libration in latitude, which arises in consequence of the moon's axis of rotation being inclined to her orbit. This phenomenon, like the two already mentioned, is purely optical.

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* Job xxxviii. 2.

Cassini is the next person whose researches contributed to throw light upon this interesting subject. This astronomer discovered the remarkable fact, that the nodes of the moon's equator always coincide with the nodes of her orbit. He also found that a plane drawn through the moon's centre, parallel to the plane of the ecliptic, is always contained between the planes of her equator and orbit, so that the poles of the latter are constantly situated in the same great circle with the pole of the ecliptic, but on opposite sides of it. He fixed the inclination of the lunar equator to the ecliptic at 2° 30′.

Mayer, about the middle of the last century, undertook an extensive series of observations for the purpose of verifying the conclusions of Cassini. He measured the distance between the nodes of the equator and orbit, and found it to amount to 3° 30′. He remarked, however, that the displacement might be considered as absolutely insensible, since its determination depended on the inclination of the lunar equator, an error in which of only 5′ would produce a corresponding error of 20° or 25° in the distance between the nodes. He fixed the inclination of the lunar equator at 1° 45′, a quantity considerably less than the corresponding estimate of Cassini. Lalande, a short time afterwards, made observations on the moon, and arrived at conclusions which mainly agreed with those of Mayer, but he obtained 2° 9′ for the inclination of the lunar equator. Recent observations have, however, completely confirmed the value assigned to that element by the illustrious astronomer of Göttingen.

The librations we have hitherto mentioned are apparent, not real; for they do not depend upon any actual inequality in the motion of the moon round her axis. Newton, however, did not fail to perceive that the action of the earth would, under certain conditions, affect the figure of the moon, and would thereby occasion a real variation of her rotatory motion. Proceeding upon the supposition that she was originally in a fluid state, he concluded that the terrestrial attraction would draw her into the form of a spheroid, the longer axis of which, when produced, would pass through the earth's centre. Comparing this phenomenon with the tidal spheroid, occasioned by the action of the moon upon the earth, he found that the diameter of the lunar spheroid, which is directed towards the earth, would exceed the diameter at right angles to it by 186 feet. He discovered in this elongation of the moon the cause why she always turns the same side towards the earth, for he remarked that in any other position the action of the earth would not maintain her in equilibrium, but would constantly draw her back, until the elongated axis coincided in direction with the line joining the earth and moon. Now, in consequence of the inequalities of the moon in longitude, the elongated axis will not always be directed exactly to the earth. Newton therefore concluded that a *real* libration of the moon would ensue, in virtue of which the elongated axis would oscillate perpetually on each side of its mean place.

D'Alembert was the first of Newton's successors who undertook to investigate the subject of the moon's physical libration. In 1754 this great geometer, encouraged by his researches on the Precession of the Equinoxes, applied the same method of investigation to the problem of the moon's motion about her centre of gravity. He did not, however, pay sufficient attention to the modification rendered necessary by the slow rotatory motion of the moon, and the near commensurability of the motions of revolution and rotation. For these reasons the results obtained by him did not accord so well with observation as those to which he was conducted by his previous researches of a similar kind relative to the motion of the earth. The Academy of Sciences of Paris having offered their prize of 1764 for a complete theory of the moon's libration, Lagrange composed an admirable memoir on the subject, which obtained for him the prize. It was in this investigation that he first employed the principle of virtual velocities in combination with the dynamical principle recently discovered by D'Alembert. By this step he reduced every question relating to the motion of a system of bodies to the integration of a series of differential equations of the second order, whence the only difficulties that remained to be overcome were those of a purely analytical nature. This refined conception forms the basis of his celebrated work, the *Mécanique Analytique*, which he published at a subsequent period of his life, and in which all the great problems of mechanical science are investigated by a process divested of every trace of geometrical reasoning.

Lagrange, in the memoir above mentioned, proceeded first to consider the figure which the moon would assume in consequence of the various forces exerted upon the particles composing her mass, which he supposed, with Newton, to have been originally in a fluid state. It does not appear to have occurred to the latter, that the centrifugal force generated by the rotatory motion of the moon would affect her figure to an extent comparable with the effect occasioned by the action of the earth. Lagrange, however, found that both effects were of the same order, and that the moon would in reality acquire the form of an ellipsoid, the greatest axis being directed towards the earth, and the least being perpendicular to the plane of the equator. The greatest axis, and the mean axis, will both lie in the last-mentioned plane; the mean position of which, as we have already stated, is parallel to the plane of the ecliptic. Lagrange also found that the excess of the axis turned towards the earth over the least axis was four times greater than the excess of the axis at right angles to it over the same axis.

Considering next the effect of the earth's attraction upon the rotatory motion of the moon, Lagrange found that the mean motion would be affected by a series of inequalities corresponding to those of the mean motion in longitude. The velocity of rotation is on this account sometimes accelerated beyond its mean state, and at other times retarded behind it, whence there ensues a real libration similar to that remarked by Newton. Lagrange shewed that it was not necessary to suppose that at the origin the motions of rotation and revolution were exactly equal. If they differed by an arbitrary quantity confined within certain narrow limits, the effect of this difference would be merely to occasion a slight inequality, in the motion of rotation, in virtue of which the axis directed towards the earth would librate continually on each side of the line joining the earth and moon. The most careful observations of the moon's surface have not disclosed to astronomers any traces of a libratory motion of this character; whence we may conclude that the motions of rotation and revolution did not originally differ by a sensible quantity*.

Lagrange next considered the libration of the moon in latitude. In this memoir he did not succeed in explaining the singular fact discovered by Cassini relative to the coincidence of the nodes of the lunar orbit and equator. When he assumed this coincidence in the outset of his researches, he found that the lunar equator, instead of being fixed with respect to the ecliptic, continually approached towards that plane, while observation on the other hand went to prove that it was inclined to it at nearly an invariable angle. This portion of his researches being imperfect, he resumed the subject fifteen years afterwards, and in the volume of the Berlin Academy for 1780, he published an admirable memoir, in which he completed the theory of the moon's motion about her centre of gravity. On this occasion he was conducted to the remarkable conclusion, that if the mean nodes of the lunar equator and orbit be supposed to have originally coincided, the action of the earth upon the lunar ellipsoid would constantly maintain this coincidence. He also determined the laws of the small oscillations, by which the true node of the lunar equator deviates from the mean node.

The only question connected with this subject which still remained to be examined was the effect which the secular inequalities in the mean motion of the moon might produce upon the appearance of the lunar surface. These inequalities will one day derange the mean place of the moon to the extent of several circumferences of the circle, and if the rotatory motion of the moon remained constant during the whole period of their developement, the inevitable consequence would be, that the moon would present the whole of her surface in gradual succession towards the earth. Laplace investigated this interesting question, and arrived at the conclusion that such a condition was inconsistent with the theory of gravitation. He found, in fact, that the terrestrial attraction would always draw the moon's axis into coincidence with the line joining the earth and moon, so that the rotatory motion will participate in the secular acceleration of the motion in longitude, and consequently the lunar hemisphere, which is turned away from us, will remain for ever concealed from view, with the exception of the small portion disclosed by the periodic inequalities.

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* Several writers on astronomy, when describing the various librations of the moon, affirm that the fourth, or physical libration, was discovered by Lagrange. If this refers to the libratory motion mentioned in the text, it cannot be called a discovery, since its actual existence has not yet been established by astronomers. The only *real* libration which observation has detected is that depending on the lunar inequalities in longitude (chiefly the annual equation; see Chapter XI.), and this phenomenon was first remarked as a theoretical truth by the great founder of Physical Astronomy, who unfolded the whole mechanism of the planetary system, and by his unrivalled sagacity anticipated those results which his successors, by the aid of a refined analysis, have been enabled only to confirm and extend. Laplace is surprised that Newton should have failed to notice that, in order to assure the constant equality of the motions of rotation and revolution, it was not absolutely necessary that at the origin they should have been exactly equal. This, however, might be considered as a natural corollary to the remark of Newton, that any disturbance of the elongated axis of the moon would merely result in an oscillatory motion on each side of its mean place; for the possibility of allowing the arbitrary constants of any system to vary a little on each side of a mean state, without occasioning any permanent derangement of the system, is a manifest attribute of the condition of stable equilibrium, and such a condition is clearly implied in Newton's words:—"Unde ad hunc situm semper oscillando redibit."—Princip., lib. iii. prop. xxxviii. If the motions of rotation and revolution had differed a little at the origin, as Laplace conceived they might, it is clear that the elongated axis would not have coincided exactly with the line joining the earth and moon; and hence, according to Newton's statement, it would oscillate continually on each side of that line. Newton, however, evidently refers to the difference in the two motions occasioned by the inequalities in the moon's longitude. It is natural enough, indeed, to suppose that the illustrious author of the Principia did not feel any anxiety to repudiate the original equality of the motions of rotation and revolution—a relation which, although perhaps difficult to explain by the doctrine of chances, becomes very interesting and suggestive when it is considered as the result of Supreme Intel-

Laplace has considered the circumstances which determine the stability of the singular mechanism with which the planet Saturn is furnished. He considers that the rotatory motion of the rings may be accounted for by supposing the particles composing them to be homogeneous, and to move freely among each other like the particles of a fluid. Under such conditions, he shews that they would be maintained in equilibrium by the action of the planet and the centrifugal force generated by their own rotatory motion, the exterior surfaces assumed by both rings being such, that all sections perpendicular to them, and passing through the centre of the planet, would be ellipses, whose major axes when produced would pass through that point. Laplace hence concluded that the period of the rotation of the rings is equal to that of a satellite revolving at the distance of the centre of the generating ellipse. This period he found to be equal to 10h. 33′.36″. It is remarkable that Herschel inferred, from certain periodic changes in the appearance of the rings, that they accomplished a revolution round the planet in 10h. 32′.15″*.

If the rings were uniform and circular, and were not exposed to the action of any extraneous force, it would still be possible for them to revolve constantly round the planet; but it is clear that the least disturbance, as the action of a satellite or comet, would affect their stability, and ultimately precipitate them upon the body of the planet. In order, therefore, to assure the permanence of the rings, Laplace conceived that it was necessary to suppose their figures to be irregular, so that any disturbance either of them might suffer would be rapidly checked in course of rotation by the unequal distribution of the mass.

CHAPTER VII.

Jupiter's Satellites.—Galileo.—Simon Marius.—Hodierna.—Borelli.—Cassini.—His first Tables.—He is invited to France.—He publishes his Second Tables.—His Rejection of the Equation of Light.—Researches of Maraldi I.—He discovers that the Inclination of the second Satellite is variable.—Bradley's Discoveries.—Maraldi II.—His Discoveries relative to the third and fourth Satellites.—He adopts the Equation of Light.—Wargentin.—He discovers the Inequalities in Longitude of the first and second Satellites.—He remarks that the third Satellite has two Equations of the Centre.—Motion of the Nodes of the fourth Satellite.—Inclination of the third Satellite.—Libratory Motion of the Nodes.—Inclination of the fourth Satellite.

THE discovery of Jupiter's satellites is one of the most interesting events in the history of astronomy. Even in any age it would have been deemed an important contribution to science; but in the beginning of the seventeenth century, when men's minds were wavering between the ancient and modern ideas of the system of the world, it exercised an influence of which it is impossible to form an adequate conception in the present day. The existence of four bodies revolving round one of the principal planets of the solar system, exhibited a beautiful illustration of the moon's motion round the earth, and furnished an argument of overwhelming force in favour of the Copernican theory. The announcement of this fact pointed out also the long vista of similar discoveries which have continued from time to time down to the present day to enrich the solar system, and to shed a lustre on the science of astronomy. In more recent times the physical theory of Jupiter and his attendants has supplied evidence of the most varied and satisfactory character in favour of the principle of Universal Gravitation. All the irregularities which arise from the mutual action of the larger bodies of the system are here exhibited in miniature. Their study also offers peculiar advantages to the mathematician, for, as they generally pass through all their values in short periods, their real character is readily appreciable, and on this account they are eminently favourable for testing the conclusions of his theory. Nor is it merely in its relation to speculative science that the discovery of Jupiter's satellites is to be regarded as of capital importance. The eclipses of these bodies soon suggested a new solution of the great problem of the longitude. Their theory thus came to be associated with one of those questions which most deeply affect the progress of civilization—the promotion of mutual intercourse between the various nations of mankind,—and a more earnest and more generally diffused interest was naturally felt in the researches connected with its improvement.

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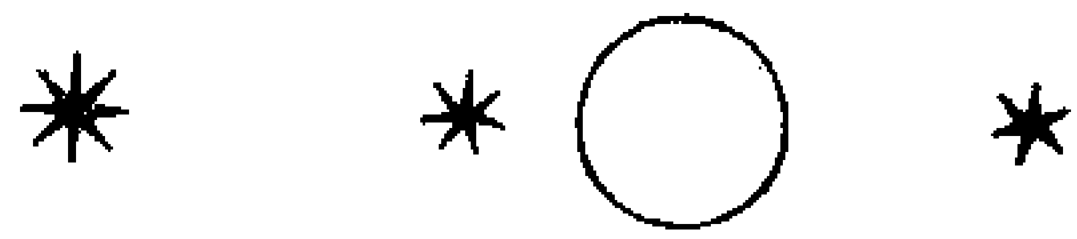
* Phil. Trans. 1790.

When Galileo first turned his telescope to the planets, he was delighted to perceive that they exhibited a round appearance like the sun or moon. Jupiter presented a disc of considerable magnitude, but in no other respect was he distinguishable from the rest of the superior planets. Having, however, examined him with a new telescope of superior power on the 7th January, 1610, his attention was soon drawn to three small but very bright stars that appeared in his vicinity, two on the east side and one on the west side of him. He imagined them to be three fixed stars, and still there was something in their appearance which excited his admiration. They were all disposed in a right line parallel to the plane of the ecliptic, and were brighter than other stars of the same magnitude.



This did not, however, induce him to alter his opinion that they were fixed stars, and therefore he paid no attention to their distances from each other, or from the planet. Happening, by mere accident, to examine Jupiter again on the 8th January *, he was surprised to find that the stars were now arranged quite differently from what they were when he

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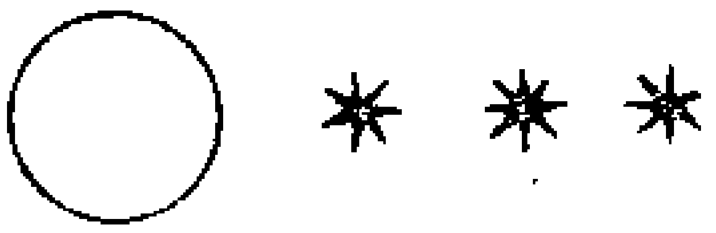
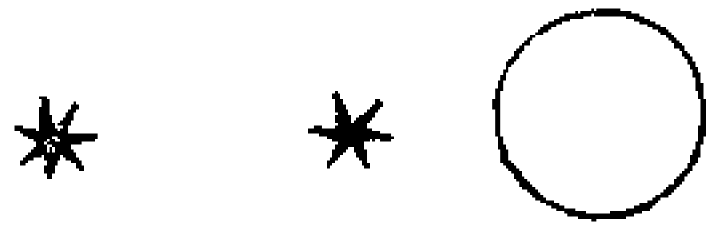
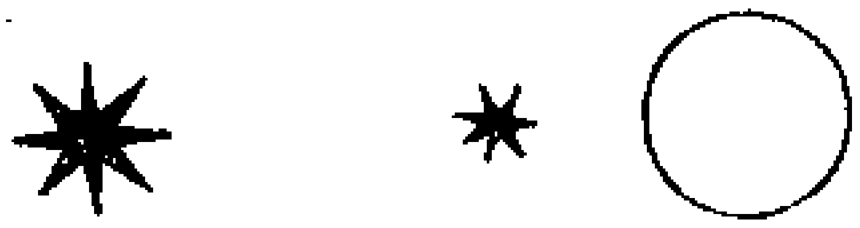
* "Cum autem die octava, nescio quo fato ductus, ad inspectionem eandem reversus essem."—Sidereus Nuncius, p. 20.

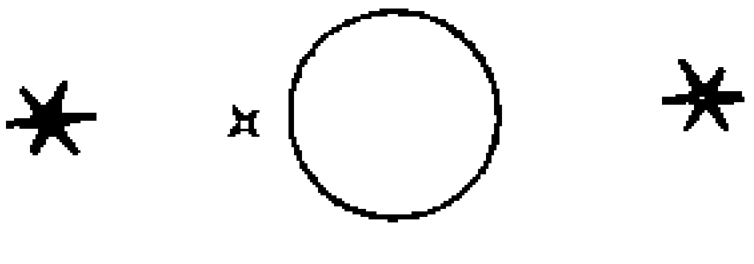
first saw them. They were all now on the west side of the planet, and were nearer to each other than they had been on the previous evening; they were also disposed at equal distances from each other. The strange fact of the mutual approach of the stars did not yet strike his attention, but it excited his astonishment, that Jupiter should be seen to the east of them all, when only the preceding night he had been seen to the west of two of them. He was induced, on this account, to suspect that the motion of the planet might be *direct*, contrary to the calculations of astronomers, and that he had got in advance of the stars by means of his proper motion. He therefore waited for the following night with great anxiety, but his hopes were disappointed, for the heavens were on all sides enveloped in clouds. On the 10th he saw only two stars, and they were both on the east side of Jupiter. He suspected that the third might be concealed behind the disc of the planet. They appeared as before in the same right line with him, and lay in the direction of the zodiac. Unable to account for such changes by the motion of the planet, and being at the same time fully assured that he always observed the same stars, his doubts now resolved themselves into admiration, and he found that the apparent motions should be referred to the stars themselves and not to the planet. He therefore deemed it an object of paramount importance to watch them with increased attention.



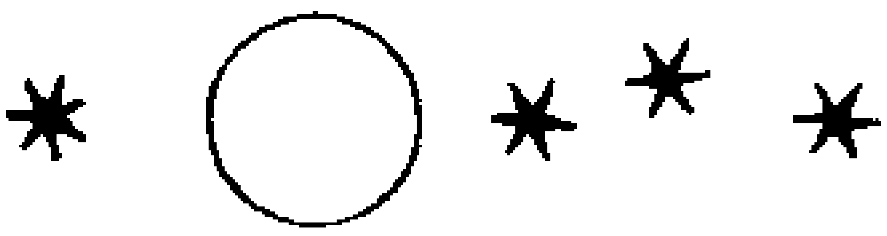


On the 11th he again saw only two stars, and they were also both on the east side of Jupiter. The more eastern one appeared nearly twice as large as the other, although on the previous evening he had found them almost equal. This fact, when considered in connexion with the constant change of the relative positions of the stars and the total disappearance of one of them, left no doubt on his mind of their real character. He therefore came to the conclusion, that there are in the heavens three stars revolving round Jupiter in the same manner as Venus and Mercury revolve round the sun. On the 12th he saw three stars; two on the east side of Jupiter, and one on the west side. The third began to appear about three o'clock in the morning, emerging from the eastern limb of the





planet; it was then exceedingly small, and was discernible only with great difficulty. On the 13th he finally saw four stars. Three of them



were on the west side of the planet, and the remaining one on the east side. They were all arranged in a line parallel to the ecliptic, with the exception of the central star of the three western ones, which declined a little towards the north. They appeared of the same magnitude, and, though small, were very brilliant, shining with a much greater lustre than fixed stars of the same magnitude [*].

The future observations of Galileo established beyond all doubt that Jupiter was attended by four satellites. He continued to examine them until the latter end of March, noting their configurations, and recording the stars which appeared in the same field of view with them.

Soon after Galileo's famous discovery, he perceived the utility of the satellites for finding the longitude, and he continued for many years to make observations on them, with the view of constructing a theory of their motions. Much has been said about his tables of the satellites, which were to have been published by his friend and pupil Rimieri, but which, by some unaccountable accident, disappeared at the death of that person, and could nowhere be found, until they were finally discovered a few years ago in a private library at Rome. We know that Galileo himself was very sanguine of their practical utility, but his opinion of their merits does not seem to be borne out by the actual examination of them consequent on their rediscovery. Indeed, when we reflect on the many painful efforts which it cost his successors to arrive at even a tolerable knowledge of the elements of the satellites, we might very reasonably conclude, *à priori*, that his tables can only be regarded in the present day as an object of scientific curiosity. An interesting fragment of his early researches on the satellites is to be found in one of his letters to Welser, the person through whom he carried on the controversy with Schener the Jesuit, relative to the discovery of the solar spots. At the end of a letter dated December 1st, 1612, he gives a sketch in rough drawings of the configurations of the satellites from 1st March till 7th May of the following year.

Simon Mayer, the German astronomer, who contended for the independent discovery of the satellites, resolved to strengthen his claims by the construction of tables of their motions. The crude labours of this impudent pretender were, however, no sooner given to the world than they fell into deserved oblivion. Hodierna, a Sicilian astronomer, is the next person who is mentioned as having devoted his attention to this subject. In 1656 he published his observations on the satellites, accompanied with remarks on the theory of their motions. He is the first astronomer who pointed out the superior importance of eclipses of the satellites as compared with other phenomena. He also calculated tables of their motions, but they are said to have been so very inaccurate, that in a few years they even ceased to represent the configurations of the different bodies. In 1666 Borelli attempted to establish a theory of the satellites, by means of his own observations and those of Hodierna, but his labours were attended with very imperfect success.

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[*] The preceding configurations are derived from those given by the illustrious discoverer in the Sidereus Nuncius.

The earliest tables which enjoyed any confidence among astronomers were those of Cassini, which first appeared in 1668 at Bologna. Picard, the celebrated French astronomer, having compared them with a number of observed eclipses, found them to be even more accurate than their author anticipated. He, in consequence, recommended him to Colbert as an astronomer, whose talents would be an ornament to France, and, at the suggestion of that minister, Louis the Fourteenth invited him to his capital. Cassini, upon his arrival in Paris, resolved to perfect his previous researches on the motions of the satellites, and during many years he continued to make observations on their eclipses. In 1693 he published his second tables, which far exceeded in accuracy any previous efforts of the kind. Those of the first satellite especially were found to represent the times of the eclipses with remarkable fidelity, and by means of them the longitude was determined with a precision hitherto unknown. In these tables the orbits of the four satellites were considered to be circular; they were inclined to Jupiter's orbit at equal angles, and their nodes had all a common position. Cassini estimated the inclination of the orbits at 2° 55′, and he fixed the nodes in 10^s 14° 30′ of longitude; both of these elements were supposed invariable. He did not employ the equation of light in his tables, although at one time he was favourably disposed towards the hypothesis of Roemer. He perceived that the successive propagation of light explained the irregularities in the eclipses of the first satellite when the earth was in different positions of her orbit; but, finding that it did not account in an equally satisfactory manner for the irregularities of the other satellites, he rejected it altogether, and instead of it he used in the tables of the first satellite an empiric equation depending on the relative positions of the Earth and Jupiter. Although the error in an eclipse of the first satellite seldom exceeded 1′ of time, yet it happened occasionally that it rose to 5′ or 6′. The inequality which principally occasioned this error was certainly not easy to discover; but it is surprising that a similar inequality in the second satellite, which rises to a much greater magnitude, should have escaped the sagacity of Cassini. He also failed to notice the principal inequality in the fourth satellite, although it causes the times of eclipses to vary to the extent of an hour. Notwithstanding these defects, the tables of Cassini mark an important epoch in the history of the satellites, and their construction will ever remain a monument of the ingenuity and patience of their illustrious author.

Maraldi I., the nephew of Cassini, also devoted much attention to the subject of Jupiter's satellites. He admitted, in common with his relative, that the equation of light gave a very satisfactory account of the errors in the first satellite, when the earth was in different parts of her orbit, but he maintained that, if this equation was founded upon true physical principles, it should vary from the perihelion to the aphelion of Jupiter's orbit, a conclusion which the observations on eclipses did not seem to him to warrant. He also remarked, that if the errors in the times of the eclipses depended upon the successive propagation of light, they should be equal for all the satellites when the earth was in the same parts of her orbit. It did not occur to him that other irregularities might exist in the motions of the satellites, and might cause the errors of eclipses to be very different for each satellite. It is true that the orbits were supposed circular, and as long as astronomers entertained this belief there could be no equation of the centre; but, as Delambre justly remarks, there might exist the inequality of the variation even in a circular orbit, and, as long as it was neglected, it would occasion an apparent discordance between the several equations of light. The arguments of Maraldi were, however, considered by some of his most eminent contemporaries to be fatal to the theory of Roemer. "It appears then," says Fontenelle, "that we must renounce, though perhaps with regret, the ingenious and seductive hypothesis of the successive propagation of light . . . . . How little prevents us from falling into great errors! If Jupiter had but one satellite, or if the eccentricity had been less, and these two things are very possible, we should have concluded with the utmost confidence that light traversed the annual orbit of the earth in 14 minutes."*

Maraldi first established the important fact, that the inclination of the second satellite is variable. He was led to this discovery by observing that the duration of eclipses was not always the same when the satellite was at the same distance from the nodes, a fact of which he assured himself by a careful comparison of a great number of eclipses. For example, on the 21st January, 1668, when Jupiter was in that part of his orbit wherein the eclipses are shortest, he found that the semi-duration of the eclipse was $1^h\ 19^m$; on the 17th September, 1715, when all the circumstances were the same, the semi-duration was only $1^h\ 7^m\ 14^s$. The difference amounted to $11^m\ 46^s$, and, as this quantity was too great to be ascribed to the errors of observation, he concluded that it must have proceeded from a change in some of the elements upon which the phenomenon depended. He remarked that the duration of the eclipse might be modified by three distinct causes; 1st, a variation in the eccentricity of the satellite; 2nd, a variation in the inclination; 3rd, a variation in the place of the nodes. With respect to the first of these causes, the variation would require to be enormous, in order that it might occasion so great a difference between the eclipses; with respect to the second, he remarked that the duration of the eclipse varied even when the satellite was at the same distance from the node. He concluded, therefore, that the phenomenon must be ascribed to a change in the inclination of the orbit. In 1707 he found the inclination to be equal to 3° 33′; whence it appeared to have increased nearly a degree since the publication of Cassini's tables.

Bradley is the next astronomer whose researches contributed to throw light upon this interesting subject. In 1719 he constructed tables of the satellites; but they were not given to the world until 1749, when they were published along with Halley's tables of the planets. In these tables the places of the satellites were given by Bradley, in degrees and minutes of space; but there were appended to them ecliptic tables of the first satellite calculated in time by Pond, his uncle. He determined the mean motions with great accuracy, by means of a comparison between the observations of preceding astronomers and those made by himself at Wanstead, after Jupiter had completed four revolutions. We have seen that Cassini and Maraldi refused to admit the equation of light; Halley, in 1694, argued more philosophically on the subject; for he maintained the necessity of applying it to all the satellites. Bradley was the first who introduced this equation into the tables of their motions. He fixed it at the usually received value of 14^m, adding a smaller equation of $3\frac{1}{2}^m$ to account for the effect of Jupiter's eccentricity. The maximum value which he assigned to the aberration of light would have given him 16^{m} 26^{s}[*] for the greater equation—a result which would have been much more conformable to observation than the quantity he actually employed. It is surprising that he should have overlooked the importance of his great discovery in furnishing an independent means of calculating this equation.

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* Mém. Acad. des Sciences, Hist. p. 80.

The irregularities in the motions of the satellites were the cause of much perplexity to Bradley. The second satellite, especially, presented anomalies which could not be accounted for either by a circular or an elliptic orbit. Sometimes it deviated from its mean place to so great an extent, and in so short a time, as to be incompatible with a small eccentricity; while, on the contrary, other observations rendered it impossible that the orbit should differ much from a circle. He discovered that the three interior satellites passed through the irregularities of their motions in 437 days; the errors returning at the close of this period in the same order and magnitude as before. He considered that about the middle of this period the inequality of the second satellite might amount to 30 or 40 minutes. He remarked that the period of the inequalities corresponded to that which brought back the satellites to the same position relatively to each other, and to the axis of Jupiter's shadow; and he hence inferred, with his usual sagacity, that the inequalities resulted from the mutual attraction of the satellites. "While we carefully attend," says he, "to future observations, by means of which the theory of the satellites may be established, *à posteriori*, let us hope that some rival of the great Newton, relying upon the sure and tried principle of gravitation, will achieve the noble task of investigating *à priori* the effects of their mutual attraction." Bradley retained the inclinations of the three interior satellites at 2° 55′, as fixed by Cassini; but he reduced that of the fourth to 2° 40′. This was a happy alteration; Delambre's tables make it 2° 40′ 42″ for the same epoch. He also discovered that the orbit of the fourth satellite is eccentric; and he fixed the maximum value of the equation of the centre at 48^{m}.

Maraldi II. devoted much of his time to researches on the satellites, and effected some very important improvements in the theory of their motions. In a memoir, which appeared in the volume of the Academy of Sciences for 1732, he proved that the inclination of the third satellite is variable; and he also established the eccentricity of the fourth satellite. With respect to the first of these points, he found that the durations of the eclipses of the satellites had been continually diminishing ever since the year 1693. It was impossible to explain this constant diminution by an eccentricity in the orbit, since the effect of such a supposition would be to produce sometimes a diminution; and at other times an increase in the duration of the eclipse. Nor would a motion of the nodes suffice for this purpose; for he shewed that the utmost change in their position which could possibly occur would not exceed 3°, and this would occasion a change of only 10^{s} in the duration of eclipses at the limits; whereas observation shewed it to amount to 16^{m} 44^{s}. Besides, upon this supposition, the same variation ought to have manifested itself at the nodes as at the limits; but the duration of eclipses varied only to a very small extent when they happened in the former of these positions. The observations, therefore, could only be reconciled together by admitting that the inclination of the orbit was continually increasing. He calculated the inclination for 1691 and 1727, and found it to be at the former of these epochs 3° 0′ 30″, at the latter, 3° 12′ 5″. Thus it appeared to have increased to the extent of 11′ 35″ in the space of 36 years.

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[*] Strictly speaking, the equation is equal only to half this quantity; but in the tables of the satellites the coefficients are doubled in order to render the results always additive.

Maraldi established, in an equally satisfactory manner, the eccentricity of the fourth satellite, and fixed the greatest equation of the centre at 55^m. This was a much more accurate determination than Bradley's; Delambre's tables make it $55^m\ 28^s$. At the conclusion of his memoir he intimates a suspicion that the orbit of the first satellite was equally eccentric. The inequality in time would be eight or nine times less on account of the more rapid motion of the satellite; but, in fact, the suggestion of Maraldi is altogether erroneous. Delambre, notwithstanding the vast extent of his researches, was unable to discover the slightest trace of ellipticity in the orbit of the first satellite; which may therefore be considered as offering, in this respect, a feature quite singular in the system of the world.

The astronomers of France, influenced by an undue admiration of Cassini, were long reluctant to introduce any change into the elements of the satellites as assigned by him in his tables. The elder Maraldi finally mustered sufficient courage to emancipate his judgment from this thraldom, by announcing that the inclination of the second satellite is variable. Bradley advanced much further in his researches; but, by a very absurd instance of negligence, his discoveries were not communicated to the world until thirty years after he was in possession of them. Meanwhile, Maraldi II. contributed, by his labours, to widen the breach his father had already made in the theory of Cassini; for he had just shewn that one of the orbits was eccentric, and that the inclination of another was variable. Fontenelle makes some very judicious reflections in connexion with these salutary innovations. "All this," says he, "begins to verify what we announced, and in some sort predicted, in 1727, that the doctrine of concentric orbits, immoveable nodes, and constant inclinations, possibly might not exist in the theory of the satellites; they were not physical enough in their character, nor did they present that kind of regularity which nature loves to follow. Already we see the constancy of the inclinations destroyed in the three first satellites, and the concentricity in the fourth. The immobility of the nodes still holds out; but it is very probable that in the end all will share the same fate." *

The same feeling of excessive veneration of Cassini to which we have just alluded, as having, in some degree, retarded the theory of the satellites, also induced the successors, and especially the more immediate relatives of that astronomer, to refuse introducing into the tables of the satellites the equation depending on the successive propagation of light. Maraldi II., in the outset of his career, imitated the example of all the members of his family, by strenuously opposing the ingenious theory of Roemer. Bradley, however, having dispelled all doubts upon this question by his discovery of aberration, Maraldi no longer persevered in rejecting the equation of light; and, in a memoir published by him in 1741, he shewed that it explained much of the irregularity observable in the motion of the third satellite.

In 1746 Wargentin, a Swedish astronomer, published tables of Jupiter's satellites, which far exceeded in accuracy any that had yet appeared. This meritorious individual devoted his whole life to researches on the

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* Mém. Acad. des Sciences, 1732, Hist. p. 85.

motions of these interesting bodies; and the success which attended his labours affords an encouraging illustration of the valuable results which may be achieved by a mind, even although gifted with no extraordinary powers, when its whole energies are perseveringly directed to any specific object. Having collected together all the observations which could be considered as worthy of any confidence, he instituted a careful comparison between them; and in this manner he was led to form a number of empiric equations, which enabled him to represent the motions of the satellites with wonderful accuracy. He applied the equation of light to all the satellites; but he judiciously profited by Bradley's discovery in fixing its maximum value at $16^m\ 25^s$ instead of 14^m, the quantity that had been originally suggested by the errors in the eclipses of the first satellite. The tables of the first satellite, constructed severally by Cassini and Pond, although they generally represented the times of eclipses with great precision, yet happened occasionally to be 6 or 7 minutes in error. Wargentin applied an equation of $3^m\ 40^s$ with a period of $437^d\ 19^h\ 41^m$, and by this means he reduced the error to 1^m. The irregularities of the second satellite were much more considerable than those of the first, and had very much perplexed astronomers. Wargentin almost entirely removed these anomalies by applying an equation of $16\frac{1}{2}^m$ with a period equal to that of the equation of the first satellite. We have seen that Bradley had already detected these inequalities; but, as his remarks were not published until 1749, the merit of independent discovery cannot be withheld from the Swedish astronomer.

In 1759 there appeared, in Lalande's astronomy, an improved edition of Wargentin's tables of the satellites. The most important change was made in the tables of the third satellite. Maraldi had already suspected that the orbit was eccentric; but he did not attempt to estimate the equation of the centre. Wargentin, whose views were directed solely towards the perfection of the tables, attempted to satisfy the observed irregularities of the satellite by means of an empiric equation of 8^m, the period of which he estimated at 12 years. In 1771 he published in the Nautical Almanack a new edition of the tables of this satellite. Instead of the equation of 8^m, which his tables of 1759 contained, he now employed three different equations. One of these was equal to $2^m\ 30^s$, and had a period of $437^d\ 19^h\ 41^m$, similarly to the equations of the first and second satellites. The others amounted to $4^m\ 30^s$ and $2^m\ 30^s$; the period of the greater equation being somewhat more than $12\frac{1}{2}$ years, and that of the smaller one being nearly 14 years. Speaking of the inequalities which formed the basis of these two equations, he says that, towards the close of the preceding century and the beginning of the current one, they both conspired in the same direction, and formed one large inequality of 15 or 16 minutes. Subsequently, in consequence of the difference of their periods, the one had been accelerating, while the other was retarding, the satellite; until at length they almost destroyed each other, and it became possible to omit them altogether by merely adding 7^m to the epoch. He confesses that his hypothesis does not posesss the character of probability; but he considers that it may be admitted until experience should put astronomers in possession of a more accurate mode of reconciling all the observations.* He concludes with the remark, that, perhaps, the orbit might somehow have a variable eccentricity, and, in that case, that the two inequalities might in reality be only one. He subsequently adopted this hypothesis as offering the best explanation of the phenomenon.

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* Tabulæ Novæ Tertii Satellitis Jovis, Lond., 1779, p. 12.

In 1781 he wrote to Lalande, stating that recent observations induced him to suppose there was only one inequality, with a period of about thirteen years. This inequality amounted to $7\frac{1}{2}^{m}$ between 1670 and 1720. From 1720 to 1760 it had diminished to $2\frac{1}{2}^{m}$, and it had remained constant during the succeeding twenty years*. This was not a happy modification of the original idea of two independent equations. Lagrange and Laplace have demonstrated, *à priori*, the existence of two distinct equations of the centre, in the motion of this satellite, and this remarkable result of pure theory has been confirmed in the most satisfactory manner by the laborious researches of Delambre.

Very little progress had been made by astronomers in the researches relative to the nodes of the satellites. In 1758 Maraldi invested this subject with a lively interest by the communication of a memoir to the Academy of Sciences, in which he announced that the nodes of the fourth satellite had a *direct* motion upon the plane of Jupiter's orbit. Newton, by considering the action of the sun upon the satellite, had found the motion to be retrograde, as in the case of the moon's nodes relative to the earth's orbit. Wargentin concurred with Maraldi in supposing that the motion was direct, and he fixed its annual value at 4′ 15″. This unexpected fact seemed to be at variance with the theory of gravitation, for Newton and his followers had shewn that the mean effect of a disturbing force was to occasion a retrograde motion of the nodes on the plane of the disturbing body. Lalande, however, shewed that the motion of the nodes might be direct upon one plane and retrograde upon another, and upon this ground he contended that, unless the principal disturbing force passed through the plane of Jupiter's orbit, the motion would not be *necessarily* retrograde. Now, in the present case, the third satellite exercises a much more powerful influence on the motion of the nodes than the sun does, and the same is true of the ellipticity of Jupiter. However, as neither the plane in which the satellite revolves, nor the plane of the planet's equator, coincides with the plane of the planet's orbit, it followed that the direct motion of the nodes on the latter plane could not be considered as invalidating the theory of gravitation.

The inclinations of the orbits presented great difficulties to astronomers, and formed the subject of much laborious research. We have seen that the elder Maraldi first discovered that the inclination of the second satellite is variable. Wargentin afterwards found that during fifteen years and a half the inclination continually increased, and that it then diminished by like degrees during other fifteen years and a half†. The whole period of variation was therefore equal to about thirty-one years. He fixed the extreme limits of the inclination at 3° 47′ and 1° 18′. Subsequently he made the greatest inclination 3° 46′, and the least 2° 46′. In 1765 Maraldi published a memoir, in which he shewed that the inequalities in the duration of eclipses could not be explained by the periodic change of the inclination. Proceeding on the supposition that the nodes were fixed, he calculated the inclination for two eclipses, observed in 1714 and 1715, and he found that, during the eleven months embraced between them, it had increased to the extent of 20′, or more than a fourth of the whole periodic variation. By a similar process he found that, during thirteen months which elapsed between two eclipses, observed in 1750 and 1751, the inclination presented a diminution of 18′ 20″. These great changes occurred in such brief intervals, that it was impossible they could have proceeded from the periodic variation of the inclination. Maraldi finally discovered that the observations might be reconciled with the generally received theory of the inclination, by supposing the nodes to librate continually on each side of their mean place to the extent of 10° 13′ 48″, the period of libration being equal to that which restored the same values of the inclination. Having compared this hypothesis with observation, he was gratified to find that a remarkable accordance generally prevailed between the results derived from both sources. Among 127 eclipses which he calculated, the difference between the observed and computed durations did not exceed 2^m, except in one instance; and only 8 were found in which the difference exceeded 1^m*.

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* Lalande, Traité d'Astronomie, tome iii. p. 145.

† Mém. Acad. Upsal. 1743.

Bailly shewed that the libration of the nodes proceeded from their retrograde motion on the orbit of the first satellite, which was in this case the principal disturbing body. He also remarked that the inclination was constant with respect to the orbit of this satellite, and that it was variable with respect to Jupiter's orbit, in consequence of the retrograde motion of the nodes upon the former orbit. This explained the coincidence of the period of libration with that which restored the inclination to the same value.

When we consider the complicated character of the phenomenon investigated by Maraldi, his explanation of it must be regarded as one of the most ingenious conceptions which mere observation has ever suggested to the astronomer. Laplace has employed this libration of the nodes as one of the data from which he derived the masses of the satellites. Wargentin published tables of the second satellite for the Nautical Almanack for 1779; the most remarkable peculiarity of which was the libratory motion of the nodes, which he admits to have been first suggested by Maraldi.

The period in which the inclination of the third satellite passed through all its values was much longer than the corresponding period of the second satellite. Maraldi had found that during the current century it had been continually increasing. In 1763 he fixed it at 3° 25′ 41″. This indicated an increase of 22′ since the publication of Cassini's tables. In 1769 he discovered that the inclination was only 3° 23′ 33″. Pursuing his researches, he found that the inclination reached its maximum in the years 1633 and 1765, and its minimum in 1699. Hence it followed that the inclination increased during 66 years, and then diminished during an equal space of time; the whole variation being consequently comprised within a period of 132 years. Maraldi also fixed the mean place of the nodes in 10^s 13° 52′, and estimated the extent of libration at 1° 32′ 24″†.

The inclination of the fourth satellite had long been considered invariable. We have seen that Bradley estimated it at 2° 42′. Wargentin, in 1750, made it 2° 39′, and Maraldi, in 1758, made it 2° 36′. In 1781 Wargentin concluded from his researches that during a few preceding years the inclination had been slowly increasing. This opinion has been confirmed by the observations of subsequent astronomers, who have found that the increase of inclination has been going on down to the present day.

Bailly, before becoming acquainted with the researches of Wargentin, made the following sagacious remark upon this subject in his History of Astronomy—"The inclination of the fourth satellite has not hitherto appeared to vary sensibly; but it *will* vary, for everything in the universe is subject to fixed laws; and the same circumstances always reproduce the same phenomena. We perceive merely that the period of the inclination must be very long and must extend to several centuries."*

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* Mém. Acad. des Sciences, 1768. † Ibid. 1769.

This prediction has been verified by the physical researches of Laplace, who has found that the nodes have an annual retrograde motion of 4′ 32″ upon a fixed plane, accomplishing a complete revolution in 532 years. It is this revolution of the nodes which occasions a variation of an equal period in the inclination of the satellite.

CHAPTER VIII.

Physical Theory of the Satellites.—Newton.—Euler.—Walmsley.—Bailly computes the Perturbations of the Satellites.—Researches of Lagrange.—He obtains for each Satellite four Equations of the Centre and four Equations of Latitude.—His mode of representing the Positions of the Orbits.—Inutility of his Theory in the Construction of Tables.—Laplace.—His Explanation of the constant Relations between the Epochs and Mean Motions of the three interior Satellites.—He completes the Physical Theory of the Satellites.—Delambre.—He calculates Tables on the Basis of Laplace's Theory.—He determines the Maximum Value of Aberration by means of the Eclipses of the first Satellite.—Agreement of his Result with Bradley's.—Conclusions derivable from it.

AFTER much laborious observation and research, the theory of the satellites was now sufficiently matured to form the basis of an explanation of their motions by the principle of universal gravitation. It is worthy of remark, that this is the order in which the various branches of astronomy have advanced towards their present high state of perfection. The phenomena were first observed, and all the details relating to them carefully recorded. They were then submitted to a critical discussion, and, by a sagacious discrimination of their several peculiarities, they were grouped together under general laws. Finally these laws, although at first merely empiric, served the valuable purpose of suggesting the physical principles on which they depended; and when once this dependance was fully established, they henceforward assumed the more elevated character of laws of nature. This order of inquiry is especially manifest in the history of the lunar theory. A similar course has also been strongly recommended in our own day for the purpose of extending our knowledge of the Tides; and, indeed, it may be considered as offering the only means of ever conducting philosophers to a complete theory of that subject founded upon rigorous principles of geometry and physics.

If we only considered the disturbing action of the sun upon the satellites, the derangements in their motions would be in all respects analogous to those in the motion of the moon; and the analysis employed in the lunar theory would suffice for their complete investigation. In the present case, however, the problem is much more complicated. Each satellite is disturbed, not only by the sun, but by the other three satellites in course of every synodic revolution round the central body. Nor are these the only sources of complication. If Jupiter were a perfect sphere, his attraction would be the same, both in quantity and direction, as if his whole mass were collected at his centre; and the question relative to his action upon each satellite would be reduced to the simple consideration of a material particle attracting the body at that point. This, however, is not the real case of nature; for observation shews that the figure of the planet is that of an oblate spheroid, whose axes [illegible]e to each other nearly as 13 to 14. This circumstance causes the law of his attraction to deviate from the inverse square of the distance, and hence originates a disturbing force which powerfully deranges the motions of the satellites.

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* Bailly, Hist. Ast. Mod., tome iii. p. 183.

Newton, in the third book of the Principia, considers the disturbing action of the sun upon Jupiter's satellites, and attempts to determine the inequalities of their motions by the principles of the lunar theory. In this manner he found that the nodes of the fourth satellite had a retrograde motion upon the plane of Jupiter's orbit, the annual quantity of which amounted to 5′*. We have seen that this result was subsequently contradicted by observation; the actual motion having been discovered by astronomers to be direct. The phenomenon in question does not, in fact, depend so much upon the sun as upon the third satellite and the ellipticity of Jupiter; causes of disturbance which were not taken into account by Newton.

Euler, in 1748, first remarked that the spheroidal figure of Jupiter would occasion an irregularity in the law of his attraction †. Walmsley, in 1758 ‡, shewed that the disturbance hence arising would produce a motion of the nodes and apsides of each satellite. In 1763 Euler communicated a memoir to the Academy of Berlin, in which he examined the perturbations of a satellite revolving round a planet of a spheroidal figure. He shewed that when the satellite revolved in the plane of the planet's equator the action of the protuberant matter generally occasioned a progressive motion of the apsides. As the orbit of the satellite became more inclined to this plane, the motion of the apsides continually diminished, and it ceased altogether when the angle of inclination was equal to 54° 44′. From this position the motion of the apsides was regressive, and it continued to increase until the orbit of the satellite was perpendicular to the plane of the equator.

Bailly §, about the same time, employed Clairaut's theory of the moon in researches on the perturbations of the satellites. He discovered, by a simple analysis, that the mutual attraction of the three interior satellites occasioned those inequalities in their motions which produced a regular return of their eclipses at the end of 437 days. We have seen that Bradley was the first astronomer who threw out a suspicion of this fact. These inequalities are precisely analogous to the lunar *variation*, the only difference being, that the disturbing body is in each case one of the satellites themselves, and not the sun. In the theory of the first satellite the principal disturbing body is the second, for the exterior satellites are too remote to exercise any sensible influence, and the effect of Jupiter's ellipticity is equally inappreciable, because the orbit of the satellite is situated in the plane of his equator, and at the same time does not possess any eccentricity. It is clear, then, that by comparing the coëfficient of the equation furnished by theory with the magnitude of the inequality, as assigned by observation, the mass of the second satellite may be readily determined. In this manner Bailly found it to be equal to 0.0000211, Jupiter's mass being supposed equal to unity. This was a tolerable approximation to the true value. Laplace makes the mass equal to 0.0000232.

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* Principia, lib. iii. prop. 23. Newton, in the same proposition, makes the inequality of the fourth satellite depending on the disturbing action of the sun, and, similar to the lunar variations, equal to 5″ 12‴.

† Recherches des Inégalités de Jupiter et de Saturne. Prix de l'Académie, tome vii.

‡ Phil. Trans., 1758.

§ Born at Paris in 1736; perished by the guillotine in 1793.

The inequality of the second satellite is essentially a more complex phenomenon than that of the first, for it depends on the combined action of the first and third satellites. In form, however, the two inequalities are precisely similar, the effects of the disturbing bodies in the theory of the second satellite being blended together so as to form one great inequality, governed by the same law, and extending over the same period as the inequality of the first satellite. This singular coincidence derives its origin from two remarkable relations, connecting together the mean longitudes and mean motions of the three interior satellites. Bailly found that the equation of sensible magnitude, depending on the action of the first satellite, is expressed by the sine of the difference between the mean longitudes of the first and second, and that the equation of a similar nature, depending on the action of the third satellite, is expressed by the sine of twice the difference between the mean longitudes of the third and second *. Now, observation shewed that these two arcs differed from 180° by only a very small quantity. Wargentin's tables, in fact, suppose the difference to be equal only to 30′ at the commencement of the year 1760. The arcs being therefore nearly supplementary to each other, it followed that their sines were equal, and hence Bailly was enabled to combine the two equations together, by merely adding their coefficients and retaining either of the arguments. It is in consequence of this union of the effects of the two disturbing satellites that the inequality of the second satellite exceeds so much the analogous inequalities in longitude of the first and third.

The derangements produced by the two disturbing satellites being thus confounded together, it was impossible to pronounce how much of the resulting inequality was due to each satellite; and hence, in order to determine the masses of those bodies, another independent datum, derived from observation, was indispensable. Bailly selected for this purpose the motion of the nodes of the second satellite. This phenomenon depends on the action of the first and third satellites, and also upon the disturbance occasioned by the oblate figure of Jupiter. Having formed an ingenious supposition respecting the density of the planet, Bailly computed the effect of his oblateness in disturbing the place of the nodes, and then, subducting the result from the observed motion, he obtained the quantity due to the action of the two satellites. Combining this datum with the one assigned by the inequality in longitude, he determined the masses of

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* In both cases there are terms depending on the two arguments mentioned in the text, but, when the first satellite is the disturbing body, the term having for its argument the difference between the mean longitudes greatly exceeds all the others; and, on the other hand, when the third satellite is the disturbing body, the most considerable term is that depending on *twice* the difference between the mean longitudes. The predominance of these terms arises from the fact, that twice the mean motion of the second satellite is very nearly equal to the mean motion of the first, and twice the mean motion of the third satellite is very nearly equal to the mean motion of the second. The circumstance is, indeed, exactly similar to that which gives rise to the long inequality in the theory of Jupiter and Saturn, or to the analogous inequality in the theory of the Earth and Venus. The inequalities of the satellites differ, however, from those just cited, in being independent of the eccentricities. Their investigation will be found in Woodhouse's Physical Astronomy, chapter xx. For the more intricate parts of the theory of the satellites, see the

the disturbing satellites; but the results he obtained in this instance were by no means so accurate as his estimation of the mass of the second satellite.

While Bailly was engaged in these researches, the Academy of Sciences offered their prize of 1766 for an investigation of the inequalities of the satellites. The successful competitor was Lagrange, who transmitted to the Academy a magnificent memoir on the subject. This illustrious geometer grappled with the real difficulties of the problem, which it must be acknowledged that Bailly left untouched. When the orbits of the satellites are supposed to be circular, and their planes to be all coincident, the effects of their mutual attraction may be computed in any instance by considering in succession the action of each body upon the disturbed satellite. For this purpose the method of approximation employed in the lunar theory is amply sufficient, and by means of it the inequalities, independent of the eccentricities and inclinations, may be easily calculated. But when the actual conditions of the orbits are taken into account, the mutual perturbations of the satellites become so entangled together as to render the ordinary method of integration totally inapplicable, and it becomes absolutely necessary to devise some adequate means of surmounting the difficulties of the problem. This important step was accomplished by Lagrange, who investigated the inequalities of each satellite by a method which embraced the simultaneous actions of the sun and the other three satellites, as well as the disturbance arising from the oblate figure of Jupiter. This comprehensive mode of treating the subject entailed upon him an extent of analytical research which, to use Delambre's words*, it is somewhat frightful to contemplate. Neglecting first the eccentricities and inclinations, he found for each of the three interior satellites an inequality in longitude of 437 days, corresponding to the inequality which Bradley and Wargentin originally derived from observation. We have already mentioned that Bailly succeeded in computing these inequalities by the aid of Clairaut's theory. Lagrange also calculated the value of the analogous inequality of the fourth satellite, but he found it to be insensible. Delambre's tables, in fact, make it only 11^s.

Considering next the inequalities dependent on the eccentricities, he obtained four equations of the centre for each satellite. One of these depended on the satellite's own eccentricity, the other three were the reflected effects of the eccentricities of the disturbing satellites. He did not determine the maximum values of these equations, nor did he perceive the analogy between those of the third satellite and the two equations of the centre, which Wargentin deduced from observation. This is only one of a number of instances in which this great genius failed to derive any substantial results from his brilliant researches.

Finally, he investigated the effects of the disturbing forces perpendicular to the planes of the orbits, and obtained for each satellite four equations of latitude similar to the four equations of the centre, to which his researches on the motion in longitude conducted him. He represented the position of the orbit of each satellite by means of four planes passing through the centre of Jupiter. The first of these revolved upon the orbit of the planet with a constant inclination; the second revolved in a similar manner upon the first; the third upon the second; and finally the fourth, which was the orbit of the satellite, revolved with a constant inclination upon the third.

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* "Une analyse nouvelle, puissante, et dont les développemens ont quelque chose d'effrayant." Astronomie Théorique et Pratique, tome iii. p. 498.

Lagrange did not prosecute his researches to the full extent of his design: he concluded by announcing his intention of resuming the subject on some future occasion. He did not, however, at any period of his life, realize this promise; it was reserved for Laplace to establish a complete theory of the satellites by developing the views of his great rival, and enriching them with several valuable discoveries of his own. An important mistake was committed by Lagrange, in assuming that the plane of Jupiter's equator coincided with the plane of his orbit. He was aware that the two planes did not exactly coincide, but he conceived that the angle of inclination was so small that the disturbing effect would be insensible. Laplace, however, has shewn that some of the most remarkable phenomena connected with the motions of the satellites derive their existence from this circumstance.

The researches of Lagrange did not in any degree contribute to the perfection of the tables of the satellites; but, as we have already mentioned, they afforded some valuable hints to his illustrious contemporary. Just before he communicated his memoir to the Academy, Bailly published his researches in a treatise entitled "*Essai sur la Théorie des Satellites de Jupiter.*" He, with good reason, suspected that Lagrange might anticipate him in his discoveries, and he took the precaution of securing his own rights by a timely publication of his labours.

In 1784 Laplace communicated a memoir to the Academy of Sciences, in which he explained the physical origin of two remarkable relations which connect the three interior satellites. Astronomers had discovered from observation that the mean motion of the first satellite was nearly double that of the second, and that the mean motion of the second satellite was nearly double that of the third. It hence followed that the mean motion of the first satellite, plus twice that of the third, was nearly equal to three times that of the second. Another relation no less interesting was the following:—the mean longitude of the first satellite, plus twice the mean longitude of the third, minus three times the mean longitude of the second, was always nearly equal to 180°. This is a direct consequence of the relation between the mean longitudes to which we have already referred, when speaking of the inequality in longitude of the second satellite.

If the preceding relations between the mean motions and mean longitudes were rigorous, it would follow as a necessary consequence that the three satellites could never be eclipsed at once. Now, it was found that the tabular values of the elements in question very nearly satisfied this condition. Thus, by employing the longitudes and mean motions of Wargentin, and setting out from the epoch of his tables, it has been calculated that simultaneous eclipses of the three satellites cannot take place before the lapse of 1,317,900 years*, and a difference of only a third of 1″ in the annual motion of the second satellite would suffice to render this phenomenon for ever impossible.

Laplace suspected that both relations were rigorous, and that the small differences which appeared to exist were really attributable to errors of observation. He therefore instituted a searching examination into the theory of the satellites, in hopes of discovering the source of these singular relations, in the mutual attraction of the bodies. In this scrutiny he was not disappointed, having found their physical origin among the terms involving the squares of the disturbing forces. It appeared, then, from his researches, 1°, that the mean motion of the first satellite, plus twice the mean motion of the third, minus three times the mean motion of the second, is rigorously equal to zero; 2°, that the mean longitude of the first satellite, plus twice the mean longitude of the third, minus three times the mean longitude of the second, is equal to 180°. Delambre's researches afforded a most satisfactory confirmation of these results. By comparing together a vast number of eclipses, that astronomer found that the relation between the mean motions differed from zero by only 9″.007, and that at midnight, on January 1st, 1750, the relation between the mean longitudes differed from 180° by only 1′ 3″. It is not necessary to suppose that, at the origin of their movements, the satellites were so disposed as to satisfy accurately the above-mentioned relations between the epochs and mean motions. Laplace shewed that, provided the relations were true within certain limits, the mutual attraction of the satellites would subsequently render them rigorous *. In this case, the mean longitude of the first satellite, plus twice the mean longitude of the third, minus three times the mean longitude of the second, would oscillate round 180° as a mean value. The three satellites participate in this oscillation; each satellite being affected to an extent depending on its mass, and its distance from Jupiter's centre. This phenomenon, in virtue of which the three bodies appear to balance each other by their movements, has been denominated by Laplace *the libration of the satellites*. The period of libration is the same for each satellite, and is equal to 2270^d 18^h or a little more than six years. Its extent, and the time when it is equal to zero, are two elements which can only be determined by observation. Delambre was unable to discover any traces of a libratory motion of this kind, notwithstanding the vast number of eclipses which he examined in the course of his researches for the purpose of determining the elements of Laplace's theory. It is clear, then, that if such a phenomenon does actually exist, it must be altogether insignificant; and therefore we may conclude that the above relation between the mean longitudes does not at any time differ sensibly from 180°.

As far then as observation indicates, the above-mentioned relations are rigorously true. In this case Laplace's researches tend to shew that they are also in a state of stable equilibrium, any disturbing force, which does not exceed a certain limit, merely occasioning oscillations of the mean motions and epochs on each side of a mean state. This condition of stability will for ever prevent the inequality of the second satellite from being resolved into its constituent inequalities depending on the action of the first and third satellites, as is evident from our explanation of the circumstances which determine the complete union of those inequalities. If the disturbing force should exceed the prescribed limits of stability, a libratory motion would cease to take place, and the two inequalities of the second satellite would then separate, and would henceforward continue quite distinct.

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* It has been frequently asserted in favour of the actual existence of these librations, that it is extremely improbable the relations between the epochs and mean motions should have been, at the origin, rigorously true. This argument might be admitted, if the arrangements of the planetary system were the result of a fortuitous combination of circumstances; but since there exist so unequivocal manifestations of a Supreme Intelligence presiding over them, it savours much less of sound philosophy than of impious presumption. The mathematician, in his chamber, may modify, *ad libitum*, the arbitrary constants of his problems so long as he confines his speculations to ideal existences; but

The permanent character of the relations discovered by Laplace is one of their most striking peculiarities. They are not altered by any secular inequalities in the mean motions, for these will be so determined by the mutual attraction of the satellites, that the secular inequality of the first satellite, plus twice that of the third, minus three times that of the second, shall be always equal to zero. They are equally independent of the effects of a resisting medium, for the accelerations of the satellites, while descending towards the planet, will always maintain the same relation as that which connects the mean motions. Laplace shewed that the libration extended to the rotatory motions of the satellites*. The attraction of Jupiter, in fact, causes these movements to participate in the secular inequalities of the mean motions, and consequently maintains them always, so that the rotatory motion of the first satellite, plus twice that of the third, minus three times that of the second, is equal to zero.

We have already mentioned it to be a necessary consequence of the relations between the epochs and mean motions, that the three interior satellites of Jupiter can never be eclipsed at once. In simultaneous eclipses of the second and third the first is always in conjunction with Jupiter; it is always in opposition in simultaneous transits of the other two.

Although no libration is perceptible in the satellites, their stability is liable to be disturbed by an extraneous cause acting unequally upon them; as, for example, by the passage of a comet in their neighbourhood. If the disturbing force was small, it would merely occasion a libratory motion similar to that already described; on the other hand, if it exceeded the prescribed limit of stability, it would permanently alter the mean motions and mean longitudes of the satellites. In either case, then, the presence of the force would be discoverable by means of its observed effects. It is remarkable, however, that, although the comet of 1767 and 1779 passed through the middle of Jupiter's system, no derangement was observed to ensue in consequence; and this fact affords conclusive proof that the masses of comets are very small.

In 1788 and 1789 Laplace published an elaborate theory of the satellites in the volumes of the Academy of Sciences for those years. By means of a comprehensive analysis, which embraced all the causes of perturbation, he computed the inequalities of the satellites, both in longitude and latitude, and obtained results which proved of incalculable service to the practical astronomer. From the perturbations in longitude he derived four equations of the centre, after the example of Lagrange, who had been conducted to a similar conclusion by his researches in 1766. The orbit of the first satellite being, according to all appearance, perfectly circular, and the orbits of the second and third being nearly so, the three equations of the centre depending upon the disturbing satellites are generally insensible, with the exception of the one in the third satellite depending upon the action of the fourth, the orbit of which is considerably eccentric. The combination of this equation with the satellite's own equation of the centre gives rise to a single equation of variable magnitude, which imparts a somewhat complicated character to the motion of the satellite, and renders it difficult to trace the respective sources of each inequality by the aid of mere observation. Laplace found from theory that the lower apsides of the two satellites coincided in the year 1682; and, in consequence, the two equations combined together into one equation equal to their sum, and amounting to 796″.411 of space. In 1777 the lower apsis of the third satellite had advanced 180° before that of the fourth, and the resulting equation was equal only to the difference of the two elementary equations. In this position it therefore only amounted to 307″.651. These results are exactly conformable to those which we have seen that Wargentin and other astronomers had previously derived from observation. In his researches on the perturbations in latitude, Laplace gave a strong proof of his sagacity by taking into account the effect of the inclination of Jupiter's equator to his orbit. We have already remarked that this important element of disturbance was entirely omitted by Lagrange.

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* In 1713 Maraldi I. concluded, from the periodic appearance of certain spots on the

The general tendency of Jupiter's ellipticity is to draw the satellites into the plane of his equator. This is clearly seen in the actual positions of the orbits, which in each case deviate to a greater extent from the plane of the equator, according as the satellite is more remote from its primary. The inclinations of the orbits had occasioned much trouble to astronomers, chiefly in consequence of the difficulty of finding a plane of reference with which they might be connected by fixed relations. Laplace discovered that the orbit of each satellite revolved with a constant inclination upon a fixed plane contained between the planes of Jupiter's equator and orbit, and passing through their common intersection. If there had been no other disturbing body than the sun, the fixed plane of each satellite would have been the plane of Jupiter's orbit; also, if the protuberant matter around Jupiter's equator alone disturbed the satellite, the fixed plane would have coincided with the plane of his equator; and a similar result would follow if any of the other three satellites were the sole disturbing body. It is clear, then, that the actual position of the fixed plane will in each case be determined by a reference to the opposing tendencies of the five disturbing forces, and it will manifestly hold an intermediate place between the planes of Jupiter's orbit and equator, which are the two extreme planes in which these disturbing forces act. This conclusion agrees with what we have mentioned above, relative to Laplace's determination of these planes. The fixed plane of each satellite might, therefore, be considered as the resultant plane of the disturbing forces; and the motion of the nodes upon it was accordingly found by Laplace to be retrograde, conformably with the general effect of a disturbing force acting continually in the plane. This result offered a satisfactory explanation of the curious movements of the nodes on the plane of Jupiter's orbit; phenomena which, when first recognised by astronomers, appeared to be irreconcileable with the principles of the Newtonian theory, in consequence of the plane of Jupiter's orbit having been erroneously assumed to be the plane of the disturbing force.

The disturbing force which exercises most influence in determining the positions of the fixed planes, is that arising from the ellipticity of Jupiter; and this force is obviously more effective relatively to the sun's action, according as the satellite is more remote from its primary. The inclination of Jupiter's equator to his orbit is 3° 5′; but the inclinations of the fixed planes of the satellites towards Jupiter's equator, counting from the first satellite, are only 7″, 1′ 3″, 5′ 2″, 24′ 33″.

As the nodes of the satellites all regress upon their fixed planes, the mutual positions of the orbits will be continually varying; and this circumstance, by occasioning a continual alteration of the mutual action of the satellites, will disturb the absolute positions of the orbits. Laplace expressed the latitude of each satellite, relative to the plane of Jupiter's orbit, by means of five terms. The first of these depended on the position of the fixed plane, with respect to the planet's orbit; the second, on the inclination of the satellite towards its fixed plane; the other three were determined by the positions of the nodes of the disturbing satellites on their fixed planes *.

The analytical expression which Laplace obtained for the latitude enabled him to explain the singular changes which astronomers had remarked in the inclination of the fourth satellite. From 1680 till 1760, the inclination appeared to have been stationary and equal to about 2°.4; since 1760 it had been sensibly increasing. Now, Laplace found from theory, that in 1680 the inclination was equal to 2°.476; in 1620 it amounted to 2°.448, and in 1760 to 2°.441. It reached its minimum in 1756. Since that epoch it had been constantly increasing; when computed for 1800, by Laplace's formula, it is found to amount to 2°.5791. "It is curious," says Laplace, "to see thus emanating from analysis those singular phenomena which observation has partially disclosed; but which, resulting from the combination of many simple inequalities, are too complicated to allow the discovery of their laws by astronomers."†

Since the motion of each satellite is determined by three differential equations of the second order, the motions of the four satellites will be determined by twelve such differential equations. The integration of these equations will, therefore, involve twenty-four arbitrary constants; the values of which must be derived from observation. Besides these twenty-four constants, there are seven others which it is necessary to determine before tables of the satellites can be constructed upon the basis of their physical theory. These are—the masses of the four satellites; the ellipticity of Jupiter; the inclination of his equator to his orbit; and the position of the nodes of his equator. Five phenomena were selected by Laplace as best adapted for assigning the masses of the satellites and the ellipticity of their primary. The first of these was the inequality in longitude of the first satellite, extending over the period of 437.659^d, which restores the eclipses of the three interior satellites in the same order. As this inequality depends on the action of the second satellite, it gave him the mass of that body with great accuracy. The similar inequality of the second satellite depends upon the combined actions of the first and third satellites; it was the second datum employed by Laplace in these researches. The third datum was the motion of the nodes of the same satellite; a phenomenon which depends upon the action of the first and third satellites, and upon the disturbing influence of Jupiter's ellip-

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* The fixed planes of the satellites are not absolutely immoveable, since their positions are determined by those of Jupiter's orbit and equator, both of which are continually varying; the former from the action of the planets upon Jupiter, the latter from the action of the sun and the satellites upon the redundant matter accumulated round his equator. Laplace took into account the effects of both these changes in computing the latitudes of the satellites.

An interesting account of the perturbations of Jupiter's satellites will be found in Airy's Treatise on Gravitation.

† Méc. Cel., tome iv., Pref. p. xiv.

ticity. The fourth datum was the equation of the centre of the third satellite, depending on the position of the perijove of the fourth. The fifth was the motion of the apsides of the fourth satellite.

By comparing these data with his analytical formulæ, Laplace determined the masses of the satellites and the ratio between the equatorial and polar axes of their primary. It appeared from his results that the third satellite contained the greatest quantity of matter, and the first satellite the least [*]. The mass of the third satellite was found to be about double the moon's mass, and that of the fourth satellite was equal to it. By assuming the equatorial axis of Jupiter to be equal to unity, he found that the polar axis was equal to .9286. It hence followed that the lengths of the polar and equatorial axes were very nearly as 13 to 14; a result which almost coincides with that derived from micrometric measurements of the two axes. Laplace, indeed, considers that in this case theory conducts to a more accurate result than direct observation. It is assuredly one of the greatest triumphs that the human mind can boast of, to have been enabled to determine the precise shape of the planet, by merely observing the eclipses of the small bodies which circulate round him.

Delambre determined the thirty-one elements of Laplace's theory by comparing together a vast number of eclipses observed by astronomers at different periods. Having executed this important task, he then computed the numerical values of all the equations, and employed them in the construction of ecliptic tables of the satellites. These tables were inserted in the third edition of Lalande's Astronomy, which was published in the year 1792, and were found to surpass greatly in accuracy the tables of Wargentin, and those of all preceding astronomers.

The eclipses of the first satellite originally led to the discovery of the successive propagation of light, and this important doctrine was afterwards established upon an indisputable basis by Bradley's discovery of aberration. Laplace, however, conceived that the order of the inquiry might be inverted, by deriving the maximum value of aberration from the velocity of light, as indicated by the eclipses of the satellite. Having suggested this view of the question to Delambre, that indefatigable astronomer undertook the laborious task of computing the velocity of light by the discussion of a great number of eclipses; and from the result obtained by him, he concluded that the maximum value of aberration is equal to 20″.25. This value agrees precisely with that which Bradley derived from direct observations on a great number of stars. It is interesting to trace so close an agreement between two methods so widely different. This coincidence shews that the motion of light is uniform within the earth's orbit, for the aberration is derived, in the one case from the velocity of light in the earth's orbit, and, in the other case, from the time which it takes in traversing the diameter of the orbit. Its motion is also uniform within the orbit of Jupiter, for the variations of the radius vector of the planet are very sensible, and the differences in the times of eclipses which these occasion are found to correspond exactly with the supposition of the uniform motion of light.

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[*] The following are the values of the masses of the satellites as given by Laplace in the Mécanique Céleste, liv. viii. chap. viii., Jupiter's mass being supposed equal to unity.

		Mass.
1st	satellite	0.0000173281
2nd	„	0.0000232355
3rd	„	0.0000884972
4th	„	0.0000426591

CHAPTER IX.

Secular Variations of the Planets.—Elements of the Terrestrial Orbit.—Variations of the Eccentricity.—Motion of the Aphelion.—Obliquity of the Ecliptic.—Its secular Variation computed by Theory.—Euler.—Lagrange.—Laplace.—Influence of the displacement of the Ecliptic on the length of the tropical Year.—Indirect Action of the Planets on the terrestrial Spheroid.—Its effect in restricting the Variations of the Obliquity of the Ecliptic and the length of the tropical Year.—Invariable Plane of the Planetary System.—Theory of Comets.—Hevelius.—Borelli.—Dörfel.—Subjection of the Motions of Comets to the theory of Gravitation by Newton.—Halley.—Clairaut.—Researches of Lagrange on Cometary Perturbation.—Lexell's Comet.—Its Perturbations investigated by Laplace.—Publication of the Mécanique Céleste.—General Reflections on the Progress of Physical Astronomy.

The theory of the secular variations of the elements of the planetary orbits forms one of the most interesting subjects of physical astronomy. The actual existence of some of these variations was long a disputed point with astronomers; but they have been established beyond all doubt in recent times by the accuracy of modern observations. The secular variations of the terrestrial orbit have naturally excited a more lively interest than the others of the same class, on account of their connexion with the physical condition of the earth. The investigation relative to the eccentricity is manifestly an object of the highest importance, since the indefinite increase of that element, at however slow a rate, would ultimately occasion such violent alternations of heat and cold at the earth's surface, in the course of every year, as utterly to destroy the existing economy of animal and vegetable life. The sublime researches of Lagrange have shewn, however, that such a condition cannot possibly ensue; for the terrestrial eccentricity will always be maintained by the action of the planets within certain narrow limits between which it will perpetually oscillate. At present it is diminishing at the rate of 18″ in a century. An immense number of ages will elapse before it reach its minimum state; but, when this takes place, it will then pursue a contrary order of variation, increasing at the same slow rate as that at which it had previously diminished. We have seen that the variation of this element forms the medium through which the action of the planets is propagated to the moon; occasioning thereby the secular inequality in the mean motion of that body, which was so long the cause of embarrassment to mathematicians and astronomers, until its physical origin was at length discovered by Laplace.

The motion of the earth's aphelion was first discovered by the celebrated Arabian astronomer, Al Batani. As in the case of the lunar apogee, it advances in the order of the signs, though at a much slower rate. Its annual motion is estimated at 11″ or 12″. The variation of this element is interesting on account of the clear evidence it affords of the disturbing action of the planets on the earth; but it obviously cannot exercise any influence on the physical condition of the latter.

But the question is very different when we consider the position of the earth's orbit. A variation of this kind, by altering the obliquity of the ecliptic, would manifestly affect the temperature and climate of the earth it was constantly diminishing. The earliest measurement of it, if we exclude the records of Eastern nations, is due to Eratosthenes, who flourished about the year 270 A.C. This astronomer found the angle between the tropics to amount to 47° 42′ 27″, whence the obliquity of the ecliptic was equal to 23° 51′ 13″. Al Batani, in the ninth century, fixed it at 23° 35′. Waltherus, the German astronomer, made it 23° 29′ 47″ about the close of the fifteenth century; and Tycho Brahé made it 23° 29′ about the year 1581. Riccioli, Gassendi, and Flamstead maintained that the obliquity was invariable, ascribing the discordances of astronomers wholly to errors of observation. On the other hand, Bouillaud and Wendelin contended that it was continually diminishing; and this opinion was urged with great ability by Louville, in the Memoirs of the Academy of Sciences for 1716. About the middle of the last century, Bradley, Lacaille, and Mayer found the obliquity to be 23° 28′ 18″, and in 1800, Delambre, Mechain, and Lefrançius made it 23° 28′. Thus it appears that, although the earlier observations are not entitled to much confidence when considered by themselves, the aggregate of the results indicates beyond all question a constant diminution of the obliquity.

Euler first explained the variation of the obliquity of the ecliptic by the theory of gravitation. In his memoir of 1748, he showed that the action of Jupiter on the earth would occasion a displacement in the plane of the ecliptic; tending to bring it nearer to the equator. He investigated the same subject more completely in the Berlin Memoirs for 1754, and also in his memoir on the perturbations of the Planets, which was crowned by the Academy of Sciences of Paris in 1756. On the last-mentioned occasion he made the secular diminution equal to 48″, a quantity which differs only about 2″ from the most recent determinations of astronomers*.

Lagrange computed the diminution of the obliquity in the Berlin Memoirs for 1782, and obtained 61″.5 for the amount of the secular variation. This result is universally allowed to be too great. The source of Lagrange's error doubtless lay in the erroneous value which he assumed for the mass of Venus, the planet which exercises the greatest influence on the position of the ecliptic.

An interesting question arises; will the obliquity continually diminish until the equator and ecliptic coincide? If this should happen, the sun will daily attain the same meridional altitude as at the equinoxes, and an eternal spring will reign over the whole earth. Lagrange first shewed that such a condition cannot possibly exist; the mutual action of the planets occasioning only small oscillations in the positions of their orbits. The ecliptic will, therefore, continue to approach the equator until it reach the limit assigned by the action of the perturbing forces, after which it will gradually recede from that plane according to the same law as that which determined its previous approach. The diminution of the obliquity is not uniform; but the law of variation can only be ascertained by theory. The formula for computing the obliquity corresponding to any assigned time may be thus expressed:—$\theta = 23^\circ\, 27'\, 54''.8 - 0''.488566t - 0''.000005t^2$, t denoting the number of years before or after 1800. This formula will be accurate enough for all the purposes of astronomy, when the value of t does not exceed ten or twelve centuries; it will even serve for all the ancient observations, when we take into account the uncertainty that hangs over them. In the lapse of ages the law of variation will be more completely developed; and it will be necessary to include in the formula the cubes and higher powers of *t*, if the same epoch should be always retained.

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* Bessel in his Tabulæ Regiomontanæ, 1830, makes it 45″.7.

Laplace compared the preceding formula with an ancient observation recorded in the annals of the Chinese. It appears that Tcheou Kong, the regent of China, measured the summer solstice about the year 1100, before the Christian era, and from the result obtained by him, combined with another recorded measurement of the winter solstice, astronomers have deduced 23° 54′ 2″.5 as the obliquity of the ecliptic in his time. Now, if we substitute 2900 for *t* in the preceding formula, applying to it the negative sign, because it represents the number of years by which the recorded observation has *preceded* the epoch of the formula, we get 23° 51′ 30″ for the value of the obliquity. The near agreement of this result with that derived from the Chinese records is very remarkable, especially when we consider that the instrument with which Tcheou Kong made his observation was a vertical gnomon only eight feet high. The accordance between theory and observation is still more striking when we institute a comparison between the results of the former and the accurate determinations of modern astronomers. Thus Arago and Mathieu, from observations on the solstices made in the years 1812, 1813, 1814, concluded that the obliquity for the year 1813 was 23° 27′ 49″.28. If we compute its value for the same epoch by the above formula, we obtain 23° 27′ 48″.69 as our result. The difference between the observed and computed values is, therefore, only 0″.59.

The displacement of the ecliptic, caused by the action of the planets on the earth, affects the precession of the equinoxes, and the length of the tropical year. This displacement is found to produce a very slow motion of the equinoctial points upon the plane of the ecliptic in the direction of the earth's motion. Its effect is, therefore, contrary to that produced by the action of the sun and moon upon the terrestrial spheroid; but, as it is very minute compared with the latter, it merely occasions a small diminution of the annual quantity of precession. The mean value of precession, as determined by observation for the epoch of 1800, is 50″.22350. The lunisolar precession computed for the same epoch by the theory of gravitation is 50″.37315; hence the effect due to the displacement of the ecliptic amounts to 0″.14965.

The progression of the equinoxes, caused by the action of the planets on the earth, will manifestly affect the length of the tropical year. The action of the sun and moon upon the terrestrial spheroid accelerates the arrival of the earth in the equinoxes, and, therefore, shortens the year; the action of the planets on the other hand retards its arrival, and consequently lengthens the year. The acceleration being, however, greater than the retardation, the tropical year will, on the whole, be shortened by the motion of the equinoctial points. If this motion was uniform, the length of the tropical year would be invariable; but such, in fact, is not the case. At present it is slowly increasing, and on this account the tropical year is gradually becoming shorter. The rate of diminution is about half a second in a century; and consequently the tropical year is now shorter by about ten seconds than it was in the time of Hipparchus.

If the position of the equator was fixed, the variation of the obliquity of the ecliptic would depend entirely on the displacement of the latter plane, occasioned by the action of the planets on the earth. Laplace, however, has shewn that this displacement gives rise to a corresponding oscillation of the equator, which considerably modifies the variation that would otherwise ensue. It is clear, from our remarks on the inequality of nutation in a preceding chapter, that, as the ecliptic approaches the equator, the mean action of the sun upon the terrestrial spheroid will continually vary. This variation of the solar force will give rise to a nutation of the earth's axis, exactly similar to that depending on the variable inclination of the lunar orbit to the equator. In the present instance, however, the period of the inequality will be vastly greater, in consequence of the extreme slowness with which the displacement of the earth's orbit proceeds. The mean disturbing action of the moon upon the terrestrial spheroid will also be subject to a corresponding variation; for the lunar orbit constantly maintains the same angle with respect to the plane of the ecliptic, and, therefore, participates in the displacement of that plane relative to the equator. Hence will arise an oscillatory movement of the earth's axis of the same period with that depending on the action of the sun. Laplace has found that, in virtue of the combined effect of these oscillations, the variation of the obliquity is reduced to about one-fourth of that which would have resulted from the displacement of the ecliptic alone. He has shewn, in fact, that if we only take into account the last-mentioned cause of variation, the obliquity will oscillate to the extent of 4° 53′ 33″ on each side of a mean state; whereas, on the other hand, if the action of the sun and moon on the terrestrial spheroid, arising from the displacement of the ecliptic, be considered, the extent of oscillation will be reduced to 1° 33′ 45″*.

The movements of the equator and the ecliptic being periodical, the phenomena which depend on their relative positions must necessarily be periodical also. Hence the tropical year, although at present in a state of slow diminution, will not always continue so. When the obliquity of the ecliptic has reached the limit prescribed by the action of the planets on the earth, it will then vary in the opposite direction, increasing according to the same law as that which determined its previous diminution. Laplace found that the utmost alteration in the length of the tropical year would not exceed 38.8 seconds. If it were not for the modified action of the sun and moon upon the terrestrial spheroid depending on the displacement of the ecliptic, the extent of variation would be 162 seconds †.

We have already remarked that the earth's aphelion has a progressive motion relative to the stars amounting to 11″ or 12″ annually. If we consider its motion with respect to the equinoxes, we must add the value of precession to the above quantity, which gives about 62″ for the tropical motion. Hence, in little more than twenty thousand years, the major axis of the earth's orbit will have accomplished a complete revolution with respect to the intersection of the equator and ecliptic. Laplace has considered two remarkable epochs in connexion with this circumstance. One of these is that at which the major axis of the terrestrial orbit coincided with the line of the equinoxes; the other is that at which it was perpendicular to the same line. The first of these epochs, when computed by theory, is found to correspond to the year 4107 before the Christian era. This was about a century previous to the creation of man; the solar perigee then coincided with the autumnal equinox. The epoch, at which the major axis of the orbit was perpendicular to the line of the equinoxes, is of much more recent date. Laplace found by computation that it fell in the year 1250 A.D. The solar perigee then coincided with the winter solstice. He therefore proposed that the year 1250 A.D. should be used as a universal epoch, and that the first day of the year should begin with the passage of the sun through the vernal equinox*.

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* Connaissance des Temps, 1827, p. 237.

† Méc. Cél. liv. xiv. chap. i.

An interesting discovery to which Laplace was conducted by his researches on the planets was the existence of an invariable plane in the solar system. It is well known that astronomers generally fix the position of a celestial body by determining its distances with respect to two great circles of the celestial sphere at right angles to each other. If the planes of these circles be immoveable, it is clear that observations, however numerous or distant from each other, may be referred to them, and any changes of position which may have occurred will be discovered by a comparison of the recorded distances from each circle. On the other hand, if the position of the correlative planes be variable, it is equally undeniable that any conclusions drawn from such a comparison will be totally vitiated, at least unless due account be taken of the altered position of the planes at the time of each observation. Now as the orbits of all the planets are continually shifting their positions, in consequence of the mutual attraction of the several bodies, it is impossible to use any of their planes as the plane of one of the great circles of reference; and the same may be said of the plane of the equator, the position of which is continually varying from the action of the sun and moon upon the terrestrial spheroid. It becomes, therefore, highly desirable to discover some plane, the position of which shall be independent of the mutual perturbations of the planets, and which may, therefore, be used as a common ground for comparing distant observations†. This important object was effected by Laplace, who ascertained that, amid the disturbances to which the planets are continually exposed by their mutual attraction, there exists an invariable plane, about which the orbits perpetually oscillate, deviating from it only to a very small extent on either side. This plane passes through the centre of gravity of the solar system, and it is so situated, that if all the planets be projected on it, and if the mass of each planet be multiplied into the area, corresponding to any given time which is described by the projected radius vector, the sum of such products will be a maximum. By means of this property, which is independent of any particular epoch, it will be easy for astronomers in future ages to determine the exact position of the plane, and to compare observations together by means of it‡.

This plane is not peculiar to the solar system. It exists in all those systems wherein the bodies are not exposed to any other forces than those arising from their mutual attraction. Its position in the solar system has been calculated, by referring it to the plane of the ecliptic, corresponding to the year 1750. In this manner it is found that the inclination of the plane is 1° 35′ 31″, and the longitude of the ascending node 102° 57′ 30″. If a similar calculation be made for the year 1950, the inclination will be 1° 35′ 31″, and the longitude of the node 102° 57′ 15″. The agreement of these results is very surprising when we take into account the uncertainty that prevails respecting the masses of several of the planets§.

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* Méc. Cél., liv. vi. chap. x.

† Of course the relation between the two planes determines the position of the other plane.

‡ Journal de l' Ecole Polytechnique, Année, 1798; see also Méc. Cél., liv. ii. chap. vii

We shall now proceed to give some account of the researches of astronomers on the motions of comets. These bodies were generally considered to be meteoric substances, generated in the upper regions of the atmosphere, until Tycho Brahé demonstrated that they were situated beyond the moon's orbit. That astronomer erred, however, in supposing them to revolve in circular orbits, nor did Kepler approach nearer the truth when he affirmed that they moved in straight lines. Hevelius first remarked that the paths of comets are curved near the perihelion, the concave side being turned towards the sun*. He even threw out the suggestion that the form of the curve might be a parabola; but he did not assert that the sun would occupy the focus. Borelli also about the same time hinted that the orbits of comets might be either parabolic or elliptic. Dörfel, a native of Upper Saxony, was the first person who proved that comets move in parabolas, having the sun in the focus. This he did, in 1681, by means of a careful discussion of the observations on the great comet which appeared in the preceding year. It is right, however, to state that he arrived at this conclusion merely by a graphic process, and that he makes no mention of any fixed law regulating the motions of comets.

Newton, having been already assured by the researches of astronomers that comets traverse the regions of the planets, was led, by his discovery of the principle of gravitation, to suppose that they move in conic sections round the sun in the focus. He was of opinion that the orbits are really elliptic, like those of the planets; but he remarked that on account of their great eccentricity they might be assumed, without any appreciable error, to be parabolas near the perihelia. He demonstrated, by a comparison of his theory with the observations of astronomers, that the comet of 1680, and several other bodies of the same class, revolved in orbits which were sensibly parabolic, and that the radii vectores drawn to them from the sun, supposed in the focus, described in each case equal areas in equal times. He also invented a method for determining the elements of a comet's orbit by means of three distinct observations. This method, being founded on the supposition of the body moving in a parabola, did not extend to the determination of the major axis when the comet revolved in an elliptic orbit. Newton, however, remarked that this element and the time of revolution might be derived from a comparison of those comets which returned in the same orbits after very long periods†.

The researches of Newton were soon enriched by a brilliant corollary. Halley, having collected all the recorded observations on comets which could be entitled to any credit for accuracy, proceeded to calculate the elements of their orbits, in hopes of thereby detecting the reappearance of some of the bodies. This illustrious astronomer, having devised an arithmetical calculus for Newton's method of investigation, succeeded, after incredible labour, in computing the elements of twenty-four comets. Among these there were three which, on account of the close resemblance of their orbits and the uniformity of their appearances, afforded strong grounds for suspicion that they were the same comet. The first was observed by Appian in 1531; the second by Kepler in 1607; and the third by Halley himself in 1682. His suspicion of their identity was further confirmed by historical records of the appearance of a comet in 1305, of another in 1380, and of a third in 1456. In all these cases the periods seemed to vary alternately from 75 to 76 years, every two successive revolutions occupying about 151 years. The only objection which offered itself to the opinion of their being the same comet arose from the irregularities of the periods, and the difference in the inclinations of the orbits. The interval comprised between the comets of 1531 and 1607 amounted to 76 years, 62 days; that comprised between the comets of 1607 and 1682 was equal only to 75 years, less 42 days. The difference between the two intervals was therefore more than fifteen months. The inclination of the orbit was also found to vary nearly to the extent of a degree on the occasion of each successive appearance of the comet. These discordances would have offered an insuperable difficulty to many inquirers in attempting to establish the identity of the comets; but Halley was too well acquainted with the principles of the Newtonian theory not to perceive that they might be occasioned by the disturbing action of the planets. He remarked that the period of the planet Saturn sometimes varied to the extent of 13 days from the action of Jupiter, and that, under certain circumstances, the alteration of the time of revolution might even amount to a month. "How much more liable to derangement then," says he, "is a comet whose excursion into space is four times greater than that of Saturn, and whose orbit is so eccentric, that, if the velocity were increased by $\frac{1}{120}$th part of its value, the ellipse described by the comet would be changed into a parabola."

He remarked, that in the summer of 1681, when the comet was approaching its perihelion, it passed so close to Jupiter that the force exerted by that planet on it amounted to $\frac{1}{50}$th of the sun's force; and, as it continued for several months exposed in this manner to the powerful influence of the planet, he inferred that the periodic time must have been in consequence affected to a very considerable extent. The question therefore which now suggested itself to him was to ascertain the precise time when the comet should again return. The period of revolution, as determined by the appearances of 1531 and 1607, would assign the month of November, 1758, for its next return; on the other hand, if the period derived from the appearances of 1607 and 1682 were adopted, the comet ought to return in the month of August, 1757. No means existed in Halley's time of computing the derangement of the comet's motion caused by the planets; but he very sagaciously concluded that the action of Jupiter would have the effect of retarding its arrival, and that in consequence it would not be visible before the end of 1758, or the beginning of 1759. "Wherefore," says this illustrious astronomer, "if it should return according to our prediction about the year 1758, impartial posterity will not refuse to acknowledge that this was first discovered by an Englishman."*

As the time fixed by Halley for the return of the comet drew nigh, an intense interest was awakened in the minds of astronomers by the expected event, but no one had the courage to calculate the precise time when the comet should be visible by taking into account the perturbing action of the planets. At length, in 1757, Clairaut, who had already distinguished himself by his researches in physical astronomy, undertook the examination of this difficult question. By applying his solution of the problem of three bodies, he computed the derangements caused by the planets Jupiter and Saturn throughout three successive revolutions. In the immense calculations to which these researches gave rise, he received efficient aid from Lalande, who was then just entering upon his long career. He finally arrived at the conclusion that the comet would be retarded 100 days by the action of Saturn, and 518 days by the action of Jupiter. Hence the whole period of revolution would extend to 76 years, 211 days, and as the comet had passed its perihelion on the 14th of September, 1682, it ought to return to it again on the 13th of April, 1759. Clairaut announced the result of his labours to the Academy of Sciences on the 14th of November, 1758. He took the precaution, however, of stating that the omission of many small quantities, which was rendered necessary by the method of approximation he employed, might cause the actual time of the comet's arrival in perihelion to differ as much as a month from the computed time. He also added "that a body which passes into regions so remote, and which is hidden from our view during such long periods, might be exposed to the influences of forces totally unknown, such as the action of other comets, or *even of some planet too far removed from the sun to be ever perceived.*"

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* "Quocirca si secundum predicta nostra redierit iterum circa annum 1758, hoc primum ab homine Anglo inventum fuisse non inficiabitur æqua posteritas." Synopsis Astronomiæ Cometicæ. This important little treatise was first published in the volume of the Philosophical Transactions for the year 1705. It was afterwards considerably enlarged by the author, but it was not published in this form until 1749, when it appeared along with his tables of the planets. The words above cited are not contained in the earlier edition.

All the astronomers of Europe were now looking forward with anxious expectation to an event which was destined to exercise so important an influence on the fate of the Newtonian theory. At length the comet was seen for the first time on the 25th of December, 1758, by George Palitsch, a native of Saxony, and an amateur astronomer. It reached its perihelion on the 13th of March, just one month earlier than the time fixed by Clairaut, but still within the limit assigned by that illustrious geometer.

Having revised his labours, Clairaut reduced the error in the computed time of the comet to 22 days, and at a subsequent period he reduced it to 19 days. He would have approached still nearer the truth if he had been in possession of more accurate values of the masses of Jupiter and Saturn.

The return of this comet so near the predicted time was one of the most brilliant triumphs which the Newtonian theory had yet achieved. It established beyond all doubt that comets, while mainly controlled by the preponderating influence of the sun, were also liable to be deranged in their motions by the other bodies of the system, and the theory of their perturbations henceforth formed a subject of deep importance to geometers. Clairaut employed in his researches the method of approximation he had previously devised for the lunar theory, but he introduced into it several ingenious modifications, in order to adapt it to the peculiar difficulties of cometary perturbation. It will readily be perceived that the same facilities do not exist in this case for calculating the influence of a disturbing body as when the question relates to a planet. In the latter case the eccentricity and inclination of the disturbed body being small fractions, it is always possible to expand the perturbing function into a rapidly converging series, arranged according to ascending powers of these quantities; but, as the orbits of comets are very eccentric, and inclined at all angles to the elliptic, the series into which the perturbing function is developed converges with such slowness as to render the usual method of integration quite impracticable. Little progress was made in these researches until the year 1780, when the Academy of Sciences of Paris having proposed to geometers the theory of cometary perturbation, Lagrange composed an admirable memoir on the subject, which obtained for him the prize of the Academy. The method which that illustrious geometer invented for the purpose of obviating the peculiar difficulties of the problem is at once ingenious and effective, and has formed the basis of all subsequent researches relative to the same object. It is founded on the theory of the variation of arbitrary constants; but in integrating the differential expressions of the comet's elements the usual mode of procedure is not adhered to. In the lunar and planetary theories the integrations are so effected, that they conduct to formulæ by means of which the place of the disturbed body may be readily calculated for any assignable instant whether past or future. A similar course not being practicable in the theory of comets, on account of the peculiarities to which we have already referred, Lagrange proposed to substitute instead of it what has been termed the method of quadratures. This consists in dividing the orbit of the comet into a number of distinct arcs, and then summing up the effect of perturbation for each arc. By this process a fresh set of elements is obtained at the end of each summation, and these form the basis of computation for the following arc. The application of this process to the whole of the orbit would demand an enormous amount of calculation, but Lagrange shewed that such a course was not necessary. When the comet is near the perihelion, the method of quadratures is indispensable; but, when it is traversing the superior part of its orbit, the ordinary method of integration may be practised on account of the great distance of the comet compared with the distance of the disturbing body.

In 1770 a comet appeared, the circumstances connected with which led to very interesting results. The observations on it seemed inexplicable by any parabolic orbit that could be devised, until Lexell finally shewed that they might be all reconciled with the supposition of the comet revolving in an ellipse in a period of nearly six years. The opinion of Lexell was confirmed in the most satisfactory manner by Burchardt, who for this purpose undertook a careful discussion of all the observations. Strange to say, the comet never afterwards appeared, notwithstanding the shortness of its period. In order to account for this fact, Lexell remarked that it had been always invisible until the year 1770, but that in 1767 it passed so near Jupiter that it was thrown by the powerful disturbance of that planet into a new orbit and thereby rendered *visible;* and that in 1779 it again passed so near the planet as to be thrown into another orbit and rendered *invisible.* Laplace undertook an analytical investigation of this interesting question, and he found that the disturbing action of Jupiter would be capable of producing the singular effects ascribed to it by Lexell *. No doubt, therefore, can exist on the cause either of the appearance or the subsequent disappearance of the comet.

As this comet approached very near the earth, it offered a favourable example for ascertaining to what extent comets in general affect the other bodies of the system. The result of its action on the earth would be to diminish the force of that body towards the sun, and thereby to lengthen the sidereal year. No such change has been detected by astronomers, whence we may conclude that the masses of comets are very small. Laplace found that, if the sidereal year had increased 1^s since the year 1770, the mass of the comet would not have exceeded $\frac{1}{5000}$th part of the earth's mass. It is evident, however, that the mass is much less than this, for, although the comet passed through the system of Jupiter's satellites both in 1767 and in 1779, it did not produce the slightest perceptible derangements in the motions of any of those bodies.

Laplace, after having investigated with the most brilliant success every subject connected with the system of the world, finally conceived the design of uniting in one great work all the discoveries that had been effected in Physical Astronomy. This design appears realised in the *Mécanique Céleste*, one of the most stupendous monuments of the human intellect which modern civilization can boast of. It consists of five quarto volumes, which were published at different times in the course of the author's life. The first and second volumes appeared in 1799; the third in 1803; the fourth in 1805; and the fifth in 1825. The whole work is divided into sixteen books. Ten of these occupy the first four volumes; the remaining six contained in the last volume may be considered as supplementary to the others. The first book of this immortal production treats of the laws of equilibrium and motion; the second, of the law of universal gravitation and of the centres of gravity of bodies; the third, of the figures of the celestial bodies; the fourth, of the oscillations of the sea and the atmosphere; the fifth, of the motions of the celestial bodies about their centres of gravity; the sixth, of the theory of the planets; the seventh, of the lunar theory; the eighth, of the theory of the satellites of Jupiter, Saturn, and Uranus; the ninth, of the theory of comets; the tenth, of the theory of refraction and other points relative to the system of the world. In the first book of the fifth volume or the eleventh of the whole work, Laplace considers the figure and rotation of the earth. The twelfth book treats of the attraction and repulsion of spheres and of the laws of the equilibrium and motion of elastic fluids; the thirteenth, of the oscillations of the fluids which cover the surfaces of the planets; the fourteenth, of the motions of the celestial bodies about their centres of gravity; the fifteenth, of the motions of the planets and comets; the sixteenth, of the motions of the satellites. Besides the five volumes above mentioned, Laplace composed at different times supplements to several of the books.

The physical theory of the planetary system is exhibited in the *Mécanique Céleste* in a state of almost complete developement. No material progress has in consequence been effected in this branch of astronomy since the publication of that immortal work. One discovery of a very remarkable character has indeed been recently added to the long list of triumphs which adorn its history; but during the present century geometers have been mainly occupied in correcting and extending previous results, in improving the methods of investigation, and in illustrating the more obscure points of the theory.

In reviewing the progress of Physical Astronomy since the close of Newton's career, it is impossible not to be struck with the truth of the remark, that great occasions always call forth from the bosom of society suitable minds to cope with the emergencies of the times, and to triumph over opposing difficulties. It has been said that Newton appeared on the theatre of the world when the materials of the magnificent structure erected by him had been already amassed by the persevering industry of preceding ages. The true state of the planetary system had been unfolded by the successive labours of Hipparchus, Copernicus, and Kepler; the laws of motion had been fully established by Galileo, and his successors Huygens, Wallis, and Wren; nay, the all-pervading principle which animates and controls the bodies of the universe had been dimly surmised by more than one philosopher. There was still, however, wanting some master-spirit to detect the mutual dependence of these disjointed principles, and by a mighty effort of generalization to reduce all the phenomena of the heavens under one dominant law of nature. The prospect of achieving this grand result was the alluring motive by which genius was invited to the study of celestial physics in the seventeenth century; and to the English philosopher was assigned the immortal honour of its realisation. We may discern a similar adaptation of intellectual power to existing exigencies in the period which elapsed between the publication of the *Principia* and the appearance of the *Mécanique Céleste.* Newton had fully established the principle of gravitation by his own unaided efforts; but he bequeathed a vast heritage of profound research to his successors. With a sagacity unexampled in the history of the human mind, he detected the agency of this principle in all the grand phenomena of the planetary system, and by the aid of a sublime geometry of his own invention he succeeded in reducing to calculation a number of its most hidden results. It still remained, however, for geometers to ascertain the effects of the minute perturbations which ensue from the mutual action of the planets, to invent formulæ enabling the astronomer to determine their positions throughout all ages, both past and future; and, finally, to solve the momentous question whether the planets are gradually being absorbed into the sun, or whether the system is so constituted that they will revolve in permanent orbits round the central body. Such were the magnificent problems which Newton's discoveries suggested to his successors. We have seen what mighty energies were awakened by these problems and with what brilliant success their solution was effected. Perhaps no period of history can exhibit an array of mathematical genius, equal to that which adorned the eighteenth century. The labours of Euler, D'Alembert, Clairaut, Lagrange, and Laplace will fill many a bright page in the annals of science, and their names will be for ever associated with that of the illustrious founder of Physical Astronomy, whose reputation they have so much enhanced by their sublime discoveries.

Among the various circumstances which are calculated to excite our admiration, while reviewing this portion of the history of Physical Astronomy, not the least remarkable are the resources of the transcendental analysis; by means of which the geometer has been enabled to unravel the most complicated relations of the system of the world, and to decipher in the anomalous movements of the celestial bodies the constant operation of one all-pervading principle. In vain would the human mind have ever attempted to penetrate into the more recondite parts of the theory of gravitation without the aid of this powerful instrument of research. Its assiduous cultivation was, therefore, essentially necessary for the developement of that theory; and, on this account, the pure analysts, such as Leibnitz and the two eldest Bernoullis, deserve to be associated with those who have more directly contributed to the progress of the science. "The discovery of the system of the world by Newton," says Delambre, "was a fortunate event for geometers. Never could the transcendental analysis find a worthier or a more sublime theme. Whatever progress is made in it, the original discoverer will always maintain his rank. Lagrange, who often asserted Newton to be the greatest genius that ever existed, used to remark also—'and the most fortunate; we do not find more than once a system of the world to establish.' It has required a hundred years of labours and discoveries to construct the edifice of which Newton laid the foundation; but all is ascribed to him, and he is supposed to have pursued the whole extent of the career on which he entered with an éclat so well calculated to encourage his successors."*

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* Mémoires de l'Institut, 1812, p. xlv. (*Notice sur la vie et les ouvrages de Lagrange.*)

An illustrious living philosopher of France*, when alluding to the discovery of universal gravitation, has said, that no Frenchman can reflect without an aching heart on the small participation of his own country in that memorable achievement. If an Englishman could be supposed to be equally sensitive, he has ample reason to regret the inglorious part his country played during the long period which marked the developement of the Newtonian theory. With the exception of Maclaurin and Thomas Simpson (the former of whom certainly contributed towards the solution of one of the great problems of the system of the world, and the latter at least gave ample proofs of his capacity for such researches), hardly any individual of these islands deserves even to be mentioned in connexion with the history of physical astronomy during that period. This deplorable fact has been generally attributed to the pertinacity with which the English mathematicians adhered to the synthetic method of investigation, the resources of which had been already completely exhausted by Newton; and also to their perseverance in employing the fluxional calculus of that great geometer; which, besides being less commodious in point of notation, did not at any time attain the high state of perfection which, at a comparatively early stage of its history, distinguished the rival invention of Leibnitz. The feeling of veneration which they naturally cherished towards their illustrious countryman was doubtless the main cause of their injudicious attachment to his peculiar methods of research;. but it was also fostered in a strong degree by the unhappy quarrel which arose between them and the continental mathematicians relative to the original invention of the infinitesimal calculus. Were it not for the mutual estrangement which then ensued between both parties, it would have been an easy task to transfer the improvements of the differential calculus to the fluxional calculus of Newton, which, in point of fact, was identical with it; and by this means the analysts of England might have advanced at an equal pace with those on the continent. When the unpleasant feeling, just referred to, finally died away, the intellectual energies of England were already directed towards objects diametrically opposed to contemplative science; and the few persons who still cultivated mathematics, perceiving how far the analysts on the continent had advanced beyond them in the improvements of the infinitesimal calculus, appear to have abandoned in despair all intentions of original research in physical astronomy, contenting themselves merely with timid dissertations on the Principia. At the beginning of the present century there was hardly an individual in this country who possessed an intimate acquaintance with the methods of investigation which had conducted the foreign mathematicians to so many sublime results. It is gratifying to reflect that a vigorous attempt has been made since that time to recover for England her due position in the physico-mathematical world. Notwithstanding the disadvantages under which they laboured, the geometers of this country have already given ample proof that it was not from any natural defect of intellectual ability that their fathers were compelled so long to remain silent spectators of the triumphs of their neighbours. The most recondite parts of analysis are now studied with ardour and success by a number of talented persons; and England, in the present day, can boast of some of the most distinguished mathematicians of Europe. The late Professor Woodhouse, of Cambridge, deserves to be mentioned as the person who laboured most zealously in removing from the minds of his countrymen the prejudices they had so long cherished against the analytical methods that were in universal use on the continent. In the sequel we shall have the pleasure of noticing occasionally some of the results which may be considered as the first fruits of this salutary innovation.

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* M. Arago.

CHAPTER X.

Variation of the Mean Distances of the Planets.—Researches of Poisson.—The Theory of Planetary Perturbation resumed by Lagrange and Laplace.—Uniformity of the results arrived at by these Geometers.—The General Theory of the Variation of Arbitrary Constants established by Lagrange.—Researches of Poisson on this subject.—Death of Lagrange.—Researches of Modern Geometers on the Theory of Perturbation.—Method of Hansen.—Developement of the Perturbing Function.—Burchardt.—Binet.—New Methods devised for obtaining the coefficients of the Perturbing Function.—Secular Inequalities of the Planets.—Researches of Le Verrier.—Theory of the Moon.—Irregularities of the Epoch.—Equation of Long Period.—Researches of Damoiseau, Plana, and Carlini.—Lunar Tables calculated by means of the Theory of Gravitation.—Researches of Lubbock and Poisson.—Reduction of the Greenwich Observations.—Discovery of the True Cause of the Irregularities of the Moon's Epoch, by Hansen.—Researches for the purposes of determining the Value of the Moon's Mass.

THE first important discovery which distinguished the progress of physical astronomy in the nineteenth century related to the variation of the mean distances of the planets from the sun. We have already given an account of the researches of Lagrange on this point of the planetary theory, and have mentioned the remarkable conclusion at which he arrived. It appeared that the mean distance of a planet, when disturbed in its elliptic orbit by the other bodies of the system, was not subject to any variations which increased constantly with the time, but was merely affected by a series of periodic inequalities depending on the relative places of the planets in their orbits. Lagrange shewed that this theorem was true for all powers of the eccentricities and inclinations; but his investigation did not extend beyond a first approximation, and, therefore, was limited to terms of the first order with respect to the disturbing forces*. The interesting question, therefore, still remained to be examined, whether a repetition of the approximation would introduce into the expression of the mean distance any terms increasing with the time, and thereby occasioning a secular inequality in the mean motion. This problem was first attacked by a young geometer, who was destined to pursue a brilliant career in the physico-mathematical sciences. In the year 1808, Poisson, who was then only twenty-five years of age, communicated a memoir to the Institute, in which he investigated the variation of the mean distance, carrying the approximation to the square of the perturbing forces. By means of a most elaborate analysis, he succeeded in shewing that the repetition of the approximation would introduce into the formula for the variation of that element, only a class of terms depending on sines and cosines of angles, increasing proportionally with the time. It followed, then, that so far as the mean distances were concerned, the mean motions of the planets were invariable by the second as well as by the first approximation.

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* *Mémoire sur les Inégalités Seculaires des Moyens Mouvemens des Planètes.* This memoir was read at the Institute, in June 1808, and was subsequently published in the eighth volume of the *Journal de l'Ecole Polytechnique.*

The interesting result to which Poisson was conducted by his researches, had the effect of again inducing Lagrange and Laplace to direct their attention to the theory of planetary perturbation. Poisson, after the example of these geometers, had assumed, as the basis of his investigation, the elliptic equations of the undisturbed orbit, and obtained the variations of the elements, by supposing the constants in these equations to be every instant changing their values by infinitely small quantities. Lagrange, however, was led, by reflecting on Poisson's researches, to consider the subject under a more general aspect. He assumed as the basis of his investigation the three differential equations of the planet's motion, derived from the supposition that it was exposed only to the central action of the sun. Each of these equations being of the second order, its complete integral would contain two arbitrary constants, and, therefore, the three equations which determine the place of the planet in its orbit would contain six arbitrary constants*. When the action of the planets is taken into account, another quantity is introduced into the differential equations, termed the perturbing function. Conceiving, then, this function to arise from a continuous variation of the six constants in the primitive equations, Lagrange succeeded in obtaining the expressions for the variation of each constant, without assuming the ellipticity of these equations. The variations, when investigated by this uniform process, exhibited a very remarkable form, being all expressed by partial differentials of the perturbing function, taken with respect to the constants, and multiplied into quantities, which were functions only of the constants. This form presented a great advantage in practice, for, as it was possible to expand the perturbing function into a series of sines and cosines of angles, increasing proportionally with the time, the secular variations of the elements were at once obtained by means of the terms that were explicitly independent of the time, while the remaining terms being integrated, and then substituted in the formulæ for finding the place of a planet in an elliptic orbit, gave the longitude and latitude of the planet in the disturbed orbit corresponding to any assignable time. Lagrange then applied his analysis to the question of the invariability of the mean distance. Poisson had not considered the effect of the variations of the elements of the disturbing planets, on account of the analytical difficulties offered by the perturbing function, which did not preserve the same form throughout the investigation when that supposition was introduced. Lagrange got rid of this inconvenience by transferring the origin of co-ordinates from the centre of the sun to the centre of gravity of the sun and planets. The perturbing function being then symmetrical with respect to all the planets, the same analysis was applicable, whatever might be the planet whose elements were supposed to vary. Lagrange by this means succeeded in shewing that the second approximation would not introduce into the expression for the mean distance any term which increased constantly with the time, even when the variations of the elements of the disturbing planets were included in the investigation.

It is right to mention that the variations of most of the elements had already appeared in the form under which Lagrange just presented them. That great geometer had so expressed the variation of the mean distance as early as 1776, and Laplace subsequently obtained similar expressions relative to the eccentricity, the inclination, and the longitude of the node*. The importance of Lagrange's researches on the present occasion was, however, of a twofold character, for, besides exhibiting the variations of all the elements under the same form, the direct analysis by which he obtained his results, and the general aspect under which he viewed the elements (considering them merely as the six arbitrary constants involved in the complete integrals of the differential equations, without reference to their individual functions in an elliptic orbit), seemed to indicate a general theory, of which the variations of the elements of the planetary orbits served only as a particular illustration.

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* Mémoires de l'Institut, 1808.

On the very day that Lagrange communicated to Laplace the interesting result of his researches on the variations of the elements, the latter exhibited to him analogous expressions, to which he had just been conducted by an investigation of the same subject. Such was the remarkable parity of inventive power which distinguished the rival efforts of these illustrious geometers, and which so long held the judgment of Europe in suspense upon the question of their relative merits!

Notwithstanding the identity of the results obtained by Lagrange and Laplace on the present occasion, it was not difficult to discern in the researches of each geometer the peculiar bent of his genius. Lagrange, who wielded the powers of the transcendental analysis with unrivalled effect, considered the subject in its most abstract form, and by this means imparted a generality to his researches, which could not fail to lead to important results. Laplace, always less enamoured with the beauty of mathematical speculation than he was anxious to unfold the system of the world, contented himself with reducing the expressions for the variations of the epoch and perihelion to the same form with those relating to the other elements, not considering them in connexion with any general principle, but simply deriving them from established formulæ. In effecting this object, he was considerably aided by a relation between the two elements which Poisson had recently discovered. These results appeared in a supplement to the third volume of the *Mécanique Céleste*†. Laplace exhibited a beautiful illustration of their practical utility, by employing them in the investigation of the lunar inequalities occasioned by the spheroidal figure of the earth. The expressions which he obtained by this process for the inequalities both in longitude and latitude coincided exactly, in all respects, with those he had already arrived at by the usual method of approximation. It is exceedingly interesting to trace the final agreement of two methods differing so essentially in their original conception.

Soon after Lagrange and Laplace arrived at the above-mentioned results, the former of these geometers communicated a memoir to the Institute, in which he exhibited the application of the theory of the variation of arbitrary constants to all questions of mechanical science‡. Assuming as the basis of his researches the three differential equations of the second order to which he had already shewn, in the *Mécanique Analytique*, that the motion of every system of bodies might be reduced, he supposed the six arbitrary constants of the complete integrals to vary from the effect of a small disturbing force and to be represented in the differential equations by a small quantity termed the perturbing function. He then demonstrated, by a process similar to that which he employed in the less general case of a planet revolving round the sun, that the expression for the instantaneous variation of each constant contained only terms involving partial differentials of the perturbing function relative to the constants, each differential being multiplied by a function only of the constants.

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* Méc. Cél., liv. ii. chap. viii.

† Laplace first announced them at the sitting of the *Bureau des Longitudes*, 17th of August, 1808.

‡ Mémoires de l'Institut, P. 257, et seq.

In this memoir Lagrange remarked that the same principles were applicable to the determination of the motions of the planets around their centres of gravity, taking into account the action of the disturbing planets upon the equatorial matter of the disturbed planet. Having exhibited a statement of the general conditions of the problem and pointed out its connexion with the theory of the variation of elements, he concluded by announcing his intention of making it the subject of a future memoir. At no subsequent period of his life did he carry into effect this resolution; but, indeed, Poisson a few months afterwards rendered such a step unnecessary, by communicating to the Institute an excellent memoir on the subject. Euler first gave the differential equations relative to the motion of a body about its centre of gravity when it has received an initial impulse and is not afterwards exposed to the action of any forces. These equations being three in number, and of the second order, their complete integrals will contain six arbitrary constants. Their integration would therefore lead to the equations of a planet's motion about its centre of gravity if it were not continually disturbed by the action of the sun and the other planets of the system. By considering the latter, however, as so many perturbing bodies which tend to produce a continual change in the elements of rotation, the variations of these elements may be investigated by a process similar to that employed in determining the variations of the elements of the orbit. The formulæ for these variations being then integrated and substituted in the primitive equations depending solely on the initial impulse will conduct to three complete equations, by means of which the positions of the planet's axis and the velocity of rotation corresponding to any assignable time may be readily ascertained. By a judicious selection of constants, Poisson obtained formulæ for the variations of the elements of rotation exactly analogous to those which Lagrange and Laplace had already arrived at relative to the orbit of the planet. This remarkable result constituted the crowning triumph of the theory of the variation of elements, for by means of it the two great problems of the system of the world, although differing essentially in conception, were most unexpectedly exhibited in a relation of close affinity, and the geometer was thus enabled to contemplate all the effects of planetary perturbation through the medium of the same analysis *.

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* *Mémoire sur la variation des constantes arbitraires dans les questions de Mécanique, Journal de l'Ecole Polytechnique*, tome viii. p. 266, et seq. In this memoir Poisson investigates the expressions for the variations of the arbitrary constants in the general problem of Mechanics by a more direct analysis than that which Lagrange had employed in his original demonstration of the same results. It is right, however, to state that the last-mentioned geometer was conducted about the same time to a similar improvement of his own method, (Mémoires de l'Institut, 1809, p. 343, et seq). A complete account of the theory of the variation of arbitrary constants by means of Poisson's analysis will be found in Pontécoulant's *Théorie Analytique du Système du Monde*, tome i. liv. ii. chap. iii. See also on this subject the seventeenth chapter of Mr. De Morgan's *Treatise on the Differential and Integral Calculus*, Library of Useful Knowledge. This chapter contains a concise, but singularly able exposition of the most comprehensive theories of mechanical science. The reader is conducted by means of them to the very threshold of the great problems of the system of the world.

The complete establishment of this sublime theory was the last great effort of Lagrange's genius. We have already remarked that Euler's researches contain the earliest traces of the use of the variation of elements in computing the perturbations of the planetary motions. Although that geometer, however, has the undoubted merit of first employing this fertile method of investigation, the magnificent expansion which it subsequently received, and to which it owes all its practical value, is wholly due to the illustrious mathematician above mentioned*. In Physical Astronomy, it is indeed very naturally suggested by observation; for the elements of the planetary orbits had been already found by astronomers to be in a state of slow variation, although their elliptic forms did not appear to be undergoing any change. It is in questions of this kind, where the perturbing forces are small compared with the principal forces which animate the system, that the variation of elements can be most advantageously employed. Lagrange resolved to embody his final researches on this subject in a new edition of the *Mécanique Analytique;* and with this view he undertook a complete revisal of that immortal work; but the fatigue he incurred in consequence brought on a fever which carried him off before the termination of his labours, on the 10th April, 1813.

Lagrange deserves to be ranked among the greatest mathematical geniuses of ancient or modern times. In this respect he is worthy of a place with Archimedes or Newton, although he was far from possessing the sagacity in physical enquiries which distinguished these illustrious sages. From the very outset of his career he assumed a commanding position among the mathematicians of the age, and during the course of nearly half a century previous to his death, he continued to divide with Laplace the homage due to pre-eminence in the empire of the exact sciences. His great rival survived him fourteen years, during which he reigned alone as the prince of mathematicians and theoretical astronomers.

In recent times the general theory of planetary perturbation has derived much improvement from the profound researches of Poisson, Plana, Ivory, Lubbock, and other geometers. The most remarkable innovation is due, however, to M. Hansen. That distinguished geometer conceives the elliptic elements of the planet to be invariable, and assumes the time alone to be subject to perturbation †. The method of investigation founded on this refined notion possesses some important advantages over the ordinary methods employed for the same purpose. M. Hansen has applied it with success to the theory of Jupiter and Saturn, and also in researches on the lunar perturbations.

The derangements in the elliptic motion of a planet, occasioned by the action of another planet on it, depend upon an algebraic expression, involving the mutual distance of the two planets, and termed the perturbing function. Before any progress can be made in computing these perturbations, it is necessary that the algebraic expression on which they depend should be decomposed into individual terms. This object is generally effected by expanding the function into a series of sines and cosines of angles increasing proportionally with the time, and arranged according to ascending powers of the eccentricities and inclinations. If no other operation was necessary, and the inequalities were represented by the terms, simply as they appear in the expanded function, the computation of those that are of sensible magnitudes would not be very troublesome; for, as the eccentricities and inclinations of the planets are generally very inconsiderable, at least if we except the smaller planets, the series converges with great rapidity; and for all purposes of comparison between theory and observation, it would not be necessary to take into account any terms beyond those of the second order with respect to these quantities. But this is by no means the real state of the question. After the perturbing function has been expanded in the form just mentioned, it undergoes two successive integrations before the terms composing it can be made subservient in representing the planet's inequalities in longitude. By this process the circumstances of the problem are totally altered, and its difficulties in a vast degree augmented, for it may happen that a term which is quite insignificant in the original expansion will acquire by double integration a divisor which will render it very considerable *. This divisor is a *multiplier* of the time in the original terms of the expanded function, and it is generally formed by combining together in endless ways, by addition and subtraction, the mean motions of the disturbing and disturbed planets. The geometer, therefore, cannot safely reject any terms in the expanded function from a mere regard to their order in the series, unless he has assured himself at the same time that they cannot be affected to a sensible extent by any of the divisors depending on the various combinations of the mean motions. It is this circumstance which occasions the necessity of calculating the terms of the third and even the fifth orders in the theory of Jupiter and Saturn; and indeed a similar course is indispensable when the question refers to any of those inequalities of long duration which extend over several revolutions of the disturbing and disturbed planets *. As the terms to be taken into account rise in degree the labour of computing the coefficients increases with frightful rapidity. With the view of facilitating the researches of geometers, Burchardt calculated all the terms of the perturbing function to the sixth powers of the eccentricities †. It still remained to execute a similar calculation with respect to the inclinations. This laborious task was performed by Binet, who carried the developement to the seventh powers of the eccentricities and inclinations. This has been found to be amply sufficient for computing the perturbations of the older planets, but it is by no means so, when the question refers to the small planets lying between the orbits of Mars and Jupiter, or when the perturbations of comets are considered. In both these cases the eccentricities and inclinations are so considerable that the terms of the disturbing function converge with extreme slowness; and on this account, it is necessary to calculate a much greater number of them than in the theory of the older planets, in order to attain an equal degree of accuracy. Geometers appear to have abandoned all hopes of determining the coefficients of the higher terms by the ordinary process of algebraic developement, the operation rapidly assuming so complicated a form as to become totally unmanageable. Other methods have accordingly been devised for effecting this object. One of these is founded on the principle of mechanical quadratures ‡. M. Hansen has determined the coefficients of the disturbing function by this method in a memoir on the theory of Jupiter and Saturn which obtained for him the prize of the Academy of Sciences of Berlin.

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* Euler explained the method of the variation of elements in his memoir on the perturbations of the planets, which he transmitted to the Academy of Sciences in 1756; but his investigation was imperfect, because he omitted to take into account the variation of the epoch. The same defect characterized the researches of Lagrange, which appeared in the volume of the Turin Academy for 1763. It was not until he published his famous memoirs on the planetary perturbations in the volumes of the Berlin Academy for 1782-3-4, that the last-mentioned geometer gave a complete theory of the subject by investigating the expressions for the variations of the six elements of the undisturbed orbit.

† Pontécoulant has expounded this method in the *Connaissance des Temps* for 1837.

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* The expression for the longitude contains a term of the form $k\int_{t,}\int_{t}\frac{d.R}{d\epsilon}$ in which R denotes the perturbing function, ϵ the epoch of the disturbed planet, and h a constant quantity. Now R, when expanded, is found to contain a series of terms of the form $P\cos\left((in-i'n')t+\epsilon-i'\epsilon'+Q\right)$, in which n n' denote the mean motions of the disturbed and disturbing planets, ϵ ϵ' the epochs, i i' any integers whatever, P a function of the eccentricities and inclinations of the two planets, and Q a function of the perihelia and the nodes. Hence $k\int_{t}\int_{t}\frac{d.R}{d\epsilon}$ will contain a series of terms of the form $\frac{k\,i}{(in-i'n')^2}P\sin\left((in-i'n')t+\epsilon-i'\epsilon'+Q\right)$, and it is clear that if in any case $i\,n$ be very nearly equal to $i'\,n'$, the corresponding term of the longitude may be considerable, even although P be of a high order relative to the eccentricities and inclinations. For example, in the theory of Jupiter and Saturn, $5\,n$ is very nearly equal to $2\,n'$, and hence arises the famous inequality in the longitudes of those planets which Laplace was the first to trace to its true physical source.

Another method consists in giving particular values to the disturbing function, and then, by means of the equations formed between them and the corresponding values of the series, eliminating as many coefficients as may be deemed desirable. This method has been practised with complete success by M. Le Verrier in computing the perturbations of Uranus by Saturn §.

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* We have mentioned in the preceding note that R contains a series of terms of the form $P\cos\left((in - i'n')t + i\varepsilon - i'\varepsilon' + Q\right)$. Now the lowest terms which involve any given values of i i' are of the order $i - i'$ relative to the eccentricities and inclinations. The next lowest terms having the same arguments would be of the order $i - i' + 2$; and the order of the succeeding term swould increase by 2 at each step. For example, if $i = 5$ and $i' = 2$, then the lowest terms of the form

$$P\cos\left((5n - 2n')t + 5\varepsilon - 2\varepsilon' + Q\right)$$

would be of the third order; the next lowest of the fifth order, and so on. Hence we perceive the reason why the terms upon which the long inequality of Jupiter and Saturn depends, are *at least* of the third order. Similarly in the long inequality of the Earth and Venus, which arises from the smallness of $13n - 8n'$, the terms are at least of the fifth order.

† Mémoires de l'Institut, 1808.

‡ This method, the germ of which may be traced to D'Alembert, forms the subject of two papers by Poisson, which appear in the volumes of the *Connaissance des Temps* for 1825 and 1836. Pontécoulant has explained it in his *Théorie Analytique du Système du Monde*, and has also given an example of its application in computing the great inequality of Jupiter and Saturn. See the work just cited, tome iii. liv. vi. chap. vi. vii. xxi.

§ *Recherches sur le Mouvement de la Planète Herschel, Connaissance des Temps*, 1849. Le Verrier does not give the details of his investigation of the perturbations of Uranus by this method, his object being merely to shew the accordance of its results with those he obtained by the ordinary method of algebraic developement. In a memoir already

We have given some account in a previous chapter of the researches of Lagrange and Laplace on the secular inequalities of the planetary motions, and have also mentioned the results to which they were conducted relative to the stability of the system of the world. Laplace demonstrated that neither the eccentricities nor the inclinations would increase indefinitely with the time, but he did not assign any means of determining the exact limits between which they would perpetually oscillate. It is conceivable, indeed, that both elements might vary to a considerable extent, without invalidating the famous theorems of that geometer relative to their limits. Le Verrier found that a small planet revolving round the sun at twice the mean distance of the earth would be so disturbed by Jupiter and Saturn that the inclinations of its orbit relative to the orbits of those planets would attain considerable magnitudes, even although they were originally very small *. Now it is remarkable that the small planets discovered between the orbits of Mars and Jupiter, whose inclinations we know to be greater than those of the other planets, have all been found to revolve in the neighbourhood of this region. Le Verrier also found a similar region between Mercury and Venus, in which, if a small planet revolved, its inclinations relative to the orbits of the Earth and Venus would experience considerable variations from the disturbing action of those planets.

The only definite conclusion which could be drawn from Laplace's researches, was that the expressions for the eccentricities and inclinations would consist of a series of sines and cosines of angles, increasing with the time, but would not contain any term involving the time, *without* the functional symbols. It still remained to compute the numerical values of the constants entering into these terms, in order to ascertain the values of the elements corresponding to any time, past or future, and to assign the exact limits within which they would perpetually oscillate. This operation, however, was one of extreme difficulty, for it involved the resolution of an algebraic equation, equal in degree to the number of planets whose mutual action was considered, and demanded also a most laborious process of elimination. Lagrange made the first successful attack on this problem, by the aid of an ingenious simplification, which consisted in grouping the planets into two systems, one composed of Jupiter and Saturn, to which he subsequently added Uranus; and the other composed of Mercury, Venus, the Earth, and Mars, taking also into account the action of the larger planets upon each of these bodies. By this process he found that the eccentricities and inclinations would continually oscillate between very narrow limits, and he assigned the numerical values of the limits for each planet. This investigation of Lagrange's, although valuable as a first attempt to establish an important point in the system of the world, was considerably vitiated by the erroneous values he assigned to the masses of the smaller planets, especially Venus, the mass of which he estimated at a half more than its true value. In more recent times, when the masses of the planets and the elements of their motions came to be ascertained with greater accuracy, a strong desire was felt that the

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des Planètes, No. 1) he explained the ingenious process by which he eliminated the coefficients of the disturbing function. A translation of this memoir is given in *Taylor's Scientific Memoirs*, part xviii. See also on this subject a paper by Sir John Lubbock, in the Philosophical Magazine for August, 1848.

* *Mémoire sur les Mouvements des Inclinaisons et des Nœuds des trois Planètes.* In

question should be submitted to a fresh investigation. This task was undertaken by Le Verrier, who took into account the simultaneous action of the seven planets*. This excellent geometer was conducted by his researches to some very interesting conclusions. He found that after the lapse of a few hundred years, Lagrange's formulæ for the elements would become inaccurate; but it is remarkable that the superior limits of the eccentricities as assigned by that geometer did not differ materially from those obtained by Le Verrier. This circumstance depends upon a curious relation which Le Verrier found to connect the variation of the limit, with supposed errors in the masses of the disturbing planets. He discovered, in fact, that the limit would vary only to a very slight extent, even although considerable errors were committed in the estimation of the masses. This theorem is true with respect to all the planets, except the Earth and Venus. The coincidence in these two instances depends upon the particular value which Lagrange assigned to the mass of Venus. By employing any other value of the mass, he would have obtained very different values for the limits of the eccentricities of the two planets †.

A remarkable period which Le Verrier has considered in connexion with his researches on the secular variations, is that which would restore the eccentricities and perihelia of the three superior planets, Jupiter, Saturn, and Uranus, to the same mutual relations. When the slowness with which these elements vary, is taken into account, one might reasonably suppose that many millions of years would elapse before such a restoration could take place. Le Verrier found, however, that a period of nine hundred thousand years would suffice for this purpose, although the elements of the planets will have passed through all their values only a small number of times in that interval, and the elements of Uranus will have completed only one cycle of their values. He considers that the error of this period does not exceed four thousand years, or $\frac{1}{225}$th of its computed value.

The researches on the secular variations of the planets had hitherto been confined to the first powers of the eccentricities and inclinations. As these elements vary with extreme slowness, it was supposed that the effects of the superior powers would be very insignificant, and that their

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* Pentécoulant had calculated the expressions for the elements of the seven planets, and published his researches in the third volume of his *Théorie Analytique du Système du Monde*, but Le Verrier, upon comparing them with his own results, found them to be totally erroneous. For further particulars in connexion with this circumstance, see the *Comptes Rendus*, tome ix. p. 550, tome x. pp. 539, 739, tome xi. pp. 872, 881; see also the *Connaissance des Temps* for 1843, *Additions*, p. 24.

† The following are the superior limits of the eccentricities of the six older planets, as assigned respectively by Lagrange and Le Verrier.

	Lagrange.	Le Verrier.
Mercury	0.22208	0.22565
Venus	0.08271	0.08672
The Earth	0.07641	0.07775
Mars	0.14726	0.14224
Jupiter	0.06036	0.06155
Saturn	0.08408	0.08492

It would be difficult to account for the near agreement of these results, except by the theorem alluded to in the text, relative to the errors in the values of the masses. Le Verrier has fixed the minimum value of the Earth's eccentricity at 0.003314; whence it appears that her orbit will never attain a circular form, as some persons have imagined.

computation would not alter any of the conclusions at which geometers had arrived relative to the stability of the planetary system. Le Verrier, in a second memoir on the secular variations of the planets, investigated this subject by carrying the approximation to the third powers of the eccentricities and inclinations *. The result of his researches was in some respects contrary to what had been anticipated. He discovered that the terms of the third order produced effects which very soon became sensible, and therefore could not be neglected. With respect to the stability of the system, he found that when the planets Jupiter, Saturn, and Uranus were considered, the introduction of the terms of the third order had the effect of confirming the results of previous researches on the subject; but when the question referred to the four smaller planets, it was impossible to arrive at any definite conclusion, on account of the uncertainty that existed respecting the masses of those bodies.

The theory of the moon has in all ages occasioned much trouble to astronomers. Some of the irregularities in the motion of that body are of such magnitude as to force themselves upon the notice of the observer, even in a very rude state of astronomy, and a strong desire has in consequence been always felt to ascertain their real character. In more recent times the advantage of a knowledge of the moon's motion in promoting the purposes of geography and navigation, and in affording a ready means of testing the theory of gravitation, has imparted an unusual degree of interest to the study of her various inequalities. Towards the close of the eighteenth century the most esteemed tables of the moon were those of Mayer, revised in 1780 by Mason. In 1798 the French Institute, desirous of obtaining corrections of the elements of her motion, proposed as the subject of a prize, the determination of the mean places of the apogee and ascending node of the lunar orbit, by means of at least 500 observations. This prize was awarded to Burg, an astronomer of Vienna, who employed in his calculations as many as 3232 observations. Tables were then constructed by him upon the basis of the corrected elements. The arguments of the equations were derived from Laplace's theory, but the coefficients were determined by observation. In one instance only were so few as 668 equations of condition employed in determining the value of a coefficient. Having compared his tables with the observations of Bradley and Maskelyne, Burg discovered a general discordance in the epochs which he was unable to account for. Similar errors presented themselves when he instituted a comparison between the computed longitudes, and the observations of Lahire and Flamstead, towards the close of the seventeenth century. Laplace suspected that the errors were produced by some periodic inequality of long duration in the moon's mean longitude, and he pointed out two equations, either of which might possibly be the cause of them. One of these depended on the disturbing action of the sun, and had for its argument twice the longitude of the moon's node, plus the longitude of her perigee, minus three times the longitude of the sun's perigee. Its period amounted to 184 years. The other equation depended on the fact that the figure of the earth is not symmetrical on each side of the equator. It had a period of 179 years. The equation, depending on the action of the sun, being of the tenth order of magnitude,

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* It is easy to see from the form of the expressions for the variations of the elements, that the next step in the approximation will introduce the third powers of the eccen-

Laplace did not undertake the excessive labour of calculating its maximum value. Burg, however, selected it for the purpose of representing the errors of the tables, applying to it an empiric coefficient equal to 15″ In 1812 Burchardt obtained corrections of Burg's elements, which he employed in the calculation of new tables. He rejected the equation of long period depending on the action of the sun, and substituted instead of it, the equation which Laplace attributed to the difference between the two hemispheres of the earth. This equation was not calculated any more than the other; Burchardt merely endeavoured to satisfy the observations by applying to it an empiric coefficient equal to 12″.5.

The lunar tables were hitherto principally indebted to observation for the details as well as the groundwork of their construction, since the arguments alone of the equations were derived from theory. As this circumstance was considered derogatory to the dignity of Physical Astronomy, the French Institute in 1824 proposed as the subject of a prize, a theory of the moon's motion which should only exact from observation the data required to determine the six fundamental elements. Two memoirs were deemed worthy of being crowned; one by Damoiseau, the other by Plana and Carlini. The former of these has been published in the third volume of the "*Memoires des Savans Etrangers;*" the latter has not been given to the world, but an elaborate work on the same subject, and avowedly executed upon the same plan, was published by Plana, in 1832*. Damoiseau has used Laplace's method of investigation; but he has carried the approximation to a much greater extent than that illustrious geometer has done. His memoir has been much admired for the clearness which pervades its vast expansion, and the beautiful symmetry which distinguishes the arrangement of the terms. He calculated tables of the moon solely by the aid of his theory, which have been regarded as at least equal to any of those in previous use. The method of Plana agrees essentially with that pursued by Clairaut and his successors; but he has introduced into it several ingenious modifications. The entire researches of this distinguished geometer exhibit the most commanding mastery of his subject. It is impossible to repress a feeling of sympathy for the author, when he informs us that throughout all the immense calculations of this work, he had not the benefit of a single assistant†.

Damoiseau, as well as Plana and Carlini, found reason to believe that the equations of long period, suggested by Laplace, did not possess sensible values. Laplace, who always had strong misgivings respecting the magnitude of these equations, concurred in this opinion, and it was now generally admitted that the errors in the moon's epoch could not be accounted for by an equation of any known form. Carlini in 1824 suggested four equations for the purpose of reconciling the tables with observation. Some of these he considered to bé preferable to Laplace's, but he maintained that the errors were best represented by a term depending on the square of the time. The admission of this explanation would have been tantamount to an abandonment of the question; but such a course would have been a reproach to the advanced state of Physical Astronomy, and could not on any account be sanctioned.

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* Théorie du Mouvement de la Lune, 3 tomes 4to, Turin, 1832.

† "Je n'ai pû me faire aider par personne; j'ai dû traverser seul, cette longue châine de calculs, et il n'est pas étonnant si par inadvertence, j'ai omis quelques termes qu'il fallait considerer pour me conformer à la rigueur de mes propres principes."—Discours Preliminaire, p. xii.

Sir John Lubbock has investigated the lunar perturbations in some valuable papers which have appeared in the volumes of the Royal Society, and also in a special treatise on the subject. The most remarkable innovation which he has introduced into his researches is that of employing the mean longitude of the moon as the independent variable. This is the practice which had always been pursued in the planetary theory, but in the lunar, it had been hitherto deemed more convenient to derive the mean longitude in terms of the true, considered as the independent variable, and then by the reversion of series to obtain the expression for the true longitude in terms of the mean. The eminent geometer just mentioned conceives, however, that the use of the mean longitude as the independent variable conduces to greater simplicity in practice, even when the question relates to the lunar perturbations. His example was soon afterwards followed by Poisson, who introduced another important innovation into the lunar theory, by employing in his researches the method of the variation of elements *.

The only difficulty which still continued to attach to the lunar theory arose from the errors in the mean longitude, which had occasioned so much fruitless discussion since the commencement of the nineteenth century. Although the inequality of long period, depending on the action of the sun, which Laplace suggested as the probable explanation of these anomalies, was generally believed to be insensible, still, as no attempt had been made to calculate its real value, the question as to its adequacy to account for the errors of the tables continued to be involved in doubt. Poisson, having examined this point with great attention, discovered the important fact that the disturbing action of the sun could not produce an inequality in the moon's mean longitude of the form indicated by Laplace. He then considered the inequality depending on the difference in the compression of the two hemispheres, and he found, as Laplace had already done, that it was quite insensible. Sir John Lubbock, about the same time, very simply arrived at a similar conclusion by the aid of his own formulæ.

Doubts now began to be entertained respecting the accuracy with which the observations were reduced, for the theory of gravitation was found to be so satisfactory in all other respects that it seemed impossible to question its adequacy to account for all the real irregularities in the motions of the heavenly bodies. A step, however, was at length taken which established in the clearest light the actual existence of the phenomenon, and terminated in the triumphant vindication of Newton's principles. For some years past all the observations on the sun, moon, and planets, which have been made at Greenwich since the middle of the last century, have been undergoing a course of reduction conformably to a uniform plan, under the able superintendence of the present Astronomer Royal. In the summer of 1846, the lunar observations were so far completed as to enable Mr. Airy to obtain fresh corrections for the elements, and with these the epochs were calculated for different times and compared with corresponding observations. This comparison had the effect of confirming by the most decisive evidence the researches of Burg and other astronomers. It appeared evident that the epoch was affected by some inequality of very long period, and probably of a very complicated form. Mr. Airy, aware that M. Hansen was then engaged with the lunar theory, transmitted to him the data he had just obtained, under the full assurance of that illustrious mathematician's competency to investigate the intricacies of the moon's perturbations. As Poisson and Lubbock had already shewn that the errors of mean longitude could not be traced to the direct action of the sun, or the disturbing influence of the earth's figure, the question was narrowed to the investigation of the effects produced by the action of the planets, which had been hitherto supposed to be almost insensible. Hansen, guided by his profound knowledge of the theory of perturbation, undertook a rigorous scrutiny of all the inequalities of long period which appeared likely to afford an explanation of the errors of the tables. He calculated many equations of this nature that were found to be insensible; but he finally discovered two depending on the action of Venus, the magnitudes of which were totally unexpected. One of these arises from the fact that sixteen times the mean motion of the earth, plus the mean motion of the moon, minus eighteen times the mean motion of Venus, is a very small quantity. Indeed, when its numerical value is computed, it is found to amount only to about $\frac{1}{3600}$th of the moon's mean motion. Now the terms of the perturbing function which have this quantity for a multiplier of the time under the symbols sine or cosine, although of the third order with respect to the eccentricities and inclinations, will acquire by double integration its square as a divisor, and will on this account form an equation of considerable magnitude in the expression for the longitude. Hansen found the maximum value of this equation to amount to 27″.4, and its period to 273 years. The other equation depends on this—that the difference between thirteen times the mean motion of the earth, and eight times the mean motion of Venus, is a very small fraction. Its maximum value amounts to 23″.2, and its period to 239 years. The inequality represented by this equation is manifestly analogous to the long inequality in the earth's epoch discovered by Mr. Airy. Indeed, it is not difficult to see that the latter inequality will occasion a variation in the mean value of the disturbing action of the sun, and will thereby give rise to an inequality of a similar nature in the moon's longitude.

These two inequalities discovered by M. Hansen, when applied to the moon's computed longitude, completely account for the errors in the tables, which had so long perplexed the astronomers and mathematicians of Europe. The lunar theory may, therefore, now be considered as divested of all serious embarrassment; and in its present state it undoubtedly constitutes one of the noblest monuments of intellectual research which the annals of science offer to our contemplation. From the age of Hipparchus down to the present day, the complicated movements of the moon have formed the subject of anxious enquiry. One by one have her numerous inequalities been detected, and their laws ascertained, until the astronomer is finally enabled to predict her place with all the accuracy called for by the most refined appliances of modern observation. Perhaps no other part of astronomy exhibits so many unequivocal triumphs of the theory of gravitation as the researches connected with the moon's motion. The coincidence between the deductions of the geometer and the results of actual observation is truly astonishing, when one considers the intricacy of the subject. Newton furnished incontestable evidence of the truth of his principles when he calculated the motion of the moon's nodes to within $\frac{1}{300}$th part of the actual motion. In the present day, however, the theories of Plana and Damoiseau assign the motions of the apsides and nodes to within $\frac{1}{20,000}$th part of their observed values*. Euler conceived that the disturbing action of the planets would offer unsurmountable difficulties towards arriving at a complete theory of the moon's motion, and he asserted that this circumstance would for ever prevent astronomers from reducing the error in the computed place of that body below 30″†. This statement is well calculated to suggest the important character of the results to which M. Hansen has been conducted by his researches in the present instance‡.

The moon's mass has been variously estimated by astronomers. Newton, by a comparison of his theory of the tides with observation, concluded that it amounted to $\frac{1}{39.788}$, the earth's mass being supposed equal to unity§. Laplace similarly inferred from the height of the tides at Brest that the moon's mass was equal to $\frac{1}{58.6}$‖. This result is considerably less than that assigned by Newton; but Laplace conceives that on account of the influence of local circumstances on the height of the tides at Brest, the real value of the moon's mass is even still less. He, therefore, determined the mass by other methods, and estimated its most probable value by taking a mean of all the results. Three distinct methods offer themselves for this purpose, besides that suggested by the theory of the tides. One of these depends upon the fact that the force which retains the moon in her orbit, as indicated by her periodic time and observed distance, is due not merely to the action of the earth, but to the united actions of the earth and moon. Hence, by computing the force in this manner, we get the sum of the masses of the two bodies, and if the earth's mass is already known, the moon's mass becomes known also. Adopting 57′ 12″.03 as the mean parallax of the moon, Laplace obtained by this method $\frac{1}{74.2}$ for the value of her mass.

Another method for determining the moon's mass is suggested by the inequality in the sun's longitude, depending on the displacement of the earth from the common centre of gravity of the earth and moon. Since this is the point to which astronomers refer the computed place of the earth, it is clear that the motion of that body round it will generally cause the computed and observed places of the sun to differ. When the moon is in syzigees the inequality vanishes; for then the sun appears in the same position, whether observed from the earth, or from the centre of gravity of the earth and moon. It manifestly attains its maximum value at the quadratures, where the lines drawn from the sun to the earth and to the centre of gravity of the earth and moon, diverge most from each other. Now the amount of this divergence depends on the relative distances of the earth and moon from their common centre of gravity, and these again depend on the relative masses of the two bodies. Hence, when the maximum value of the inequality is determined by observation and the mass of the earth is at the same time known, the mass of the moon becomes known also. Now Delambre inferred, from a great number of observations on the sun, that the maximum value of the lunar equation was 7″.5. This result gave Laplace $\frac{1}{69.2}$ for the value of the moon's mass.

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* See Poisson's *Mémoire du Mouvement de la Lune autour de la Terre*, Mém. Acad. des Sciences, tome xiii. 1835.

† "Au reste je ne doute pas, qu'en corrigeant les lieux moyens de l'apogée et du nœud dans les tables ordinaires, on ne puisse par ce moyen parvenir à determiner le lieu de la lune à 30″ près. Or pour un plus haut degré de précision on ne saurait jamais l'espérer à cause de l'action des autres planètes à laquelle la Lune est assujetie." Théorie de la Lune, Prix de l'Académie, tome ix.

‡ Since the preceding lines were written, we have ascertained that at the meeting of the Astronomical Society for September 1848, Mr. Airy communicated the corrections of the elements of the lunar orbit, deduced from the Greenwich observations from 1750 to 1830. When we consider the extent and accuracy of these observations, embracing the united labours of Bradley, Maskelyne, and Pond; and the eminent talents of the astronomer who has superintended their reduction and discussion, we may confidently expect that the results which have been obtained by means of them, will impart greater precision to the lunar tables than any others of a similar character that have yet been arrived at.

§ Princip. lib. iii. prop. 37, cor. 4.

‖ Méc. Cél. liv. vi. chap. xvi.

The third method which Laplace employed for determining the moon's mass is suggested by the inequality of nutation. This inequality being due to the action of the moon on the terrestrial spheroid, it is clear that a comparison of its observed value with the formula for it, furnished by theory, will lead to a knowledge of the moon's mass. Laplace assumed the maximum value of nutation to be 9″.6, as estimated by Maskelyne, and hence inferred that the moon's mass was equal to $\frac{1}{71}$.

Comparing together these different results he finally fixed upon $\frac{1}{68.5}$ as the most probable value of the mass.

The researches of succeeding astronomers generally lead to the conclusion that the real value of the moon's mass is somewhat less than the estimate of Laplace. It is fortunate that this is a point, in which great precision is not called for by the existing state of science.

CHAPTER XI.

Theory of the Perturbations of the larger Planets.—Theory of Mercury.—Researches of Le Verrier.—Theory of Venus.—Determination of its Mass.—Theory of the Earth.—Solar Tables.—Delambre.—Long Inequality depending on the Action of Venus discovered by Mr. Airy.—Theory of Mars.—Evaluation of its Mass.—Theory of Jupiter.—Calculation of the Terms of the Long Inequality involving the Fifth Powers of the Eccentricities.—Researches of Plana.—Correction of the value of Jupiter's Mass.—Theory of Saturn.—Researches relative to the determination of its Mass.—Theory of Uranus.—Its anomalous Irregularities.—Discovery of an Exterior Planet by means of them.—Theory of the Smaller Planets.—Hansen.—Lubbock.—Theory of Comets.—Researches on the Motion of Encke's Comet.—Hypothesis of a Resisting Medium.—Perturbations of Halley's Comet calculated.—Satellites of Jupiter, Saturn, and Uranus.—Determination of the Mass of Saturn's Ring, by Bessel.—Libration of the Moon.—Nicollet.—Theory of the Figure of the Earth.—Researches of Ivory on the Attraction of Elliptic Spheroids.—Experiments with the Pendulum.—Mean Density of the Earth.—Motion of the Earth about its Centre of Gravity.—Poisson.—Researches on the Tides.—Oscillations of the Atmosphere.—Experiments of Colonel Sabine.

Although the methods devised by the mathematicians of the last century for the purpose of computing the effects of planetary perturbation were complete in so far as the more important bodies of the system were concerned, there still remained, even in this part of the theory, various points which called for further investigation. The masses of the planets in some cases of more precise data; while, in other cases, there appeared discordances in the results obtained by different methods, the origin of which it was desirable to ascertain. Perplexing errors also began to creep into the tables of several of the planets, and once more threatened to tarnish the fair fame of the Newtonian theory. It is gratifying to reflect that these anomalies, one by one, have yielded to the researches of the geometer, and the law of gravitation still retains the character of simple grandeur by which it was distinguished, when first announced by its immortal discoverer.

The planet Mercury, notwithstanding its insignificance, has in all ages given more trouble to astronomers than any other of the older planets of the system. This has mainly arisen from the imperfect character of the observations, by means of which the elements of its motion have been determined; for, previous to the invention of the telescope, it could only be seen a little before or after sunset, when the vapours of the horizon rendered it difficult to ascertain its precise place. It is clear, also, that the magnitude of the eccentricity would tend to aggravate the effect of any error in the position of the orbit. The transits of the planet across the sun's disk afford a favourable means of testing the theory of its motion, and serve as valuable data for the correction of the elements. The earliest transit which history records took place in 1631, A.D. Kepler had predicted the phenomenon by means of the Rodolphine tables, and Gassendi had the good fortune to witness its occurrence. The accordance between theory and observation was found to be pretty satisfactory, but it may be considered as the effect of a fortuitous combination of circumstances, rather than the result of well established principles; for, at a subsequent period, Hevelius and his assistants were compelled to remain four days at their telescopes, waiting for a similar phenomenon. Halley's tables of the planets were found to give the times of the transits with greater precision than those of any preceding astronomer, but still the errors frequently amounted to several hours. Lalande, after a long course of persevering efforts, published tables of the planet, which he conceived to possess all desirable accuracy. In the year 1786, a transit, calculated by means of them, having been announced to take place, the day appointed for the occurrence of the phenomenon was looked forward to with great interest by the Parisian Savans. "At sunrise," says Delambre, "it rained; all the astronomers of Paris were at their telescopes, but, fatigued with waiting, and no longer retaining any hope, they quitted their places half an hour after the time announced for the planet's egress from the sun's disk. I resolved to wait till the moment indicated by Halley's tables; but such a degree of perseverance was unnecessary, for the phenomenon took place three quarters of an hour later than the time fixed for it by Lalande, and three quarters of an hour earlier than that assigned by the tables of the English astronomer." Lalande was exceedingly annoyed by this circumstance, more especially as he had previously denounced a transit recorded by Wing, merely on the ground that it did not conform to his theory. Nowise daunted, however, he resumed his researches on the planet, and finally calculated tables, which, if not as satisfactory as could be wished, were certainly far superior in accuracy to any that had hitherto appeared. The first improvement which they received is due to Lindenau, who published new tables of the planet in 1813. His researches were founded principally on 17 recorded transits. He concluded from his results that the motion of the planet could not be sufficiently accounted for, without assigning a considerable increase to the mass of Venus. In 1844, Le Verrier instituted a profound examination into the theory of Mercury. He calculated all the terms of perturbation which could sensibly affect the motion of the planet, and then employed them in the formation of preliminary tables, for the purpose of obtaining corrections of the elements. His researches were based on about 400 meridional observations of the planet, made at the Royal Observatory of Paris during the present century, and also on a considerable number of transits, all of which he submitted to a careful discussion before introducing them into the equations of condition. Having in this manner obtained new elements of the planet, and also a correction for the mass of Venus, he constructed tables, which were found to represent the observations with wonderful precision. The following interesting account of a transit which had been previously calculated by means of them is given by Mr. Mitchell, of the Observatory of Cincinnati, in the United States:—"Five minutes before the computed time of the contact, I took my place at the instrument; the beautiful machinery that carries the telescope with the sun was set in motion, and the instrument directed to that part of the sun's disk at which it was anticipated the contact would take place. And there I sat, with feelings which no one in this audience can realise. It was my first effort; all had been done by myself. After remaining there for what seemed to be long hours, I inquired of my assistant how much longer I would have to wait; I was answered four minutes. I kept my place for what seemed an age, and again inquired as before; he told me that but one minute had rolled by. It seemed as if time had folded his wings, so slowly did the moments crawl on. I watched on until I was told that but one minute remained, and within sixteen seconds of the time I had the almost bewildering gratification of seeing the planet break the contact, and slowly move on till it buried itself, round, and deep, and sharp in the sun."

The foregoing account may serve to give the general reader an idea of the accordance existing in the science of astronomy, between theory and observation, even under circumstances of the most disadvantageous character. In the present case, this accordance reflected the highest honour on the accuracy of Le Verrier's researches, and augured favourably for the future efforts of that illustrious geometer.

As Mercury disturbs the motions of the other planets only in a very small degree, and as it is moreover unaccompanied by one or more satellites, it has been found very difficult to ascertain the precise value of its mass. In the absence of more satisfactory methods, this element has been determined by assuming the densities of the planets to vary in the inverse ratio of their mean distances from the sun, and then combining the density of the planet found in this manner with its volume, as indicated by its apparent diameter. By this means astronomers have obtained $\frac{1}{1,909,706}$ for the value of the mass of the planet, the sun's mass being represented by unity. Encke, however, has concluded from his researches on the perturbations of the comet which bears his name, that the mass of Mercury does not exceed $\frac{1}{4,865,751}$ *. Le Verrier, on the other hand, assigns $\frac{1}{3,000,000}$th as the most probable value of the mass. Mr. Rothman, again, by comparing the theory of the planet with the observed motion of Venus's perihelion, estimates the mass at $\frac{1}{3,182,843}$ †. This result agrees very nearly with Le Verrier's, but it must be admitted

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* Pro. Astr. Soc., February, 1842.

† Mém. Astr. Soc., vol. xii.

that considerable uncertainty still rests upon this point. It is fortunate that the very circumstance which renders it so difficult to ascertain the mass of this planet with precision, should also in a great measure relieve the astronomer from the embarrassment of an erroneous evaluation.

The perturbations of Venus, though very minute, are more considerable than those of Mercury, on account of her greater proximity to the other planets. The principal disturbing body is the earth, but even in this case the greatest inequality in longitude does not exceed 10″. Laplace computed the terms of perturbation as far as the third powers of the eccentricities and inclinations, but he neglected those of higher orders, under the impression that they were too minute to be appreciable by observation. Mr. Airy, however, has discovered among the terms of the fifth order a sensible inequality, depending on the action of the Earth. It takes 240 years to pass through all its values, although its greatest magnitude does not exceed 2.″9. We shall presently have occasion to mention the circumstance which gave rise to the detection of this interesting inequality.

Halley's tables of Venus were those which were held in most esteem by astronomers throughout the last century, until they were supplanted by those of Lalande towards its close. In 1810, Lindenau published tables founded on the Greenwich observations, and on those made on the continent during the present century. They are the tables now generally used by astronomers.

The mass of Venus is an element of physical astronomy, which has occasioned much research. As the planet is unaccompanied by a satellite, its mass can only be determined by means of the perturbations it produces in the motions of the other bodies of the system. These, however, are so very small, that until recent times no dependence could be placed on the results derivable from them. Lagrange, in his famous researches on the planets which appeared in the Berlin Memoirs for 1781–82–83, determined the mass of Venus, by assuming the densities of the planets to vary in the inverse ratio of their mean distances from the sun. In this manner he found a value for the mass of the planet, which exceeded the value of the earth's mass, nearly in the proportion of three to two. This will manifestly result from his hypothesis, when we consider that the volumes of Venus and the Earth are nearly equal, and that the mean distance of the latter from the sun, exceeds that of the former nearly in the proportion of three to two. That this estimate, however, greatly exceeded the real value, was evident from the result which Lagrange arrived at, relative to the displacement of the Earth's orbit by the action of the other planets. In this case, the principal disturbing bodies are Jupiter and Venus, and as the mass of the former of these planets was sufficiently well known, it was clear that any discordance between theory and observation ought to be referred to an error in the mass of the latter. Lagrange computed by theory, the displacement of the terrestrial orbit, and obtained 61″.5 for the secular diminution of the obliquity of the ecliptic; but as the observations of astronomers generally gave less than 50″ as the real value, it hence followed that the mass of Venus was too great, and that the hypothesis on which its determination was founded was erroneous. Delambre, while engaged in the construction of his solar tables, determined the mass of Venus by means of the periodic inequalities which she occasions in the Earth's longitude. Clairaut had previously sought to determine the mass of the planet by this method, but he had not for his data the accurate observations of Bradley and Maskelyne, nor was the knowledge relative to the reduction of observations, and the mode of combining them together, sufficiently advanced in his day. It is not to be wondered at, then, that he erred in his estimation of the planet's mass, nearly as much as Lagrange did by a less legitimate process, having made it, in fact, equal only to two-thirds of the Earth's mass. Delambre, under superior advantages, obtained $\frac{1}{356,632}$ for the mass of the planet. By a curious coincidence this result agrees almost exactly with the most approved evaluation of the Earth's mass *. Lindenau concluded, from his researches on the motion of Mercury, that the mass of Venus considerably exceeds the value assigned to it by Delambre. Burchardt, on the other hand, by a comparison of the solar tables with Maskelyne's observations, was induced to fix the mass of the planet at $\frac{1}{401,211}$ †. Mr. Airy, by means of later observations at Greenwich, obtained $\frac{1}{401,847}$ for the mass of the planet ‡. M. Le Verrier was conducted to a mass equal to $\frac{1}{390,000}$, by his researches on the motion of Mercury §. These values agree very nearly with each other, and also with the value assigned by the diminution of the obliquity of the ecliptic. Mr. Rothman, however, has concluded from the motion of Mercury's nodes, that the mass of Venus is at least equal to $\frac{1}{355,308}$ ‖. The more complete developement of the secular variations of the planetary orbits, can alone lead to desirable precision on this point.

A knowledge of the perturbations occasioned in the motion of the Earth by the action of the other planets, forms an indispensable preliminary to the construction of accurate solar tables. We have seen that Clairaut first investigated these perturbations by the application of his solution of the problem of three bodies. His results were introduced into Lacaille's solar tables, which continued in use among astronomers until the close of the last century.

One of the first steps taken by the French Board of Longitude, after its establishment in 1795, was to procure the construction of new tables of the sun, moon, and planets. In pursuance of this object Delambre investigated the elements of the solar orbit by means of the observations of Bradley and Maskelyne, and calculated tables which were first published in 1806. The arguments of the equations depending on perturbation were derived from Laplace's theory, but the coefficients were determined by observation. Delambre by this means obtained values of the masses of Venus, Mars, and the Moon. In 1812, Burchardt, with the view of obtaining corrections of the solar elements, compared Delambre's tables with about 4000 Greenwich observations. He concluded from his researches that the epoch, the perigee, and the eccentricity required slight corrections. He also found that the mass of Mars should be diminished by $\frac{1}{20}$th, and the mass of Venus by $\frac{1}{9}$th ¶. In 1827, Mr. Airy compared the solar tables with 86 observations of Sir James South's, and concluded that the epoch and the perigee, especially the latter, ought to be sensibly altered **. In 1828 he discussed 1200 right ascensions of the sun, observed at Greenwich with the new transit instrument between the years 1816 and 1826 *. The corrections he obtained for the elements agreed very nearly with those to which Burchardt was conducted. He also found for the mass of Venus a value equal to Burchardt's, but he concluded that the mass of Mars should be diminished in the proportion of 22 to 15. Delambre, in his tables, had fixed the coefficient of the lunar equation at 7″.5; Mr. Airy, by a method of great elegance and simplicity, obtained a correction which reduced it to 6″.4. Having discovered a series of anomalies in the mean longitude, he was led to suspect that they proceeded from an inequality of long duration, depending on the disturbing action of some of the planets. It finally occurred to him that an inequality of this nature is occasioned by the action of Venus, and that it might possibly be of such magnitude as to account for the errors of the tables. Among the terms of the disturbing function there is a certain class in which the time (under the symbols, sine, and cosine) is multiplied by the difference between thirteen times the mean motion of the Earth, and eight times the mean motion of Venus. This quantity being a very small fraction, Mr. Airy perceived that the operation of two successive integrations would introduce very minute divisors into the corresponding terms of the longitude. The terms in other respects are very small, for their arguments indicate them to be only of the fifth order, relative to the eccentricities and inclinations. It became then a matter of calculation to ascertain whether the increase which they acquired by double integration would so far compensate for their extreme minuteness, as to give rise to an inequality of sensible magnitude. This arduous task was performed by the eminent geometer above mentioned, and the results obtained by him entirely justified his previous suspicion. He found that the terms represented an inequality in the Earth's longitude equal to 2″.05; he also obtained a similar inequality for Venus, depending upon the reciprocal action of the Earth, and amounting to 2″.9. The period in each case extends to 240 years. These inequalities vanished in 1742, and attained their maximum values in 1802. Mr. Airy remarks, that if the mean motions of the two planets had been derived from a comparison of Bradley's observations, with those of recent years, the longitude of the Earth at the time of the next transit of Venus in 1874 would be too small by 4″, and that of Venus too great by 6″; and these errors would occasion a derangement in the geocentric longitude of the latter planet, amounting to between 20″ and 30″ †. This inequality is exactly similar to the long inequality of Jupiter and Saturn, but the labour of computing it is vastly greater. We must admit that its detection reflects the highest honour on the sagacity of Mr. Airy, especially when we consider the very minute form under which it appeared among the observations, and the slowness with which it is developed.

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* Pontécoulant, in the third volume of his *Théorie Analytique du Système du Monde*, gives $\frac{1}{356353}$ as the value of the earth's mass.

† Connaissance des Temps, 1816.

‡ Phil. Trans., 1828.

§ Connaissance des Temps, 1847.

‖ Mem. Astr. Soc., vol. xii.

¶ Connaissance des Temps, 1816; see also *Mémoires de l'Institut*, 1812.

** Phil. Trans., 1827.

The comparative magnitude of this inequality shews how unsafe it is to estimate the perturbations of a planet by the mere order of the terms relative to the eccentricities and inclinations. In the theory of Venus the greatest inequality among the terms of the second and third orders does not amount to 1″.4; and all the inequalities depending on the terms of the fourth order fall below 0″.1. In the theory of the Earth the contrast is still more striking. The greatest inequality among the terms of the

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* Phil. Trans., 1828.

† Ibid., 1832.

second order is equal only to 1″.07; and all those depending on the terms of the third and fourth orders are less than 0″.1 *.

The researches of geometers on the perturbations of Mars have not led to any interesting results; they form a striking contrast in this respect with those memorable researches in connexion with the same planet by means of which Kepler was conducted to his great discoveries relative to the fundamental laws of the motions of the planets and the forms of their orbits. The perturbations produced by this planet in the motions of the other bodies of the system are so small as to render the determination of its mass exceedingly difficult. Laplace effected this object by the aid of the hypothetic principle that the densities of the planets vary in the inverse ratio of their mean distances from the sun. In this manner he obtained $\frac{1}{1,846,082}$ for the mass of the planet *. Delambre, by comparing Laplace's formula of the Earth's perturbations with the solar observations of Bradley and Maskelyne, was induced to fix the mass at $\frac{1}{2,546,320}$ †. Burchardt by a similar process obtained $\frac{1}{2,680,337}$ for its true value ‡. The accordance between these results is sufficiently satisfactory; but Mr. Airy has inferred from his researches on the solar theory that Delambre's estimate should be diminished in the proportion of 22 to 15. It is fortunate, as in the case of Mercury, that the disturbing effects of this planet are so insignificant as to dispense with the necessity of extreme accuracy.

In the theory of Jupiter the most important point is the long inequality depending on the action of Saturn. This inequality is mainly contained among the terms of the third order relative to the eccentricities and inclinations; but Laplace suspected that the terms involving the fifth powers of the eccentricities might also be sensible. Burchardt performed the laborious operation of calculating these terms, but unfortunately he applied them with the wrong sign. Laplace soon afterwards noticed this circumstance in a supplement to the third volume of the *Mécanique Céleste*, and he shewed that, when it was duly taken into account, the theory would present a most satisfactory accordance with a conjunction of Jupiter and Saturn observed by Ibyn Jounis, at Cairo, towards the close of the eleventh century. That astronomer has assigned 1439″ as the excess of Saturn's geocentric longitude over that of Jupiter on the 31st October, 1087, at 16^h mean time of Paris. Now, when the places of the two planets were calculated by theory for the same epoch previous to the detection of Burchardt's error, the excess of longitude was found to amount only to 729″. When the effect of that error, however, was subsequently taken into account by Laplace, the same quantity rose to 1117″. The difference between this result of theory and the recorded excess of longitude amounts to 322″ or 5′ 22″, a quantity which falls considerably within the errors of the observations of the Arabian astronomers.

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* Méc. Cél., liv. vi. chap. ix. et x.; Théorie Analytique du Système du Monde, liv. chap. xiv. et xv. The largest inequalities in Venus and the Earth depending on the terms of the third order are due to the disturbing action of Mercury. The reciprocal action of these planets occasions analogous inequalities in the motion of Mercury, the maximum effect of Venus amounting to 8″, but that of the Earth only to 0″.5.

† Méc. Cél., liv. vi. chap. vi.

‡ Tables du Soleil, 1806.

§ Connaissance des Temps, 1816. Pontécoulant attributes this value to Bessel (Théorie Analytique du Système du Monde, tome iii. p. 346); but as it is less than Delambre's exactly by $\frac{1}{9}$th, which was the correction obtained by Burchardt, it is clear that we ought to read for Bessel's name that of the last-mentioned

In 1824, Plana having raised an objection against Laplace's researches on the part of the long inequality of Jupiter and Saturn depending on the square of the disturbing force, a controversy arose, in which these two geometers, as well as Poisson and Pontécoulant, took part, and which finally terminated in establishing the point at issue in a more satisfactory state*.

The most important improvement which the theory of Jupiter has received in recent times consists in a correction of the value of his mass. This element may be determined by means of the perturbations which the planet occasions in the motions of the other bodies of the system. It may also be found by comparing the periodic time and mean distance of one of the satellites round its primary with the periodic time and mean distance of the planet round the sun. This was the method which Newton employed in the Principia. Having assumed that the period of the fourth satellite amounted to $16^d.6888$, and its greatest heliocentric elongation to $8' \, 15''.85$, he hence concluded that the mass of the planet was equal to $\frac{1}{1067}$, the sun's mass being represented by unity†. This value was found by Laplace to agree very nearly with that derived from the perturbations of Saturn, and was introduced by him into the calculations of the *Mécanique Céleste*. Astronomers, however, subsequently discovered that it was irreconcileable with the perturbations occasioned by the planets in the motions of the other bodies of the system. In 1826 Nicolai concluded from the perturbations of Juno that the mass of Jupiter was equal to $\frac{1}{1053.924}$. About the same time Encke obtained $\frac{1}{1050}$ for the value of the mass by means of the perturbations of Vesta; while his researches on the motion of the comet which bears his name assigned $\frac{1}{1054}$ as the true value. These results, agreeing all so nearly with each other, derived additional confirmation from the researches of Gauss on the perturbations of Pallas. It now became exceedingly desirable to verify the original determination of the planet's mass which presented so considerable a discordance with these results. The observations on the fourth satellite which formed the data of Newton's investigation were made by his contemporary, Pond; but these could hardly be expected to possess the accuracy attainable by astronomers in the present day. Mr. Airy, suspecting that the discordance might be traced to an error in Pond's observations, undertook a series of measurements of the elongations of the fourth satellite, and arrived at a result which accorded very satisfactorily with that derived from the perturbations of the smaller planets. The mean distance of the satellite from its primary as indicated by these elongations assigned $\frac{1}{1048.69}$ as the mass of the planet. Mr. Airy subsequently undertook a more extensive course of observations on the satellites, and derived from them a similar result. Bessel also about the same time measured the elongations of the satellites, and obtained $\frac{1}{1047.871}$ for the mass of the planet, a result differing only about a thousandth part from Mr. Airy's. Thus a serious source of perplexity has been in a great measure removed from Physical Astronomy by these researches, for the mass of Jupiter is so considerable that a small error in its value might occasion a very sensible discordance between the observed and calculated perturbations of some of the planets. The only difficulty which still remains in connexion with this question arises from the anomalous result derived from the perturbations of Saturn, Bouvard having by this means obtained $\frac{1}{1070}$ for the value of the mass. It is to be hoped that the planet Neptune will be found to derange the motion of Saturn to such an extent as to account for the errors of perturbation which arise from the assumption of the more probable value of Jupiter's mass.

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* Mem. Astr. Soc., vol. ii. In the same paper Plana points out several other inaccuracies into which Laplace had fallen in the *Mécanique Céleste*.

† Princip., liv. iii. prop. viii. cor. i.

The theory of Saturn is so closely linked with that of Jupiter, that any remarks relative to the perturbations of one of the planets are generally applicable to those of the other. The masses of the two planets have also been determined by similar methods. Newton obtained $\frac{1}{3021}$ for the mass of Saturn, by assuming that the period of the sixth satellite amounted to $15^d.9453$, and its greatest elongation to 3′.4″*. The latter quantity, however, considerably exceeds the real value, and therefore the results derived from it were erroneous. Laplace supposed the elongation to be equal only to 2′ 59″, and hence inferred that the mass of the planet is equal to $\frac{1}{3359}$†. Bouvard obtained $\frac{1}{3512}$ for the value of the mass by means of the perturbations of Jupiter. This result is confirmed by the researches of Bessel, who has been conducted to a mass equal to $\frac{1}{3500.5}$ by a careful measurement of the elongations of the sixth satellite.

In 1808, Bouvard published tables of Jupiter and Saturn, but they were soon found to be vitiated by the errors in the theory of both planets, to which allusion has already been made. This defect was remedied by the astronomer just cited, who in 1821 published tables of the planets adapted to the corrected theory. Mr. Adams has recently discovered an important error in the tables of Saturn. While engaged in researches on the motion of that planet, he found that the calculated values of one of the terms of the perturbation in latitude were totally irreconcileable with the formula from which they were professedly derived. He has explained the probable origin of this discordance, which is somewhat curious ‡.

Until very recently the theory of Uranus has occasioned much trouble to astronomers. Tables of the planet were published by Delambre in 1790, and by Bouvard in 1821; but, notwithstanding the care and skill which had been employed in their construction on each of these occasions, it was found that they failed to represent the actual motion. Irregularities were indicated by the observations, which could not be accounted for either by the principles of elliptic motion, or by the disturbing action of the other bodies of the system. Without pursuing this interesting subject further at present, we shall merely state that these anomalous errors in the motion of Uranus have led to the discovery *à priori* of a new planet exterior to it. In the ensuing chapter we shall give a detailed account of the circumstances connected with this remarkable result of the theory of gravitation.

Astronomers have not yet arrived at a sufficiently satisfactory result relative to the mass of Uranus. Sir William Herschel having announced that the fourth satellite revolved round the planet in $13^d.4559$, and that its greatest heliocentric elongation was equal to 44″.23 §, Laplace hence concluded that the mass of the planet was equal to $\frac{1}{19504}$ ‖. Bouvard, on

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* Princip., liv. iii. prop. viii. cor. i.

† Méc. Cél., liv. vi. chap. vi.

‡ The principal terms of the perturbation in latitude are—

$$9''.67 \text{ sin. } (\phi - 2\,\phi' - 60^\circ.29) + 28''.19 \text{ sin. } (2\,\phi - 4\,\phi' + 66^\circ.12)$$

where ϕ ϕ' denote the mean anomalies of Saturn and Jupiter. In tabulating the last term, Bouvard appears to have employed $\phi - 2\,\phi'$ instead of $2\,\phi - 4\,\phi'$, so that the two terms may be united in a single term, represented by $25''.85 \sin (\phi - 2\,\phi' + 49^\circ.88)$. The

the other hand, has obtained $\frac{1}{17918}$ for the value of the mass by means of the perturbations produced by the planet in the motion of Saturn. It is to be hoped that the researches on the satellites, in which M. Otto Struve is known to be engaged at present, will lead to more satisfactory results relative to this important point*.

The theory of the smaller planets still continues in a very imperfect state. This circumstance is attributable to the magnitude of the eccentricities and inclinations, in consequence of which the disturbing function converges with such slowness as to render the usual methods of approximation generally inapplicable. The only one of the ancient planets which bears any analogy in this respect to those more recently discovered is Mercury; but in this case the disturbed body is so near the sun, and at the same time so remote from the larger planets of the system, that its perturbations are very insignificant, and a small number of the terms of the disturbing function suffice for the calculation of all the inequalities that are of sensible magnitude. The smaller planets, on the other hand, all revolve in the region comprised between the orbits of Mars and Jupiter, and on this account their elliptic motions are very much deranged by the powerful action of the latter planet. Attempts have frequently been made to investigate the perturbations of these bodies by the usual methods of approximation, but their places when thus determined have been found very soon to present a marked discordance with those indicated by actual observation. The perturbations of Vesta, Juno, and Ceres, have been computed with more or less success by Daussy, Santini, Damoiseau, and other geometers; but those of Pallas, which revolves in an orbit, inclined at an angle of 34° to the ecliptic, have deterred even the most persevering analysts from undertaking their complete investigation. In recent times, attention has been principally directed towards the algebraic form of the disturbing function, with the view of devising modes of development, which shall be practicable whatever be the magnitude of the eccentricities and inclinations. The researches of Cauchy, Liouville, Le Verrier, Hansen, and Lubbock, in connexion with this subject, have resulted in various ingenious processes by means of which it is to be hoped that this part of the planetary theory will soon attain a degree of perfection, equal to that which is so conspicuous, when the question relates to the perturbations of the larger bodies of the system. Le Verrier has applied his method to the computation of a remarkable inequality in the mean motion of Pallas, occasioned by the disturbing action of Jupiter. This inequality depends upon the near commensurability of the mean motions of Jupiter and Pallas. *Eighteen times the mean motion of Jupiter, minus seven times the mean motion of Pallas*, forms a quantity which amounts to only $\frac{1}{130}$th of the mean motion of the latter planet. Now, as this quantity appears in the disturbing function under the symbols sine and cosine, the operation of two successive integrations will introduce its square into the denominators of the corresponding terms of the longitude. This circumstance may cause the term sto acquire a sensible magnitude, although in other respects they are very inconsiderable, being, according to the theory of planetary perturbation, only of the eleventh order with respect to the eccentricities and inclinations. Le Verrier shewed it to be one of the advantages of his method, that the great inclination of the disturbed planet, so far from forming a barrier to his researches, on the contrary, conduced to their simplification. He found the greatest value of the inequality to amount to 896″, or 14′ 56,″ and its period to upwards of 675 years. This inequality is manifestly similar to several others of long duration, to which we have already had occasion to allude, but it differs from them in so far as the particulars relative to it are not susceptible of being tested by observation, on account of the short time that has elapsed since the discovery of the planet. As it was desirable to verify the calculations of Le Verrier, M. Cauchy determined the value of the inequality by a method of his own, and obtained a result which completely accorded with that of the original discoverer.

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* Dr. Lamont, of Munich, by observing the elongations of the satellites, has obtained a value for the mass of the planet considerably less than either of those mentioned in the text. See Mem. Ast. Soc., vol. xi.

The researches on the perturbations of the smaller planets gave rise to an interesting discusssion among astronomers respecting the essential nature of the principle of gravitation. In 1826, Nicolai having compared the analytical expressions for the perturbations of Juno by Jupiter, with fifteen observed oppositions of the planet, met with such discordances as induced him to suppose that the absolute attraction of Jupiter on the sun and on the planet were unequal, or, in other words, that the total amount of attraction exerted by one body upon another depended on the quality of the matter contained in the attracted body, as well as upon its quantity. This doctrine, being at variance with the fundamental principle of gravitation, attracted a considerable degree of attention on the occasion of its first announcement; but the subsequent researches of astronomers have served to shew that it is untenable. Bessel, for this purpose, made a great number of experiments with pendulums, composed of different substances, such as ivory, glass, marble, meteoric stones, &c.; but he was unable to discover in the times of oscillation any indication that the intensity of the terrestrial attraction depended on the quality of the pendulous body.

The theory of Comets depends on the solution of two problems of capital importance. The one relates to the investigation of the species of conic section, in which the comet moves, and the determination of the elements of the orbit; the other relates to the calculation of the effects produced by the disturbing action of the planets. Both of these problems have largely occupied the attention of geometers, from the establishment of the theory of gravitation by Newton, down to the present day. The first solution of the problem for determining the orbit of a comet, by means of observations on its motion, was given by Newton in the Principia. It was founded on the supposition, that the species of conic section, described by the comet, is a parabola. This assumption conduced much to the simplification of the problem, nor did it entail any sensible error on the ultimate results, when the eccentricity of the orbit is very great. On the other hand, the solution was defective, inasmuch as it assigned no means of ascertaining the value of the mean distance in the case of the orbit being really elliptic. This element could only be determined by means of the relation between it and the periodic time, the latter being deduced from the interval comprised between two successive appearances of the comet. Various solutions of the same problems have been given by geometers since Newton's time, some of which are independent of any assumption with respect to the form of the orbit. The most celebrated of the latter class are those of Laplace and Gauss.

The researches on the perturbations of comets offer difficulties pre-

As the usual solutions of the Problem of Three Bodies completely fail in this case, the effects of planetary disturbance are computed by the method of mechanical quadratures. We have already mentioned that Lagrange first presented this method in a systematic form. In 1810, Bessel published his famous researches on the comet of 1807. He concluded, from the observations, that the comet moved in an ellipse of great eccentricity. In computing the action of the planets on it, he resolved the disturbing force in the directions of the radius vector, of a perpendicular to it, and a perpendicular to the plane of the orbit. He then determined, by the method of quadratures, the values of the elements for every thirty days during which the comet was within the sphere of planetary influence. He assigns 1543 years as the most probable value of the periodic time. A comet which appeared in 1815 was found by Olbers to revolve in an elliptic orbit with a period of about 72 years. The perturbations were computed by Bessel, whose researches on the subject appeared in the *Berlin Memoirs* for 1813*. Fresh elements were computed for every 25 days during which the comet was visible; for every year from 1815 to 1833, and for every two years throughout the remaining part of the orbit. He has fixed the 9th February, 1887, as the time of the next return to perihelion. In 1818, a comet was discovered by Pons, the observed motion of which could not be reconciled with the supposition of a parabolic orbit. M. Arago remarked that its elements bore a strong resemblance to those of a comet which appeared in 1805, and Olbers about the same time was led in a similar manner to suspect its identity with a comet which appeared in 1795. Encke found that the observations might all be satisfied by supposing the comet to move in an elliptic orbit, with a period of about three years and a half. He also computed the perturbations, taking into account the action of all the planets, with the exception of Uranus and the small bodies revolving between the orbits of Mars and Jupiter. The perturbations of Mercury were computed for every 4 days; those of Venus and the Earth for every 12 days; and those of Mars, Jupiter, and Saturn for every 36 days. This process was continued until the action of the planets ceased to be sensible, after which the motion of the comet was referred to the centre of gravity of the sun and planets†. The comet, on the occasion of its perihelion passage in 1822, was not favourable for observation in the northern hemisphere, but it was seen by Rumker at Paramatta in New South Wales. A comparison of the earlier with the more recent observations seemed to indicate that the period of revolution was continually diminishing, and Encke was hence led to suspect the existence of a resisting medium. The perihelion passage of 1825 was not favourable for deciding so delicate a question, but that of 1829 offered peculiar advantages for this purpose. In order to understand this, it is necessary to remark that the axis of the comet lies almost in the plane of Jupiter's motion, and that the aphelion extends very nearly to the orbit of that planet. Hence it is clear that, when Jupiter is in that part of his orbit which is in the neighbourhood of the comet's aphelion, he will very much disturb the motion of the latter; and, if an erroneous value be assumed for his mass, the difference between the computed and observed places of the comet may be very considerable, even independently of the effects which might be produced by a resisting medium. This condition was nearly fulfilled during the comet's revolution of 1819–22, on which occasion Encke, having computed the action of Jupiter by means of the ancient value of his mass, found that the error in the time of the comet's arrival in perihelion could not be wholly accounted for by the supposition of a resisting medium. He now investigated the perturbations of the comet, assuming the existence of a resisting medium, and ascribing an indeterminate error to Jupiter's mass. Equating then the effects produced by these causes to the observed errors of the comet's motion, he obtained a number of equations of condition, the subsequent discussion of which conducted him to a most satisfactory conclusion. He now found for the mass of the planet a value which almost coincided with that derived from the perturbations of the smaller planets; and this improved value, combined with the hypothesis of a resisting medium, afforded a complete explanation of the errors in the various revolutions of the comet. Comparing together the observed and computed places throughout three revolutions, he found that the mean error of a single place was only 18″.3, whereas, by rejecting the hypothesis of a resisting medium, the error rose to 217″.6. The effect of the resistance is to shorten the time of each revolution by about half a day.

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* Airy's account of the progress of Astronomy, British Association Report for 1832.

† Ibid.

The doctrine of a resisting medium has always been a favourite subject of speculation with astronomers; but on no occasion has it been supported by evidence of such a plausible character as in the example above cited. It is manifest, however, that more extensive indications of such a medium must be discovered before the problem of its existence can be considered as having received a definitive solution. It has not yet affected to a sensible extent any of the other celestial bodies, and, until such is found to take place, the question relative to it must remain in abeyance.

On the 27th February, 1826, M. Biela, an Austrian officer residing at Josephstadt, in Bohemia, discovered in the constellation of Aries a round nebulous body, which appeared to him to be a comet. His suspicion was confirmed by a re-examination of the same object on the following evening, when he found that, during the period that had elapsed since his first observation it had advanced about a degree to the east of its original place. The comet was seen on the 9th March, by M. Gambart, at Marseilles; and, on the 10th, by M. Clausen, at Altona. Its elements, when calculated on the supposition of the orbit being a parabola, were found to resemble those of other two comets which appeared in 1805 and 1772. Gambart and Clausen, therefore, simultaneously undertook the calculation of elliptic elements, and obtained results which not only agreed with each other, but also satisfied the observations much better than the original elements. The mean period of a revolution was found to be about 6¾ years.

M. Damoiseau computed the perturbations which the elements of this comet would experience during the revolution of 1826–32, and he found that it would again return to its perihelion on the 27th November, 1832*. The planets whose disturbing influence he took into account were Saturn, Jupiter, and the Earth. He found that the combined action of these bodies would have the effect of retarding the comet's arrival in perihelion to the extent of 9.6642 days. Some degree of alarm was excited by the announcement that the comet, a little before its perihelion passage, would cross the plane of the ecliptic at a distance of only 20,000 miles from the Earth's orbit, and near the place where the latter would then be moving. The results of exact calculation were sufficient, however, to dissipate all fears on this point, for it was found that the comet would cross the ecliptic on the 29th October, 1832; but that the Earth would not arrive at the same place until the 30th November. This comet returned agreeably to prediction, and has subsequently reappeared in 1839 and 1846. On the last-mentioned occasion it underwent a singular transformation, having separated into two distinct comets, which continued to travel together at a mutual distance of 3′ or 4′ during the whole period of their visibility. One of these objects was a little fainter than the other, but each of them exhibited the distinctive features of a comet. The tails were parallel to each other, and extended in a direction perpendicular to the line joining the centres of the nuclei. This extraordinary change in the constitution of the comet appears to have taken place very suddenly. It was first observed in Europe on the 15th of January, 1846, by Mr. Challis of Cambridge, and M. Wichmann of Kœnigsberg; but it was afterwards found that it had been seen on the 12th of the same month by Lieutenant Maury, at the Observatory of Washington, in the United States. M. Plantamour of Geneva determined the elements of each comet by observation, and then computed the perturbations occasioned by the Earth, Jupiter, and Mars. The motions of both comets, when calculated by this process, agreed very closely with their observed motions for the whole period during which they were visible. M. Plantamour found that the absolute distance between each nucleus was constantly the same, and was equal to about two-thirds of the radius of the lunar orbit.

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* *Mémoires de l'Académie des Sciences,* tome viii.; see also the *Connaissance des Temps* for 1830.

The approaching return of Halley's comet in 1835 excited a lively interest in the scientific world, and a strong desire was felt that the perturbations of its elements should be computed. The data necessary for this purpose are the elements of the comet corresponding to the time of its perihelion passage in 1759. These are readily deducible from the observations of that year with the exception of the major axis. This element may be determined by assuming as the major axis corresponding to the perihelion of 1682 the value indicated by the time of revolution between 1682 and 1759, supposing the comet to move in an ellipse, and then applying to it the perturbations it would suffer from the action of the planets during the same period. But these perturbations cannot be computed without a knowledge of the fundamental values of the other elements. It is clear, then, that in order to obtain a complete set of data for calculating the perturbations of the comet relative to its perihelion passage in 1835, the geometer must possess a knowledge of the perihelion elements for 1682. In the *Connaissance des Temps* for 1819, Burchardt has given the elements for 1682 and 1759. His results relative to 1682 are founded on the observations of Flamstead, and those relative to 1759 on the observations of Messier.

A comparison of the three revolutions of this comet comprised between the years 1531, 1607, 1682, and 1759, affords a striking indication of the powerful perturbations which it experiences from the action of the planets. The first of these periods includes 27,811 days; the second 27,352 days; and the third 27,937 days: thus the first exceeds the second by 469 days, and falls short of the third by 126 days. It was clearly impossible, therefore, to arrive at any accurate conclusion relative to the next return of the comet by means of these data alone, or even to affirm that the revolution subsequent to 1759 would be longer or shorter than the preceding revolution. In 1812 the Academy of Turin proposed as the subject of a prize, the perturbations of Halley's comet. The prize was awarded to Damoiseau, whose investigation was subsequently published in the Turin Memoirs for 1817. He determined the value of the major axis, at the time of its perihelion passage in 1759, by computing the action of the planets on it throughout the preceding revolution. Setting out, then, from the complete elements of 1759, he computed the alterations they would suffer throughout the time which would elapse until the comet would again arrive in perihelion. The method he employed in his researches is founded on the variation of elements, and coincides essentially with that explained by Lagrange in 1780. He did not, however, adopt the mode of proceeding suggested by that geometer for the superior part of the orbit, preferring to execute the whole of the calculations by the safer though more laborious process of mechanical quadratures. In computing the perturbations of the major axis from 1682 to 1759, he changed the elements only once, but, in performing a similar operation upon all the elements throughout the revolution subsequent to 1759, he used the corrected elements at every 30° of eccentric anomaly. The planets whose action he took into account in his original memoir were Jupiter, Saturn, and Uranus; and his final result relative to the time of return was, that the comet would pass its perihelion on the 16th November, 1835. Discovering on a subsequent occasion that the Earth would exercise a sensible disturbance on the comet after it had passed its perihelion in 1759, he computed the derangement arising from this cause, and found that it would have the effect of shortening the next revolution to the extent of 12 days. Hence, according to his previous conclusion, the passage of the perihelion would take place on the 4th November.

The researches of Encke having rendered probable the existence of a resisting medium, the return of Halley's comet was anxiously looked forward to by astronomers, under the impression that it would throw some light upon this interesting question. It was principally with this object in view that the Academy of Sciences of Paris proposed the perturbations of Halley's comet as the subject of a prize. After twice offering the prize without obtaining any competitors, the Academy finally awarded it, in 1829, to M. Pontécoulant. The details of this geometer's researches were published in the sixth volume of the *Mémoires des Savans Etrangers*. He assumed, as the basis of his calculations, the elements of Burchardt for 1682 and 1759, and computed the perturbations by a process similar to that employed by Damoiseau. He found that the comet would pass its perihelion on the 7th of November, 1835. In the *Connaissance des Temps* for 1837 * he corrected this result by taking into account the action of the Earth before the perihelion passage of 1759, and by employing, instead of Bouvard's value of Jupiter's mass, that which Nicolai had recently deduced from the perturbations of Juno. His final conclusion was that the comet would pass its perihelion on the morning of the 14th of November, 1835.

The perturbations of Halley's comet formed also the subject of profound investigations by Lehmann and Rosenberger, two German mathematicians of great eminence. The researches of Rosenberger were more complete than those of any of his contemporaries. He determined the elements of 1682 and 1759 by an able discussion of all the observations that were in each case available to him. He also computed the perturbations occasioned by Mercury, Venus and Mars, as well as those due to the larger planets of the system.

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* Published in 1834.

It was announced by astronomers that the comet, if visible at all to the naked eye, would be so about the middle of October, when it attained its nearest distance to the Earth. The event fully justified this prediction. On the evening of the 5th of August the comet was seen for the first time, at the Observatory of Rome. It gradually increased in brilliancy until it finally was visible to the naked eye in the beginning of October. By the 12th of that month it appeared like a star of the second magnitude, and was accompanied by a tail seven or eight degrees long. It soon afterwards plunged into the rays of the sun, and ceased to be visible; but it was again discovered by astronomers early in the following year, after passing its perihelion. On account of its southern declination, its reappearance was not favourable for observation in Europe, but it was seen to great advantage at the Cape of Good Hope by Mr. Maclear, the Astronomer Royal for that station, and also by Sir John Herschel, who was then engaged in prosecuting his sidereal observations in the southern hemisphere. These astronomers continued to observe it until the 5th of May, 1836, when it finally ceased to be visible [*].

The various calculations which have been undertaken, for the purpose of determining the elements of this famous comet by means of the observations made on the occasion of its last appearance, generally concur in assigning noon of the 17th of November as the instant of the perihelion passage. This result presents a very satisfactory agreement with that which we have seen that Pontécoulant arrived at by the aid of theory. Having subsequently revised his calculations, this geometer rendered the accordance still more complete. By employing improved values of the masses of the perturbing planets, he found that the passage of the perihelion would take place at noon on the 16th of November [†].

It must be ackowledged that Rosenberger's result did not coincide so closely with that derived from observation, notwithstanding the elaborate character of his researches. In consequence of the errors which inevitably affect all observations in a greater or less degree, and the uncertainty which exists respecting the real values of the masses of the planets, it is not to be expected that an error of a few days may not occur in the calculated time of the comet's return. The fidelity with which it responded to the deductions of the geometer on the occasion of its last appearance forms one of the many magnificent triumphs which adorn the history of the Theory of Gravitation [‡].

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[*] Some interesting particulars relative to the physical changes which this comet underwent, during the period of its visibility, are contained in Sir John Herschel's *Account of Astronomical Observations at the Cape of Good Hope.*

[†] See the *Connaissance des Temps* for 1838. In this final revisal of his researches Pontécoulant employed the masses of Saturn, Jupiter, and the Earth, as assigned severally by Airy, Bessel, and Encke.

[‡] The following statement of the elements of the comet, derived from the observations of 1759 and 1835, and the elements for 1835 as assigned by the theory of gravitation, will at once indicate the magnitude of the perturbations which the motion of the comet experiences, and the accuracy with which they have been computed:—

On the 22nd of November, 1843, M. Faye, of the Royal Observatory of Paris, discovered a comet, the motion of which could not be reconciled with parabolic elements. Dr. Goldsmicht, a German astronomer, shortly afterwards found that the observations might be satisfied by supposing the comet to move in an ellipse with a period of about 7½ years. Le Verrier has calculated the perturbations of this comet, and has fixed the 4th of April, 1851, as the time when it will return to its perihelion. He found that the action of the planets would retard its arrival 7.67 days.

We have alluded on a former occasion to the comet of 1770, generally known as Lexell's comet, which was rendered visible by the action of Jupiter in 1767, and was subsequently thrown into an orbit of larger dimensions, and rendered invisible by the action of the same planet in 1779. The discovery of a new comet revolving in an elliptic orbit nearly equal in magnitude to the orbit of Lexell's comet, and occupying the same region of the heavens with it, induced some astronomers to suspect that the two bodies were identical. M. Valz of Marseilles appears to have first advanced this hypothesis. He remarked that, by supposing a slight increase in the period of Faye's comet, it would have approached very near to Jupiter in the year 1815, and would have been so powerfully disturbed by that planet, that if it had been previously revolving in an orbit of wider dimensions it would have been thrown by the action of the planet into the small ellipse in which it is now moving. Considering next the effect produced on Lexell's comet by the action of Jupiter in 1779, he found by means of Burchardt's data that the new ellipse into which the comet was then thrown would have a period of rather more than sixteen years. Hence, when the comet had made a little more than two revolutions, it would have again arrived in a position favourable for the action of Jupiter, and would be thrown into the smaller orbit in which it was moving previous to 1779. Comparing the elements of this orbit with those of the orbit in which Faye's comet moved, he conceived that the resemblance was so strong as to justify the conclusion that the two orbits referred to the same comet, the one being periodically convertible into the other by the disturbing action of Jupiter. This idea was no less novel and ingenious than it was interesting and plausible; but, as it rested merely on a rough estimate of Jupiter's influence during the period comprised between the actual appearances of the two comets, it could not be

	Elements derived by Burchardt from the Observations of 1759.	Elements derived by Lieutenant Stratford from the Observations of 1835.	Pontècoulant's Elements for 1835, obtained by computing the Perturbations of the Planets.
Passage of the Perihelion, M.T. Greenwich.	1759, Mar. 12.58	1835, Nov. 15.93	1835, Nov. 15.01
Semi Axis Major.	18.01861	18.0779386	18.00008
Ratio of the eccentricity to the Semi Axis Major.	0.9675571	0.9675509	0.9672807
Longitude of the Perihelion on the Orbit.	303° 10′ 1′,	304° 32′ 9″.2	304° 31′ 42″
Longitude of the Ascending Node.	53° 50′ 11″	55° 8′ 21″.2	55° 10′ 15″
Inclination	17° 37′ 12″	17° 45′ 56″.7	17° 44′ 53″

recognised as an established truth in physical astronomy. The subject was one well adapted to the searching powers of a Laplace or a Poisson, and in their countryman, M. Le Verrier, astronomy found on the present occasion a worthy representative of these illustrious geometers. The mode of proceeding which Le Verrier proposed to adopt in this inquiry was first to determine the effect produced by Jupiter on Lexell's comet, from its discovery in 1770 until the action of the planet ceased to be sensible, and then to trace back the elements of Faye's comet through the various revolutions comprised between the appearance of that body in 1843, and the epoch down to which he had conducted his researches on Lexell's comet. If the ultimate elements of Faye's comet, when thus determined, were found to coincide with those of Lexell's for the same epoch, it might then be concluded that the two comets were identical; but on the other hand, if they exhibited discordances which could not be accounted for by any admissible supposition of errors, whether of observation or calculation, it must necessarily follow that the comets were two distinct bodies. Le Verrier found that the ultimate results would be very much affected by the errors in the original elements of both comets, for the effects of these errors continually increased with the lapse of time, insomuch that in some instances a very small variation of the fundamental elements would be sufficient to alter entirely the character of Jupiter's influence. The investigation was therefore much more complicated and the conclusions less definite than might have been expected if the data had been more precise. Le Verrier having submitted to a careful discussion the observations of 1770 on Lexell's comet, determined by means of them the elements of the orbit, and then investigated the action of Jupiter on the comet. He found that, when the latter arrived within the sphere of the planet's influence, it was compelled to deviate from its elliptic orbit, and described an hyperbola round the planet. The errors of the elements might even be such that, after the action of the planet ceased to be sensible, the comet would still continue to move in an hyberbola, and in that case would never again return to the solar system. Le Verrier computed the elements of the comet for the time when it quitted the sphere of Jupiter's influence, and then compared the results with the elements of Faye's comet, the latter being traced up to the same epoch through the various revolutions anterior to its appearance in 1843. He found that, upon any possible supposition of the errors of observation, the elements of Faye's comet could not be reconciled with those of Lexell's, and he therefore came to the conclusion that they were two distinct bodies.

It is possible that Faye's comet may have originally emerged from the boundless regions of space describing a parabola or hyperbola having the sun in the focus, but that on its arrival within the sphere of Jupiter's influence it was thrown into a new orbit by the powerful action of that planet, and was permanently fixed in the solar system. If the fundamental elements of the comet were mathematically accurate, this question might be decided, at least for any determinate period, by tracing back the comet through the various revolutions anterior to 1843. The errors, however, with which the elements are affected by the inevitable errors of observation, impart so indeterminate a character to the results, that, even if the comet had been so introduced into the solar system, it would have been impossible to ascertain the exact time when this event happened. But although the question does not admit of a definitive solution, on account of the circumstance to which we have just alluded, still it is possible to assign the minimum time during which the comet has been revolving within the limits of the solar system. Le Verrier found that it must have been introduced into the system at least as early as the year 1747; whence it followed that it had made at least thirteen revolutions in an elliptic orbit previous to its discovery.

The interest excited by the periodic comet of 1843 had only abated in a small degree when the solar system was enriched by another discovery of the same class. On the 29th of August, 1844, De Vico, the director of the Observatory at Rome, discovered a comet, which was soon afterwards found by M. Faye to revolve in an ellipse with a period of about five years and a half. MM. Laugier and Mauvais, of the Royal Observatory of Paris, instituted a comparison between the elements of this comet and those of one observed in 1585 by Tycho Brahé and Rothmann. Halley had calculated the elements of the last-mentioned comet on the supposition that it revolved in a parabola, but the French astronomers shewed that the motion might be satisfied better by an elliptic orbit with a period of five years and two months. From the close resemblance which the elements of this comet bore to those of the comet of 1844, MM. Laugier and Mauvais concluded that the two bodies were identical, the actual discordances of the elements being ascribed by them to the effects of planetary perturbation. The grounds upon which this conclusion rested were indeed very plausible, for the eccentricities of both orbits were almost the same, the places of the perihelia did not differ more than 30°, and the distance between the nodes was only 22°. It is manifest, however, that the identity could only be established beyond all doubt by a comparison of the elements of both comets when rigorously determined for some common epoch. Le Verrier investigated the subject by tracing back the comet of 1844 through the anterior revolutions up to 1585, and then comparing the elements for that epoch with those of the recorded comet. The results at which he arrived shew how unsafe it is in researches of this nature to adopt any conclusion which is not verified by rigorous calculation. As we have mentioned already, the distance between the perihelia of the comets observed in 1844 and 1585 was only 30°, and it was reasonably enough supposed that this displacement might have been produced by the action of the planets. Le Verrier, however, found that, when the orbit of the comet of 1844 was traced back, the perihelion, instead of approaching *towards* that of the comet of 1585, on the contrary receded further and further *from* it, insomuch that, at the epoch of 1585, the perihelia of the two comets were diametrically opposite to each other. The nodes also of the comet of 1844 continually receded from those of the comet of 1585, so that, in order to coincide with the latter at the epoch of comparison, they would require to move through 338°, and not 22° during the period comprised between 1844 and 1585. The elements of both comets, when thus determined for the common epoch of 1585, offered discordances of such magnitude as appeared to Le Verrier to be incompatible with identity, and he therefore came to the conclusion that the two comets were totally distinct bodies. He also ascertained in the course of the same researches that the comet of 1844 was not identical with Lexell's comet. He discovered, however, such a strong resemblance between the elements of the new comet and those of a comet observed in 1678 by La Hire, that he considered himself fully justified in concluding that they were identical. Thus, although the comet of 1844 has doubtless formed a part of the solar system for many ages, and has frequently ap-

appearance previous to the year 1844. The planet Jupiter, which in all probability chained it down orginally to the system; will one day act upon it with equal intensity, but in an opposite direction, and we may reasonably presume that, when it has escaped from his influence, it will again fly off into infinite space, describing a parabola or hyperbola.

The theory of Jupiter's satellites has not received any material improvement since the publication of the *Mécanique Celeste.* In 1817 Delambre published new tables of the satellites founded on all the eclipses that had been observed from 1660 down to the commencement of the present century. These were succeeded by Damoiseau's tables which appeared in 1836.

The satellites of Saturn have hitherto occupied the attention of geometers only in a very small degree. This has chiefly arisen from the small progress made by astronomers in determining by observation the elements of their motions, and in tracing, *à posteriori*, the more considerable inequalities resulting from their mutual perturbation. We have seen a striking proof of the utility of observation for both these purposes, in the history of Jupiter's satellites. The influence of the spheroidal figure of Saturn is clearly indicated by the positions of the ring and the orbits of the interior satellites, which coincide almost with the plane of his equator. As the satellites recede from their primary, the disturbing action of the sun increases relatively to that occasioned by the spheroidal figure of the planet, and the orbits commence to deviate sensibly from the plane of his equator, and to incline towards that of the ecliptic. The sixth, or Huygenian satellite, which is the brightest of all, and therefore the most favourable for observation, has formed the subject of an elaborate investigation by Bessel. From the motion of the apsides of this satellite, he concluded that the mass of the ring amounted to $\frac{1}{118}$ of the planet's mass. He also obtained for the latter a value agreeing very nearly with that deduced by Bouvard from the equations of condition employed in the construction of Jupiter's tables. Some valuable observations of the satellites were made by Sir John Herschel in the course of his residence at the Cape of Good Hope. From those of the sixth satellite he derived elements which agreed very nearly with the values assigned by Bessel for the epoch of 1830, taking into account the variation of each element during the intermediate period. He also computed the epochs and mean motions of the other satellites, and obtained results which accorded very well with those deducible from the earlier observations of Sir William Herschel.

If the theory of Saturn's satellites is still in its infancy, the remark applies with still greater force to the satellites of Uranus. These bodies can only be rendered visible by means of the most powerful telescopes, and therefore it may naturally be presumed that a considerable time will elapse before a correct theory of their motions be formed. When their elements are once determined by observation, they will offer peculiar interest to the geometer in consequence of their motion's being retrograde and the planes of their orbits being nearly perpendicular to the plane of the ecliptic.

The consummate researches of Lagrange left little to be accomplished in the theory of the Libration of the Moon. Poisson applied his powers of analysis to this interesting subject, but the results at which he arrived had only the effect of confirming those obtained by the illustrious geometer just mentioned *. Although the mean motions of the moon with respect to rotation and revolution be both equal to each other, still the inequalities in longitude will cause them to differ generally for any assignable time. The longest axis of the lunar ellipsoid will therefore deviate always a little from the direction of the earth's action, and hence will arise a series of inequalities in the moon's rotation corresponding to her various inequalities in longitude, and causing a real libration in her motion. The magnitude of these inequalities manifestly depends on the figure of the moon, or in other words on the ratios of the three axes of the lunar ellipsoid. Hence, if the ratios of these axes be known, we shall be enabled to compute the maximum values of the inequalities, and, *vice versâ*, if the latter be determined by observation, we can readily derive from them the ratios of the axes, and consequently the exact elongation of the moon's figure. Bouvard and Nicollet undertook for this purpose a series of careful observations of the moon's libration in longitude at the Royal Observatory of Paris. The *Connaissance des Temps* for 1822 contains a beautiful paper by Nicollet, in which he submitted these observations (amounting in number to 174) to a searching discussion. The only sensible inequality was that corresponding to the *annual equation* in longitude: it appeared by observation to have a maximum value equal to 4′ 45″. The results at which he arrived, relative to the ratios of the axes do not accord with the generally admitted opinion respecting the primitive condition of the moon. He found, in fact, that the difference between the least and greatest axes was greater than what it would be on the supposition that the moon was originally a fluid mass.

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* Connaissance des Temps, 1821-22. The researches of

It has been well remarked by the illustrious Humboldt, that no inquiry in physical science can compare with that relating to the Figure of the Earth in the disproportion which exists between the ultimate object of attainment and the number of ingenious and refined processes both in mathematical and astronomical science, which the long and arduous pursuit of it has given birth to. The theory of this subject remains nearly in the condition in which Clairaut left it, for the researches of Laplace, notwithstanding the remarkable character of the analysis employed in their exposition, contributed only in a small degree to its advancement. We have mentioned in one of the foregoing chapters that the attraction of elliptic spheroids is intimately associated with the question of the figures of the celestial bodies. The most important improvement which this part of the theory of gravitation has received in recent times is due to the late Mr. Ivory[*]. The problem for determining the attraction of a spheroid upon a particle situated in its interior had yielded at an early period to the resources of the ancient geometry, and was afterwards found to admit of an easy solution by analysis. The analogous problem for an exterior particle, on the other hand, offered difficulties which long seemed to be insuperable, and, although Laplace finally succeeded in devising its solution, his method was so incomplete as to leave ample scope for further research. The subject continued to engage the attention of the ablest analysts until Mr. Ivory finally discovered the well-known beautiful theorem, by means of which the attraction of a spheroid upon a point without it is immediately derived from its attraction on a point within it[†]. This theorem is remarkable for being the most important contribution to mechanical science which had been made by a native of the British Isles since the days of Maclaurin. The memoir which contains its original announcement deserves also to be mentioned, on account of the intimate acquaintance which the author of it exhibits with methods of analytical investigation, the use of which had been hitherto confined exclusively to the mathematicians of the continent. Mr. Ivory soon afterwards gave another striking proof of his talents by a critical examination of Laplace's researches relative to the attraction of spheroids of small eccentricity. Having pointed out what he conceived to be certain defects in Laplace's reasoning, he expounded the peculiar calculus of that great geometer by a method of his own, remarkable for its clearness and elegance. The objections urged against Laplace's demonstration are now admitted to have originated mainly in a misconception of the author's meaning; but the memoir of Mr. Ivory has been universally admired as a fine exhibition of analytical skill *. The important subject of the attraction of spheroids has more recently engaged the attention of Plana, Gauss, Poisson, Airy, and other eminent geometers, but no striking results have been elicited by their researches.

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[*] Born at Dundee in 1765; died at Hampstead, near London, in 1841. During the early period of his career he was in all probability the only person in Britain who possessed an intimate acquaintance with the methods of analysis employed in the higher investigations of Physical Astronomy.

[†] Phil. Trans., 1809.

Before proceeding to notice the applications which have been made of Clairaut's theorem to the determination of the Earth's ellipticity, it may not perhaps be uninteresting to give a brief account of the geodetic operations which have been carried on in recent times with the view of attaining the same object by the actual measurement of arcs on the Earth's surface.

An arc of the meridian has been measured in the present century by Gauss, extending from Gottingen to Altona, and embracing an amplitude of 2° 0′ 57″. The latitudes at the two extremities of the arc were determined by means of Ramsden's famous zenith sector. An arc of still greater extent has been measured in Russia by M. Struve. Its northern extremity is situated in Hochland, an island in the Gulf of Finland; and from this point to Jacobstadt, its southern extremity, it embraces an amplitude of 3° 35′ 5″. One of the peculiarities connected with the measurement of this arc consisted in the determination of the latitudes by means of the transits of stars across the prime vertical †. We may remark that this mode of observation is extensively practised by the German astronomers of the present day, for the purpose of ascertaining the declinations of the stars. The operations of M. Struve were subsequently connected with those of Von Tenner in the south of the Russian empire, and the whole arc now extends to 8° 2′ 28″·91. The geodetic operations by means of which Bessel ‡ connected the arc of M. Struve, in Russia, with the extensive triangulations of the west and south of Europe, exhibited in a remarkable degree the power of that illustrious astronomer to sytematize and perfect every subject which bore any relation to his favourite science. It may be remarked that in all geodetic operations more angles are generally observed, than those which the principles of geometry render indispensably necessary to be known for the purposes of computation. This circumstance will manifestly give rise to a number of relations between the observed angles, which would be rigorously satisfied by the latter if they were mathematically accurate. This condition, how-

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* Phil. Trans., 1812-22.

† Born at Minden 1784; died at Königsberg 1846. He is generally allowed to be the greatest astronomer which the present century has hitherto produced.

‡ The latitudes were also found with the zenith sector, and a mean of both results was in each case taken for the final determination.

ever, being practically unattainable, it remains for the computer to investigate the true correction which should be applied to the values of the several angles. For this purpose each angle is assumed to be affected with an unknown error, and then, by means of the relations above mentioned, a number of equations of condition are formed between the unknown quantities. Now Bessel determined the values of these quantities, not according to the practice hitherto pursued, of grouping the equations of condition into a number of isolated systems, but by combining them all together, and submitting them to one uniform and systematic mode of treatment. The superior advantage of thus making the totality of the observations subservient to the determination of their individual errors is too obvious to require any further notice.

The numerous arcs of the Earth's surface which have been measured in recent times, with the most scrupulous attention and skill in all their details, have enabled astronomers in the present day to arrive at more definite conclusions relative to the magnitude and figure of the earth than it was possible to have deduced at an earlier period. In 1832, Mr. Airy discussed all the arcs of any value that had been measured either in the direction of the meridian, or perpendicular to it. Rejecting the arcs of parallel as unworthy of confidence in so delicate an inquiry, and also all those arcs of the meridian which, being situated in mountainous countries, might be vitiated by the effects of local disturbance, he derived the elements of the earth's figure from the remaining data by a very simple process, in which he was mainly guided by a sagacious appreciation of the relative merits of the ancient and modern measurements. His final conclusion was that the exterior surface of the earth may be represented by an ellipsoid of revolution, the polar semidiameter of which is 20,853,810 feet, or 3949.585 miles; and the equatorial semidiameter 20,923,713 feet, or 3962.824 miles*. This gives 69,903 feet, or 13.239 miles for the excess of the equatorial over the polar semidiameter, and $\frac{1}{298.33}$ for the value of the ellipticity. These results have received a most satisfactory confirmation from the researches of Bessel, who in 1841 was conducted to an ellipticity equal to $\frac{1}{299.15}$ by an elaborate discussion of all the most reliable arcs of the meridian. We may mention that the elements of Mr Airy are those which have been employed in all the more recent calculations connected with the Ordnance Survey. We shall have occasion presently to notice a very satisfactory confirmation of their accuracy which has been afforded by the measurement of an extensive arc of parallel.

The great meridional arc of India has recently received a considerable extension. The operations connected with this arc were commenced by Colonel Lambton about the beginning of the present century, and the first section, extending from Punnœ in lat. 8° 9′ 35″ to Damargida, in lat. 18° 3′ 15″, was completed by that officer in 1815. Another section extending from Damargida to Kalianpur, in lat. 14° 7′ 11″, was executed by Colonel Everest, who succeeded Colonel Lambton in the superintendence of the operations, upon the death of the latter in 1823. The whole arc from Punnœ to Kalianpur embraced, therefore, an amplitude of 15° 57′ 40″, and was consequently the most considerable which had yet been measured. The methods employed were similar to those practised in the measurement of the English arc, and the result was generally considered by competent judges to be equal in point of accuracy to the very best modern determinations of a similar character. The recent extension to which we have alluded is due to Colonel Everest, who has now prolonged the arc to Kaliana, in latitude 29° 30′ 48″. He effected this object not by continuing the triangulation from Kalianpur, the northern extremity of the great arc, but by proceeding in the opposite direction from Kaliana to Kalianpur. For this purpose a base was measured in the Valley of Dhera Dhun, near the inferior range of the Himalayah Mountains. The extreme station of the arc was situated at a distance of seventy miles to the south of the base, Colonel Everest justly fearing that, if a less remote locality were selected, the attraction of the Himalayah Mountains might exercise a disturbing influence on the celestial observations. In 1837, Colonel Everest completed the triangulation as far as Kalianpur; and, as he had some doubts respecting the accuracy of his previous operations beyond this point, he continued to advance southwards, until he reached Damargida, the northern extremity of Colonel Lambton's arc. The celestial amplitude was determined by dividing the terrestrial arc into two sections at Kalianpur, and then making simultaneous observations on the stars at the two extremities of each section. Thirty-six stars were observed in determining the amplitude of the northern section, and thirty-two in determining that of the southern section. In each instance half of the stars were situated to the south and the other half to the north of the zenith points of both extremities of the arc; but the greatest distance of any star from the nearest zenith did not exceed 5°. The amplitude of the northern section was found to be 5° 23′ 37″.051, and that of the southern section, 6° 3′ 55″.973. This gives 11° 27′ 33″.024 for the amplitude of the whole arc measured by Colonel Everest. The terrestrial length of the northern arc was found to be 1961157.117 feet, and that of the southern arc 2202926.196 feet. Comparing these results with the amplitudes, it appears that the length of a degree of the meridian in mean latitude 26° 49′ is 363,606 feet, and that the length of a degree in mean latitude, 21° 5′, is 363,187 feet. The whole of Colonel Everest's arc, when compared with the English arc between Dunnose and Clifton, gives $\frac{1}{318}$ for the ellipticity of the earth. A similar comparison with the French arc gives $\frac{1}{316}$; with the Russian arc $\frac{1}{309}$; and with the Swedish arc $\frac{1}{313}$. Besides the base measured in the valley of Dhera Dhun, two bases of verification were measured by Colonel Everest, one at the southern extremity of the whole arc, and the other near Kalianpur, the middle station. The verification was effected by computing the bases at the two extremities of the arc from the base in the middle, and then comparing the results with those derived from actual measurement. In this manner the length of the base at the northern extremity was found to be 39183.273 feet by computation, and 39183.873 feet by actual measurement. The difference, therefore, amounted only to $\frac{6}{10}$ths of a foot, or about seven inches. Again, the length of the base at the southern extremity was found to be 41578.178 feet by computation, and 41578.536 feet by measurement. This gives a difference of $\frac{358}{1000}$ths of a foot, or a little more than four inches. The near agreement of these results affords a strong guarantee for the accuracy of the whole operation. The bases were measured with an apparatus devised by General Colby while engaged in the operations of the Irish Survey. It consisted essentially of two metallic bars, each ten feet long, so connected together as by their unequal expansion to obviate the necessity of applying a correction for temperature*.

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* Encycl. Metrop., Art. Figure of the Earth.

The great Indian arc from Punnœ to Kaliana now embraces an amplitude equal to 21° 21′ 13″. The southern portion may not perhaps be so unexceptionable in its execution as it was at one time generally supposed to be; but still, when taken in its full extension, this arc is unquestionably one of the most valuable data we possess for determining the figure of the earth, and is destined in all ages to shed a brilliant lustre on the history of British rule in India.

The arc of the meridian measured by Lacaille, at the Cape of Good Hope, presented an unaccountable anomaly when compared with similar measurements executed on the opposite side of the equator. It would appear, from the result at which he arrived, that the earth's surface is less curved in the southern than it is in the northern hemisphere. This conclusion excited the surprise of astronomers, being totally at variance with the theory of gravitation, which assigns the same ellipticity to both hemispheres. On the other hand, the high celebrity of the astronomer upon whose authority it rested, served only to render the question still more perplexing. When Colonel Everest visited the Cape of Good Hope, in 1821, he carefully inspected the tract of country in which the arc was measured, and drew up the result of his observations in a letter addressed to Colonel Lambton, which appears in the first volume of the "Memoirs of the Astronomical Society." The southern extremity of the arc was situated at Lacaille's Observatory in Cape Town, and the northern extremity at Kleip Fonteyn. The celestial amplitude was 1° 12′ 1″.55, and the latitude of the middle point was 33° 18′ 30″. Lacaille found the terrestrial length of the arc to be 68469 toises,—whence 1° was equal in length to 57037 toises. Now, if we assume the earth to be an oblate spheroid, having an ellipticity equal to $\frac{1}{300}$, a supposition which agrees very well with the result of a comparison of meridional arcs in the northern hemisphere, the amplitude of an arc whose terrestrial length and mean latitude is the same as that of Lacaille's will be found to be 1° 12′ 10″.54. This result exceeds, by 8″.99, the amplitude of the arc as determined by Lacaille. Colonel Everest strongly suspected that the discordance arose from the disturbing influence occasioned by the attraction of the mountains in the neighbourhood of the two terminal stations. He remarked that the mountains at Cape Town would so affect the plumb line as to make the zenith appear a little to the south of its true place, while, on the other hand, those at Kleip Fonteyn would cause the zenith of that station to deviate a little to the north of its true place. Hence the apparent amplitude of the arc as derived from the zenith distances of the stars at its two extremities would be less than the true amplitude by the sum of both zenith errors. Colonel Everest therefore concluded, that Lacaille's measurement might be reconciled with the usual value of the ellipticity, by assuming that the zeniths of the two extreme stations were disturbed by local attraction to the extent of 8″.99. After alluding to the inexpediency of remeasuring the arc, in consequence of the practical difficulty of observing the angles from Lacaille's stations, this distinguished officer then proceeds in the following terms:—"It might be interesting, no doubt, to ascertain the exact latitudes of both extremes of the arc, by a series of triangles connecting them with the observatory now about to be erected in this neighbourhood, and this, *which will doubtless be hereafter done*, may in able hands furnish a new datum respecting the attraction of mountains; but, as to the arc itself, it seems to me to be too small to be of any weight, even were all other objections removed, and the labour of correcting the old result, except for the mere curiosity of the matter, would therefore be much better expended upon a new series of triangles. Such a series, instead of terminating at Kleip Fonteyn, might very easily be carried through the country of the Namaquas to the northern boundary of the colony, which would furnish a very pretty arc of nearly 4° in amplitude, and, I doubt not, set for ever at rest the anomalous hypothesis of the different form of the two opposite hemispheres of the globe."

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* The accuracy of the results obtainable by the use of this apparatus was put to a severe test by the remeasurement of the bases. The base of Dhera Dhun was remeasured in an opposite direction, and, although the whole length exceeded seven miles, the two results did not differ by so much as 2½ inches. Another mode of verification consisted in dividing the base into three sections, and, having remeasured the middle section, deriving the other two from it by triangulation. The whole length of the base when determined by this process did not differ more than a quarter of an inch from the original measurement. An equally satisfactory result was obtained by a similar remeasurement of the base at Damargida, the southern extremity of the arc.

We have cited these remarks, not only because they reflect the highest credit on the sagacity of Colonel Everest, but also on account of the interesting confirmation which they have recently derived from the labours of Mr. Maclear, the Astronomer Royal at the Cape of Good Hope. In the year 1837 the latter determined the latitude of Lacaille's Observatory by means of a triangulation connecting it with the Royal Observatory. The result, when compared with that derived by Lacaille from direct observation, seemed to indicate the existence of local attraction, but the discordance was not sufficiently great to account for the whole anomaly of Lacaille's arc. Mr. Maclear resolved, therefore, to verify the entire operations of that astronomer by a careful remeasurement of the arc. The latitudes at the extreme stations, as determined by means of the zenith sector, were found to agree with the values assigned by Lacaille, but the computation of the triangles had the effect of shortening the arc by 200 feet, and thereby reducing the anomaly in its length to about half its previous magnitude. Suspecting that the remaining part of the error arose from the ill-conditioned character of Lacaille's triangles, he chose another set of stations, and then repeated the whole operation; but to his great disappointment he obtained a result which agreed almost exactly with that at which he had previously arrived. He now resolved to measure an arc of three or four degrees, and to select the two extreme stations, so as to be beyond the reach of local disturbance. This object he successfully effected notwithstanding many hardships he had to encounter in consequence of the inclemency of the weather, and the impassable nature of the country in which his operations were conducted. The triangulation was carried southwards to Cape Point, and northwards as far as Kamies Berg. The distance between these two extremes comprehended an arc of nearly 4° 2′; but, as Mr. Maclear suspected that the station at Cape Point was subject to local disturbance, he fixed the southern limit of the arc at the Royal Observatory. The distance between this station and Kamies Berg included an arc of 3° 34′. Two intermediate stations were selected, one at Kleip Fonteyn, and the other to the north of that station. The final result at which he arrived was of the most gratifying character. He found that the length of a degree was now almost exactly reconcilable with the ellipticity derived from a comparison of meridional arcs in the northern hemisphere. He also completely succeeded in tracing the various circumstances which conspired to vitiate Lacaille's arc. We have already mentioned that the error was reduced to half its original magnitude by the remeasurement of the terrestrial arc. The remaining half was found by Mr. Maclear to arise from errors in the latitudes of the two extreme stations, occasioned by the attraction of the mountains in their neighbourhood. When the latitudes were determined at stations removed from the influence of local attraction, and then connected trigonometrically with the arc, they were found to be very nearly reconcilable with the usually received value of the ellipticity. A slight deviation of the plumb-line took place at the southern station, but a much greater deviation was occasioned by the attraction of the mountains at Kleip Fonteyn. The errors at both stations, however, conspired together in reducing the amplitude of the arc, agreeably to the remark of Colonel Everest. Mr. Maclear has earned for himself a high place among living astronomers by the ability with which he has executed this important geodetic operation. It is gratifying to reflect that his labours have removed a serious difficulty from the science of astronomy, since we are now assured that the actual measurement of meridional arcs on each side of the equator concurs with the theory of gravitation, and the experiments with the pendulum, in assigning the same ellipticity to both hemispheres.

The arc of the meridian connected with the trigonometrical survey of Great Britain now extends from Dunnose, in the Isle of Wight, to Balta, one of the Shetland Isles, and embraces an amplitude of 10° 7′ 55″.28. The latitudes were determined with Ramsden's zenith sector at ten different places, including the two extreme stations. When the whole course of triangulation was executed, the latitudes at the various stations were then computed by means of an assumed value of the ellipticity, and the relative bearings and distances, setting out from Greenwich, with which the operations were trigonometrically connected. The latitudes found in this manner for the two extreme stations, on being compared with those determined with the zenith sector, presented a discordance which seemed to imply an error either in the observations, or in the assumed value of the ellipticity. The latitude of Dunnose was again determined with the new Ordnance zenith sector*, but no error was discovered in the previous measurement. Another station was then selected about a mile distant, and the latitude, on being determined with the same instrument, was found to differ as much as 3″.2 from the result obtained at the original station. This circumstance is the more remarkable, as the surrounding country does not seem to indicate the existence of any disturbing influence. The latitude as thus determined at the new station coincided exactly with that previously found by a geodetic process, and thereby afforded an important confirmation of the value of the ellipticity upon which the latter result depended. The computed and observed latitudes of the northern extremity of the arc at Balta presented also a similar discordance, and were similarly reconciled together by repeating the observations with the zenith sector at a place in the neighbourhood of the original station. The method of determining the latitude of a place by connecting it trigonometrically with a distant station, is probably destined to throw much valuable light on the subject of local disturbance, especially since the true figure of the earth is now pretty well ascertained.

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* The superb instrument of Ramsden, having been deposited for safety in the armoury of the Tower, was unfortunately destroyed by the fire which consumed that part of the building in 1841.

It will readily appear on the inspection of a map that the position of the British Isles is well adapted for the measurement of an extensive arc of parallel. An important illustration of the truth of this remark has been recently afforded by Mr. Airy's determination of the arc of longitude comprised between Greenwich and Valentia, a small island on the south-west coast of Ireland*. This island is situated about 11° west of Greenwich, and lies nearly in the same latitude with it. Mr. Airy proposed in the first instance to determine the exact difference of the longitudes of the two places by the transportation of chronometers, and then to effect the same object by means of the bearings and distances assigned by the Ordnance Survey, combined with certain assumed elements of the earth's figure. If the two results were found to agree within sufficiently narrow limits, it might then be fairly presumed that the elements of the earth's figure were well determined. On the other hand, if they differed to a sensible extent, the amount of the difference would serve to indicate the correction which it would be necessary to apply to the assumed elements. Feagh Main, in the island of Valentia, was selected as the extreme western point of operations, while Kingston, near Dublin, and Liverpool, were used as intermediate stations. The elements of the earth's figure, assumed as the basis of the geodetic calculations, were those which Mr. Airy had arrived at, by a discussion of arcs of the meridian, and have been already mentioned in this chapter. The comparison of the chronometrical and geodetic arcs exhibited a most gratifying accordance, and thereby afforded a valuable confirmation of the assumed elements of the earth's figure †. This is assuredly not the least valuable of the many results for which astronomy is indebted to Mr. Airy.

We now proceed to give some account of the researches that have been prosecuted for the purpose of deducing the ellipticity of the earth from the variation of gravity at its surface. We have already mentioned that

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* Mem. Ast. Soc., vol. xvi.

† The following are the results obtained by a comparison of the various geodetic and chronometrical arcs.

Geodetic arc of longitude from Greenwich to Liverpool . .	$12^m\ 0^s .35$
Chronometrical arc	12 0 .05
Chronometrical arc smaller	0 .30
Geodetic arc from Greenwich to Kingston	$24^m\ 31^s .48$
Chronometrical arc	24 31 .20
Chronometrical arc smaller	0 .28
Geodetic arc from Greenwich to Feagh Main	$41^m.\ 23^s .07$
Chronometrical arc	41. 23 .23
Chronometrical arc larger	0 .16
Geodetic from Liverpool to Kingston	$12^m.\ 31^s .13$
Chronometrical arc	12 31 .15
Chronometrical arc larger	0 .02
Geodetic arc from Kingston to Feagh Main	$16^m\ 51^s .59$
Chronometrical arc	16 62 .03
Chronometrical arc larger	0 .44

Clairaut's theorem enables us to ascertain the value of this element when once we know the relative intensities of gravity in two different latitudes. The pendulum which Huygens had already applied so admirably to the measurement of time derived from this circumstance a vast accession of importance in the estimation of astronomers. In order to understand how the oscillations of a pendulum lead to a knowledge of the figure of the earth, it is necessary to obtain a clear view of the various circumstances which affect the rate of oscillation. A brief notice of them here may not perhaps be unacceptable to the reader, as they are inseparably associated with the history of the application of the pendulum to scientific purposes.

When a pendulum oscillates in vacuo through very small arcs, the rate of oscillation will depend on the length of the pendulum and the intensity of the moving force. If the oscillations be supposed indefinitely small, a simple relation connects these three elements together, so that by means of it we can ascertain the value of any one of them when the other two are already known. Hence the intensity of the moving force may be readily derived from the length of the pendulum and the rate of oscillation; and if the latter element remain constant, or, in other words, if the pendulum continue to perform the same number of oscillations in the same time, the variation of the moving force will be indicated by the different lengths which it will be found necessary to assign to the pendulum. On the other hand, if the length of the pendulum be assumed to be constant, the variation of the moving force will be indicated by the greater or less quickness with which the oscillations are performed. Hence arise two distinct methods of comparing the different intensities of a force by means of experiments with the pendulum. In the one case, the variation of force is thrown upon the length of the pendulum; in the other, it is thrown upon the rate of oscillation. Both these methods have been employed in determining the variation of gravity at the earth's surface.

The preceding remarks have reference to the purely mathematical theory of the pendulum, and therefore do not take cognizance of those disturbing causes which in all cases complicate physical inquiries. It is necessary then to investigate the effects produced by these disturbing causes, so that by subducting them from the phenomenon we may arrive at the abstract conditions which form the basis of our reasoning. Hence arise various corrections which it is necessary to take into account before the experiments with the pendulum can be made available for the purpose of determining the ellipticity of the earth.

In the first place, the theory of oscillation above stated supposes that, in all experiments with the same pendulum, its length remains constant. This, however, is a condition which cannot obtain in nature, for the fluctuations of temperature will cause the pendulum continually to vary in length, and the effect of this variation will manifestly be to disturb the rate of oscillation. In order then to render the theory applicable to the actual results of experiment, a certain standard of temperature is assumed, and the effect due to the deviation from this standard as indicated by the thermometer is then computed and applied to the actual rate of oscillation. This is termed the correction for temperature.

Again, the oscillations of the pendulum are supposed to be indefinitely small. In reality, however, they all possess a finite magnitude, and also vary continually from one oscillation to another. Both these circumstances will cause the number of oscillations actually performed in a given time to differ in all cases from the number of indefinitely small oscillations which would be performed in the same time. In every case of actual experiment the time of a single oscillation will exceed a certain finite quantity depending on the length of the pendulum and the intensity of the moving force; but, the smaller the arc is, the more nearly will the time of oscillation approach this quantity; which may, therefore, be considered as the time corresponding to indefinitely small oscillations. Hence, in order to reduce the experiments to an accordance with theory, the effect due to the magnitude of the arc is computed, and then applied to the actual rate of oscillation. This is termed the correction for the amplitude of the arc.

The third correction which it is necessary to take into account, in conducting experiments with the pendulum, depends on the fact that the force of gravity varies at different distances from the centre of the earth. Clairaut's theorem has reference to the variation of gravity at the *surface only* of the terrestrial spheroid. It is necessary, therefore, to make allowance for the altitude of the place where the experiments are made, by diminishing the force indicated by the oscillations, in the ratio of the square of the distance from the earth's centre.

Lastly the oscillations are supposed to take place in a vacuum. In reality, however, they are performed in a medium of air. Newton first considered, in accordance with mathematical principles, the effect produced on the motion of a pendulum by the resistance of the air. He has demonstrated, on the two hypotheses of the resistance being proportional to the first and second powers of the velocity, that, when a pendulum makes very small oscillations in a cycloid, it does not experience any sensible retardation from this cause*. Poisson subsequently showed that this was also true for a pendulum oscillating in a circle when the arcs are supposed very small†; in fact, although the resistance of the air opposes the action of gravity when the pendulum is descending to a vertical position, and in consequence tends to prolong the time of oscillation, yet, when the pendulum is ascending on the opposite side, the same cause conspires with gravity in destroying the velocity, and on this account tends to shorten the time of oscillation. A compensation thus takes place between every two half oscillations, which restores the times of the whole oscillations to nearly a uniform state, rendering necessary only the correction due to the amplitude of the arc. It must be remarked, however, that this conclusion is founded on an imperfect conception of the nature of the resistance offered by the air; for it is assumed that the particles of the latter, as soon as impinged on, immediately afterwards cease to exercise any influence upon the motion of the pendulum, either directly or by means of the agitation they excite among the surrounding particles of the medium. Newton, indeed, did not fail to perceive that this view of the question did not accord with the real condition of nature, for he remarked that the pendulum while descending with accelerated velocity would continue to operate upon the particles of the medium after the first impact, and that the time of oscillation would in consequence be prolonged, while, on the other hand, when the pendulum was ascending on the opposite side, the continual diminution of the velocity would allow the particles to escape from a similar succession of impacts; and hence the effect of the resistance in the one half of the arc of oscillation would not be exactly compensated by the corresponding effect in the other half. It was generally considered, however, that, when the oscillations are very small, the disturbance arising from the resistance of the air was altogether insensible. We shall have occasion presently to mention a series of experiments which clearly established the fallacy of this opinion.

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* Princip., lib. ii. prop. 26 et 27.

† Journal de l'Ecole Polytechnique, tome vii.

But the circumambient air exercises on bodies a statical effect, which is totally independent of the resistance it offers to their motion. Since a body immersed in a fluid suffers a diminution of weight equal to that of the fluid displaced by it, a pendulum will manifestly oscillate more quickly in a vacuum than it will do in air. It is necessary, therefore, in all experiments to reduce the oscillations to those which would take place in a vacuum, by computing the effect due to the weight of the atmosphere as indicated by the barometer. This has been termed the correction for the buoyancy of the atmosphere.

Numerous experiments were made with the pendulum in the course of the last century, for the purpose of ascertaining by means of them the ellipticity of the earth; but, as they were not conducted with due attention to all those minute circumstances which affect such delicate operations, they cannot be considered as worthy of any reliance. We have already mentioned that experiments of this kind may be conducted by two distinct methods. The length of a pendulum which performs a certain number of oscillations in a given time (for example, the seconds' pendulum) may be determined in different latitudes, and the results on being compared together will indicate the variation of gravity, and hence, also, the ellipticity of the earth. The same object may also be effected by transporting an invariable pendulum to different latitudes, and noting the number of oscillations which it makes in a given time at each place. This is generally considered to be the safest mode of experimenting, as it is independent of the absolute length of the pendulum, the ascertainment of which at any time is an operation of extreme delicacy.

Borda, a French philosopher of great merit, was the first who determined the length of the seconds' pendulum with sufficient accuracy for scientific purposes. This step was suggested by the measurement of the great arc of the meridian of France, which took place towards the close of the last century, and the operation was conducted by him with consummate skill in all its details. In 1807, Biot determined, by this method, the lengths of the seconds' pendulum at various stations of the arc between Dunkirk and Formentera, and the results obtained by him may be considered as the earliest data of this kind which fully deserved the confidence of astronomers. This distinguished philosopher afterwards made similar experiments at various other places in the south of Europe, and also at Leith and Unst in the British Isles. Comparing together the lengths of the seconds' pendulum at Unst and Formentera, he obtained $\frac{1}{304}$ for the value of the earth's ellipticity. This result agrees very well with that derived from the comparison of geodetic measurements, and is doubtless a very near approximation to the truth. With respect to other stations, however, it must be acknowledged that the results were not in all cases equally satisfactory.

In 1818, fresh interest was awakened in experiments of this kind by Captain Kater's invention of a new method of great ingenuity for determining the length of the seconds' pendulum. Huygens had already shewn that, whatever be the form of an oscillating body, the centres of suspension and oscillation are convertible. Availing himself of this beautiful property, Captain Kater attached two points of suspension to a pendulum, and then by means of a simple adjustment he brought it to make the same number of oscillations in a given time from whatever point it was suspended. When this took place, it clearly followed that the length of a simple pendulum oscillating at the same rate was measured by the distance between the two points of suspension*. Captain Kater shortly afterwards made experiments with the pendulum at various stations of the British arc. He employed a pendulum of a constant length in all his operations, and determined the variation of gravity by counting the number of oscillations which it made at each station in a mean solar day. A comparison of the results obtained by him at Unst, in the Shetland Isles, and Dunnose, in the Isle of Wight, the two extreme stations, gave $\frac{1}{320 \cdot 1}$ for the ellipticity of the earth†. One peculiarity of these experiments consisted in the correction for the altitude of the station being different from that hitherto employed. Dr. Young had previously remarked that this correction was in all cases too great, as no account was taken of the attraction of the elevated mass on which the experiments in each instance were made‡. If we were raised on a sphere a mile in diameter, and having a density equal to the mean density of the earth, its attraction would be about $\frac{1}{8000}$th of the attraction of the whole earth, and the correction for the elevation of the station, which would be $\frac{1}{2000}$ if the attraction varied merely according to the inverse square of the distance, would be thereby reduced to $\frac{3}{8000}$, or only about three-fourths of the usual correction. Dr. Young's researches induced him to conclude, that if the mean density of the earth be assumed equal to 5.5, and that of an elevated tract of table land to be 2.5, the true correction will be less than that hitherto employed in the ratio of 66 to 100. The necessity for a change in the usual correction, on the grounds assigned by the philosopher just cited, cannot admit of any doubt; but the true correction, which it will be necessary to apply in each case, will always be liable to great uncertainty on account of our ignorance of the density of the elevated mass, and the exact conformation of its materials. For these reasons it will generally be the safest mode of proceeding to perform all experiments of this kind at inconsiderable altitudes above the level of the sea.

In the year 1817, Captain Freycinet, of the French Navy, was sent out by his government on a voyage round the world, one of the principal objects of which was to swing the pendulum in different latitudes. Experiments were made by him at various stations on each side of the equator, and a comparison of his results gave $\frac{1}{267 \cdot 60}$ for the ellipticity of the earth §. His countryman, Captain Duperrey, made a number of similar experiments in the course of an expedition of discovery which he commanded in the years 1822–25. The ellipticity assigned by a comparison of his results was $\frac{1}{266 \cdot 40}$‖. A great number of valuable experiments with invariable pendulums were made by Captain Sabine¶ in different parts of the world. He obtained $\frac{1}{288 \cdot 4}$ for the ellipticity, as the final result deducible from them. Experiments with invariable pendulums have also been made by Goldingham**, at Madras, and, by his directions, at a small island in the

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* Phil. Trans. 1818. † Ibid. 1819. ‡ Ibid. 1819.

§ This is the value of the ellipticity which Mr. Baily deduced from Freycinet's observations after applying to them the true correction for the reduction to a vacuum. See *Mem. Ast. Soc.*, vol. vii. For an account of Freycinet's experiments, see *Voyage autour du Monde*. Paris, 1825.

‖ This result is also that of Mr. Baily. For an account of Duperrey's experiments, see the *Connaissance des Temps* for 1830.

¶ Account of experiments for the purpose of determining the Figure of the Earth, London, 1823.

** Phil. Trans. 1822.

Indian Ocean, almost under the equator; by Hall*, at various places in the Pacific Ocean; by Sir Thomas Brisbane†, at New South Wales; by Foster‡, at Port Bowen, in the North Seas; by Fallows§, at the Cape of Good Hope; and by Leutke‖, an officer in the Russian navy, at several places in both hemispheres. In 1826, Bessel determined the length of the seconds' pendulum with great accuracy by a new method. Having fixed two points of suspension in a vertical position with respect to each other, he suspended from each of them a spherical ball by a fine wire. The distance between the two points of suspension was entirely arbitrary. Bessel made it exactly equal to the Peruvian toise. The two pendulums when in a vertical position were equally depressed, and therefore the difference of their lengths was measured by the distance between the points of suspension. The depression of the lowest parts of the surfaces of the balls to the same level was effected by means of a delicate apparatus, termed the lever of contact. From an approximate knowledge of the lengths of the pendulums, it was easy to derive the correction due in each case to the spherical form of the ball. Hence, the difference of the lengths and the difference of the corrections being given, the difference of the lengths of the simple pendulums corresponding to the two points of suspension was readily assignable. Again, by counting the number of oscillations performed in a given time by each pendulum, the ratio of the lengths of the simple pendulums was also ascertained. By means of these two data, Bessel was enabled to compute the absolute length of each of the simple pendulums. This method of determining the length of the seconds' pendulum is generally considered to be susceptible of greater precision than any other which has been devised for the same purpose.

While Bessel was engaged in the experiments just referred to, he discovered that the usual correction for reducing the oscillations of a pendulum to a vacuum were erroneous to a considerable extent. This important fact was first announced by him in a memoir which appeared in the volume of the *Berlin Academy of Sciences* for 1826. He established its reality beyond dispute; first, by swinging two pendulums in air, one of brass and the other of ivory, and then by swinging the same brass pendulum in air and in water. It appeared from both experiments that the retardation of a pendulum oscillating in a fluid medium was greater than had been hitherto imagined. In the one case he found that the true correction for reducing the oscillations to a vacuum exceeded the correction usually employed in the proportion of 1.956 to 1; and in the other case, in the proportion of 1.625 to 1. He attempted to account for this increase in the amount of retardation by ascribing it to the influence of a coating of air, which, according to him, adheres to the pendulum, and is dragged along with it during its motion. The mass of the pendulum being enlarged by this addition, while the moving power remains the same, it is clear that the accelerative force of each particle will be less intense than it would be in a vacuum, and that a diminution of motion will ensue as a necessary consequence. It is singular that, as early as the year 1786, Du Buat, a French experimentalist, recognised the existence of this retardation, and even endeavoured to give an explanation of its physical origin; but it appears to have entirely escaped the notice of all philosophers until the period of its rediscovery by Bessel.

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* Phil. Trans. 1823. † Ibid. 1823. ‡ Ibid. 1826. § Ibid. 1830.

‖ Mém. Acad. des Sciences de St. Pétersburg; see also Phil. Mag. 1832.

The researches of the astronomer just mentioned, being mixed up with theoretical views which were not clearly established, the Board of Longitude of this country resolved to submit the question to the issue of direct experiment, by swinging the same pendulum both in air and in a vacuum. The operations for this purpose were executed with great care by Colonel Sabine, at the Royal Observatory of Greenwich, in the year 1828. The mean of his results gave 10.36 as the excess of the number of oscillations in a mean solar day, made by a pendulum swinging in vacuo over the corresponding number of oscillations made by the same pendulum swinging in air. The old correction for the buoyancy of the atmosphere gave only 6.26 oscillations. It followed then that the coefficient for the reduction to a vacuum should be increased in the proportion of 1.655 to 1*. Mr. Baily shortly afterwards repeated these experiments with a great variety of pendulums and arrived at results of a similar character†. The amount of retardation differed with the pendulum employed in the experiment, and was found to depend mainly on its form and extent of surface. It appeared also that the air was dragged chiefly by the parts of the surface which were perpendicular to the plane of motion, and that only a small quantity adhered to the sides. Mr. Baily clearly shewed that the true correction can only be determined with precision by means of direct experiments with each particular pendulum‡.

The usual correction for reducing the oscillations of a pendulum to a vacuum having thus been demonstrated, by evidence of the most indisputable character, to be too small, it followed that all the results which had been derived from experiments hitherto performed were more or less erroneous. When the question relates to the determination of the figure of the earth, the error depending on the imperfect reduction is indeed small; since in this case the *variation* of gravity at the surface of the terrestrial spheroid, and not its absolute intensity, forms the subject of research; but, as in delicate inquiries of this nature the most minute circumstance must be taken into account, the true correction has been employed in all subsequent reductions.

The most extensive series of experiments with the pendulum which have been made by any individual are due to the late Captain Foster, who commanded a scientific expedition which was sent out to the South Seas by the Government of this country in the year 1829. This talented officer having unfortunately been drowned a little before the time appointed for the return of the expedition, his papers, upon being brought to England, were put into the hands of the late Mr. Baily, who drew up a detailed account of them, which is contained in the seventh volume of the *Memoirs of the Astronomical Society*. The experiments were made at fourteen different stations included between 10°.38′ north latitude and 62°.56′ south latitude. All the details of these experiments appear to have been conducted with the most scrupulous regard to accuracy, and the results deserve to be classed among the most valuable data we possess for determining the ellipticity of the earth by means of the pendulum. Mr. Baily obtained $\frac{1}{289\cdot48}$ for the value of the ellipticity as the final result deducible from Captain Foster's experiments. He also arrived at an ellipticity equal to $\frac{1}{285\cdot26}$, by combining together the results of all the experiments hitherto made with invariable pendulums. These evaluations agree very nearly with each other, and also with that due to Colonel Sabine, whose experiments were more extensive than those of any other voyager, previous to Captain Foster.

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* Phil. Trans. 1829.

† Phil. Trans. 1832.

‡ Poisson has adopted a different opinion respecting the physical origin of this correction. He considers that the successive impulses of the pendulum would give rise to alternate contractions and expansions of the surrounding medium, whence the particles of the latter, by their reaction, could not fail to produce a sensible effect on the rate of oscillation. The correction is thus made to depend on the resistance offered to the pendulum by the aerial medium, the mode of action being considered in its most absolute character, and independently of any assumed relation between the resistance and the velocity. Proceeding upon this principle, Poisson has investigated the correction by a profound analysis, and has arrived at results which, in some instances, are confirmed by direct experiment. See *Mém. de l'Institut*, tome xi., or the *Connaissance des Temps* for 1834. This view of the subject has also been adopted by Plana, Sir John Herschel, and several other eminent philosophers.

The experiments with the pendulum generally agree in assigning to the earth an ellipticity somewhat greater than that derived from the comparison of arcs of the meridian. In the one case the ellipticity may be said to be $\frac{1}{289}$; in the other case only $\frac{1}{299}$. The perturbations of the moon again indicate an ellipticity equal to $\frac{1}{306}$. The agreement of the second and third of these values is sufficiently satisfactory. It is difficult, however, to account for the discordance of the first and second; and also for the anomalies which present themselves in some instances when only two arcs of the meridian or two experiments with the pendulum form the grounds of comparison. The results of recent geodetic operations concur in shewing that the meridional anomalies are mainly attributable to the effects of local disturbance, and that, when the amount of this disturbance can be in any case independently ascertained, and then eliminated from the measurements, the comparison of the latter may be satisfied by an ellipticity a little greater than $\frac{1}{300}$ *. With reference to the anomalies which occur in pendulum experiments, it appears that the results generally indicate the intensity of gravity to be greater on islands than on continents. This is partly explained by the greater density of islands which are frequently of volcanic origin; but, on the other hand, it is not to be denied that the density of the surrounding ocean is considerably less than the mean density of the earth, and therefore ought to produce an effect of an opposite kind. Much doubtless will depend on the depth of the ocean at the place where the experiments are made, and the nature of the stratum on which it rests; for it must be borne in mind that, although Clairaut's theorem allows the density of the terrestrial spheroid to vary from one stratum to another, according to any law whatever, it supposes the density to be uniform throughout each stratum. The influence exercised on experiments with the pendulum by the geological character of the substratum was first remarked by Colonel Sabine.

When we consider how difficult it is to form a just estimate of the irregularities of the earth's surface, and the variations in the density of the exterior stratum, we have ample reason to be surprised that the anomalies we have been referring to are not greater than they really are. "If the form of the earth's meridian," says Mr. Airy, "were traced on paper, the nicest eye would be unable to distinguish it from a circle. The

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* Some idea of the effect of local disturbance may be formed when we state that, at the station of Banog, within fifty miles of the Himalayah Mountains, Colonel Everest found the difference between the computed and observed azimuths to amount to as much as 20″.156. It is true that the measurement of azimuth angles is the most delicate part of geodetic operations; but, in the present instance, the discrepancy manifestly exceeds the probable error of observation.

ellipticity is so small that the closest inspection without measure could not judge which was intended for the greater and which for the smaller axis. The whole quantity in dispute is less than one-sixteenth of this ellipticity. Instead of being surprised that such a difference exists, we may well be astonished at the accuracy of modern measures of all kinds, which make so small a quantity a subject of controversy."*

As soon as astronomers were persuaded that the principle of gravitation extended to the constituent parts of the celestial bodies, it became an interesting question to ascertain whether the attraction of masses at the earth's surface exerted any appreciable influence. The first attempt to establish this fact was made by Bouguer and his associates, while engaged in measuring an arc of the meridian in South America. Astronomical observations having been made in the neighbourhood of the lofty mountain Chimborazo, it was found that the attraction of the latter caused the plumb line to deviate towards it to the extent of 7″.5. This result was so far confirmatory of the Newtonian theory; but it was generally admitted that the experiment had not been made with sufficient care to merit the confidence of astronomers. The question was not mooted again until 1772, when, Dr. Maskelyne having suggested the repetition of the experiment to the Royal Society, it was resolved by that body that immediate steps should be taken to carry it into effect. The mountain of Schehallien, in Perthshire, was selected as offering peculiar advantages for this purpose. It is a narrow ridge running nearly east and west, and rising to an elevation of about 2000 feet above the level of the surrounding country. The mode of proceeding employed in the experiment was this:—two stations were selected, one on the north side and the other on the south side of the mountain; and the difference of their latitudes was determined, first by means of astronomical observations at each station, and then by means of the meridional distance between the two stations, and certain assumed elements of the earth's figure. Now it is clear that the attraction of the mountain would compel the plumb line to deviate from its vertical position, and would thereby occasion a displacement of the zenith point opposite in direction at each station. Hence the difference of latitudes as determined by astronomical observation ought to exceed the difference resulting from geodetic computation by the sum of the displacements of the zenith points; and this latter quantity is manifestly the measure of the sum of the attractions exerted by the mountain at the two stations. Now the zenith distances of a certain number of stars, having been determined at both stations, were found to indicate a difference of latitude amounting to 54″.6. The meridional distance between the stations, as computed by a process of triangulation, was 4364.4 feet, and the difference of latitude hence deduced was 42″.9. The resulting difference, 11″.7, was, therefore, the quantity due to the attraction of the mountain at the two stations. Thus it was established by actual experiment that the principle of gravitation operates not only between the bodies of the solar system, but also between the smaller masses of matter of which they are composed.

An interesting question suggested by the experiment just alluded to is that relative to the mean density of the earth. It is clear that, if we knew the quantity of matter contained in the mountain, this datum, combined with the ratio of the attraction of the mountain to that of the earth, as assigned by the preceding experiment, would enable us to ascertain the absolute mass of the earth; and, the volume of the latter being already known, we might hence determine its mean density. The mass of Schehallien was determined by computing its volume trigonometrically, and assuming its mean density to be two and a half times the density of pure water. The final result was that the mean density of the earth might be considered equal to four and a half times the density of pure water*. The astronomical observations connected with this famous experiment were made by Dr. Maskelyne, and the laborious calculations to which it gave rise were executed by Dr. Hutton. Playfair, having subsequently ascertained by experiment the densities of the different materials composing the mountain, and also their positions relative to the two places of observation, arrived at the conclusion that the mean density of the earth is contained between 4.867 and 4.559, the mean density of pure water being supposed equal to unity. This gave 4.713 for the mean result†.

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* Encycl. Metrop., Art. Figure of the Earth.

Soon afterwards an attempt was made upon a much smaller scale to accomplish the object sought for in the Schehallien experiment, by means of an apparatus termed the torsion balance. It consisted essentially of a deal rod suspended by a fine wire and having two leaden balls about two inches in diameter attached to its extremities. Two leaden spheres about eight inches in diameter were then brought near the smaller ones on opposite sides of the rod, and their attraction, combined with the torsion of the wire, produced an oscillatory horizontal motion of the rod on each side of a position of equilibrium. Now the extent and the time of oscillation depend, *cæteris paribus*, on the relative intensities of the acting forces, or, in other words, on the torsion of the wire and the attraction of the spheres. Hence, if these two elements be determined by observation, we shall be enabled to ascertain the intensities of the two forces, and to compare the attraction of the spheres with the earth's attraction, as indicated by the oscillations of a vertical pendulum. Finally, since we know the quantity of matter contained in the spheres, we can arrive at the quantity of matter contained in the earth; and, knowing its volume, we may hence derive its mean density. This delicate apparatus was originally devised by Michell, but we owe to Cavendish the first application of it to the determination of the mean density of the earth. From seventeen experiments made with it, that philosopher concluded that the mean density of the earth is 5.48, that of water being unity ‡.

In 1836, M. Reich, of Freiburg, repeated Cavendish's experiments; the only difference being that he used one ball instead of two. The average of all his results gave him 5.438 for the mean density of the earth. By far the most valuable experiments that have been made with the torsion balance are due to the late Mr. Baily §. They were conducted at the public expense upon a recommendation to that effect having been made to the Government by the Astronomical Society; and they extended over the whole period comprised between October, 1838, and May, 1842. The final result deduced by Mr. Baily was that the mean density of the earth is equal to 5.660 ‖. This may be considered as the nearest approximation to the truth which has yet been arrived at by philosophers*. It is remarkable that Newton conjectured that the mean density of the earth is about five or six times the density of water†.

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* Phil. Trans., 1776.

† Phil. Trans. 1811.

‡ Phil. Trans. 1798.

§ Born at Newbury, 1774; died in London, 1844.

‖ Mem. Ast. Soc., vol. xiv.

The Schehallien experiment for determining the mean density of the earth is purely statical; on the other hand, the experiment with the torsion balance is founded on dynamical principles, being in fact a case of the horizontal pendulum. The vertical pendulum has also been employed for a similar purpose. The attraction on the top of a high mountain may be decomposed into two parts; one of which is the attraction at the level of the sea diminished in the inverse ratio of the square of the distance, and the other is the attraction of the mountain itself. Hence, if the intensities of gravity on the mountain and at the level of the sea be determined by observation, the difference of the two forces will indicate the attraction due to the mountain, and, by a process similar to that employed in the Schehallien experiment, we shall be enabled to ascertain the mean density of the earth. Experiments for this purpose were made by Carlini at the hospice of Mount Cenis, which has an elevation of 6375 feet above the level of the sea; and the conclusion at which he arrived was that the mean density of the earth is 4.39, that of water being unity.

We have mentioned already that D'Alembert succeeded in accounting completely for the motion of the earth's axis in space, arising from the action of the sun and moon upon the redundant matter accumulated round the equator. It still remained, however, for geometers to ascertain whether the disturbing forces affected the position of the axis relative to the surface of the terrestrial spheroid, or whether they occasioned any change in the velocity of rotation. Observation, indeed, gave a negative answer to both of these questions; for neither the latitudes of places on the earth's surface nor the length of the day appeared to have undergone any variation from the earliest period of history. It was desirable, however, to establish these facts by demonstrative reasoning founded on the theory of gravitation; for, in the absence of such conclusive evidence, there existed no grounds for supposing that the position of the axis and the velocity of rotation might not be affected by secular inequalities, which, though slow in their developement, would in the lapse of ages become sensible by continual accumulation. If the axis experienced any such displacement, the position of the equator would be constantly shifting with inconceivable slowness, and the sea, by always flowing towards the new position to restore the equilibrium of the particles, would eventually occasion a total change in the relation of land and water on the earth's surface. It is clear also that the latitudes of places would be ultimately affected by the displacement, and hence would ensue a corresponding alteration of the seasons. Poisson first examined this point with all the attention due to its importance. In an admirable paper, which was published in the

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[*] Notwithstanding all the precautions used by Mr. Baily, he found that the resting points of the balls and the times of oscillation were subject to disturbances, the cause of which he was unable to explain. In the *Philosophical Transactions* for 1847, there is a paper by Mr. Herne, of the Royal Military College of Sandhurst, in which he attempts to account for these anomalies by the supposition of a magnetic state of the masses and balls.

[†] Verisimile est quòd copia materiæ totius in Terrâ quasi quintuplo vel sextuplo major sit quam sit tota ex aqua constaret.—Princip., lib. iii. prop. 10.

Memoirs of the Academy of Sciences for 1824, he has shewn that the disturbing forces of the sun and moon cannot produce, in the variables which determine the relative position of the earth's axis, any secular inequalities which might ultimately become sensible. He also found that the velocity of rotation could not be sensibly affected by the same cause; whence it followed that the length of the sidereal day is not subject to any variation depending on the action of the sun or moon.

The conclusion at which Poisson arrived is fully borne out by an examination of ancient eclipses. It is clear that if the diurnal motion of the earth be variable, the period comprised between two successive returns of a star to the same position relative to the horizon cannot constitute a fixed standard of time; and consequently the interval between the present time and any remote epoch, when expressed in terms of the sidereal day as determined by modern observation, will not correspond to the number of revolutions which the earth has actually accomplished, as indicated by historical records. It will follow also that if the diurnal motion constantly vary in the same direction, the difference between the computed and the historic epochs will increase with the lapse of time. We may therefore conclude that the places of the planets, when computed for any remote epoch by means of the modern value of the sidereal day, will differ from their actual places as assigned by the recorded observations of astronomers; and this difference will be more considerable for the moon than for any other body, on account of her rapid motion. Now, if the rotation of the earth is really invariable, the longitudes of the sun and moon, when computed for any ancient lunar eclipse, ought not to differ from 180° by a quantity greater than the sum of their semi-diameters, and the difference may naturally be expected to be in many cases much less. In the *Connaissance des Temps* for 1800, there is a paper by Laplace containing calculations of this nature for 27 eclipses recorded by the Chaldeans, Greeks, and Arabians, and the results in all instances go to prove the invariability of the sidereal day. The greatest quantity by which the distance between the centres of the sun and moon differs from 180°, amounts to 27′ 41″ and relates to an eclipse which happened in the year 382 A.C. Even this difference, however, falls short of the sum of the solar and lunar semi-diameters, and, therefore, does not preclude the possibility of an eclipse having taken place. It is clear, then, that the length of the sidereal day is not subject to any sensible inequalities, since the conclusions deducible from the supposition of its being constant accord so well with observation. In order to illustrate this interesting fact more fully, Poisson assumed that the length of the day had diminished by a ten-millionth part since the most ancient of the Chaldean eclipses, which happened in the year 720 A.C.; and then, calculating the longitudes of the sun and moon for that epoch, he found them to differ from 180° by 34′. This quantity being greater than the sum of the semi-diameters of the two bodies, is incompatible with the occurrence of an eclipse, whence it follows that, during the lapse of 2500 years, the length of the sidereal day has not altered by so much as the ten-millionth part.

An interesting question which Laplace first considered in connexion with the length of the sidereal day is that relating to the mean temperature of the earth. Various facts concur to strengthen the opinion that the earth was originally a fluid mass, which subsequently became solid by a process of cooling, which is even still going on. This gradual diminution of temperature being necessarily accompanied by a corresponding diminution of the earth's mass, the particles composing the latter will all in consequence approach nearer to the axis. Now, it follows immediately from a well-known principle in Mechanical Science, that when a body is endued with a rotatory motion, and is not exposed to the action of any extraneous forces, the principal moment of inertia, or, in other words, the sum of the products formed by multiplying each particle into its angular velocity, and the square of its distance from the axis of rotation, is a constant quantity. The number of particles then remaining the same, if their distances from the axis be diminished, their angular velocities must be increased, and *vice versâ*. Hence, if the dimensions of the earth be in a state of contraction from cooling, the velocity of rotation must be increasing, and the length of the sidereal day must be diminishing. It is not difficult, however, to show that if the earth is really becoming cooler, the diminution of temperature must be proceeding at a very slow rate. We have seen that, during a period of 2500 years, the sidereal day has not been shortened by so much as the ten-millionth part. Now, the principles of Mechanics teach us that this result would ensue if the earth's radius experienced a diminution of only one twenty-millionth part of its length. It follows, then, that during the period which has elapsed since the earliest Chaldean observations, the mean temperature of the earth has not varied to such an extent as to cause the terrestrial radius to contract by one twenty-millionth part of its length.

The important question of the Tides has recently attracted considerable attention in this country. The *Encyclopædia Metropolitana* contains a valuable essay on the mathematical part of the subject, by Mr. Airy, founded on the theory of undulations. Sir John Lubbock and Dr. Whewell have been engaged during many years in determining the laws of the tides by observation, and in tracing their connexion with the places of the sun and moon. The results to which they have been conducted by their researches are contained in a series of admirable papers, which continue to be published from time to time, in the volumes of the Royal Society. These distinguished philosophers are now endeavouring to do for the theory of the tides what astronomers had done for the lunar theory previous to the establishment of the theory of gravitation. Let us hope that their efforts will be attended with similar success, and that the day is not very remote when this important branch of Physical Astronomy will be in a condition to invite the researches of the geometer, and to reward him with a rich harvest of results.

It is clear that if the sun and moon by their action on the earth occasion oscillations in the waters of the ocean, they ought to produce a similar movement in the atmospheric fluid. Laplace investigated the theory of this subject upon a somewhat restricted hypothesis*, and Bouvard undertook an extensive series of observations of the height of the barometer, with the view of detecting periodical oscillations depending on the places of the disturbing bodies. The effects, however, were so very minute as to be almost entirely masked by irregularities arising from other causes, and no satisfactory conclusion could be deduced from the observations.

The oscillations of the atmospheric tide will manifestly be greatest near the equator, and the most favourable station for detecting them would be some small island in the middle of the ocean, because the barometer would in that case be least liable to be affected by the fluctuations arising from the irregularities of the earth's surface. With the view of throwing some light on this delicate subject Captain Lefroy, of the Royal Artillery, undertook a series of barometrical observations at the island of St. Helena. These observations were conducted solely with reference to the place of the moon, as the effect of the sun's influence was naturally expected to be insensible. They extended from August, 1840, to December, 1841, and therefore comprised a period of seventeen months. The mean result of these observations clearly indicated the existence of a lunar atmospheric tide. It gave 28.2714 inches for the height of the barometer when the moon was on the meridian, and 28.2675 when she was on the horizon. The difference was therefore .0039 inches*. The observations were subsequently resumed by Captain Smythe in October, 1842, and were continued till September, 1843. The average of all the results obtained during this period gave .00255 inches for the excess of the altitude of the barometer, when the moon was on the meridian, over the altitude when she was six hours distant from it. The observations for the following two years were compared together by Lieutenant Colonel Sabine at Woolwich, and the results he derived from them presented a satisfactory accordance with those previously obtained by Lefroy and Smythe. The average excess of barometrical pressure during this period amounted to .00365 inches, or in round numbers to .004 inches. It is manifest that the effect of the moon's action ought to be greatest when she is in perigee, and least when she is in apogee. This is unequivocally indicated by the observations, as appears from the following results of the mean barometrical excess, obtained by a comparison of observations made when the moon was on the medidian, and when she was six hours distant from it:—

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* Méc. Cél. liv. iv. chap. iv., liv. xiii. chap. vii. The temperature of the atmosphere was assumed to be uniform, and the density at each point proportional to the compressing force.

Mean excess of pressure derived—

from 13 epochs of perigee, between Oct. 1843 and Sept. 1844.	.00407 inches.
from 13 epochs of perigee, between Oct. 1844 and Sept. 1845.	.00394 ,,
from 13 epochs of apogee, between Oct. 1843 and Sept. 1844.	.00341 ,,
from 14 epochs of apogee, between Oct. 1844 and Sept. 1845.	.00347 ,,

These quantities are small, but still they are sufficiently sensible to establish beyond doubt the existence of oscillations in the atmosphere, similar to those which affect the waters of the ocean. This is not the least interesting of the many facts in physical science which would have for ever escaped detection, if their existence had not been suggested by the theory of gravitation.

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* Phil. Trans., 1847.

CHAPTER XII.

Introductory Remarks.—Ancient Observations of Uranus.—Calculation of Tables of the Planet by Delambre.—Tables of Bouvard.—Irregularities of the Planet.—Speculations respecting their Origin.—Errors of Radius Vector.—Researches of Geometers.—Bessel.—Adams.—Inverse Problem of Perturbation.—Account of Adams' Researches relative to the existence of a Planet exterior to Uranus.—Results obtained by him.—Researches of the French Astronomers on the Theory of Uranus.—Eugene Bouvard.—Le Verrier.—Account of his Researches.—Near agreement of his Results with those of Adams.—Steps taken by Airy and Challis for the purpose of discovering the Planet.—New Results obtained by Adams.—Explanation of errors of Radius Vector.—Account of the second part of Le Verrier's Researches on the Trans-Uranian Planet.—Address of Sir John Herschel at Southampton.—The Planet discovered at Berlin by Galle.—Admiration excited by the Discovery.—Account of Challis' Labours.—Public Announcement of Adams' Researches.—Impression produced by it.—Historical Statement of the Astronomer Royal.—Publication of the Researches of Le Verrier and Adams.—Remarks suggested by the Discovery of the Planet.

THE Theory of Gravitation is not more remarkable for the sublimity of its results than for its varied and effective character, when considered as an instrument applicable to the discovery of truth. By unfolding its principles, the astronomer, without leaving his observatory, has been enabled to determine the distances of the sun and moon from the earth, to weigh the planets as in a balance, and to educe order and stability from the countless irregularities of their motions. It has conducted him to a knowledge of the figure of the earth by merely watching the motion of the moon or the swinging of a pendulum, and it supplies the means of ascertaining the figures of the celestial orbs without measuring their apparent dimensions. The eccentric aberrations of comets, the ebbing and flowing of the tides, and the oscillations of the atmosphere, all equally attest the value of its guidance in exploring the hidden operations of nature. But a still more striking triumph of this magnificent theory was reserved for our own day, when the mathematician, by meditating in his chamber upon its principles, has succeeded in revealing the existence of a new planetary world, vastly exceeding the earth in magnitude, which had hitherto escaped the scrutinies of astronomers, aided by all the powerful appliances of optical science.

Soon after the discovery of Uranus by Sir William Herschel in 1781, it was ascertained that astronomers had observed it on many previous occasions without recognising it to be a planet. Even as early as 1690, Flamstead had designated it as a star of the sixth magnitude; and from that year, down to 1781, astronomers had determined its position no fewer than nineteen different times, under an erroneous impression of its real nature *.

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* Bode first discovered two ancient observations of Uranus; one in the *Historia Celestis* of Flamstead (the observation of 1690), and the other in one of Mayer's Catalogues. Soon afterwards Lemonnier detected three positions of the planet among his own observations. Bessel, while engaged in reducing Bradley's observations, found that the position of Uranus

These observations have proved eminently serviceable in conducting astronomers to the true theory of the planet's motion; for, as it takes eighty-four years to effect a complete revolution round the sun, it is manifestly impossible to arrive at definite ideas respecting the irregularities of its motion except by means of observations which are considerably distant from each other. In 1790, the observations of the planet had become sufficiently numerous to induce the Academy of Sciences of Paris to propose the theory of its motion as the subject of a prize. Delambre was on this occasion the successful competitor. The elliptic elements were determined by a skilful combination of the observations subsequent to the discovery of the planet, and the perturbations occasioned by Jupiter and Saturn were computed by means of the formulæ of the *Mécanique Céleste*. The theory of the planet established upon these principles was found to agree very well with three ancient positions which had been already detected among the observations of Flamstead, Lemonnier, and Mayer. Delambre employed it in calculating tables of the planet, which continued for a few years to represent the actual motion with tolerable accuracy. As time, however, wore on, the discordance between theory and observation constantly increased, and a growing desire was felt by astronomers that the theory should be revised and employed in the calculation of new tables. This task was undertaken by Bouvard, whose tables of the planet appeared in 1821. While engaged in correcting the elements for this purpose, he experienced a singular cause of perplexity. He found that the earlier observations of Flamstead, Mayer, Lemonnier, and Bradley, might all be satisfied by an ellipse of a determinate form and position; but he attempted in vain to include in the same orbit the observations of the planet made subsequently to its discovery, although these, on the other hand, might also be satisfied by an ellipse, the elements of which were different from those of the other. "The construction of the tables, then," says Bouvard, "involves this alternative:—if we combine the ancient observations with the modern, the former will be sufficiently well represented, but the latter will not be so, with all the precision which their superior accuracy demands; on the other hand, if we reject the ancient observations altogether, and retain only the modern, the resulting tables will faithfully conform to the modern observations, but will very inadequately represent the more ancient. As it was necessary to decide between these two courses, I have adopted the latter, on the ground that it unites the greatest number of probabilities in favour of truth, and I leave to the future the task of discovering whether the difficulty of reconciling the two systems is connected with the ancient observations, or whether it depends on some foreign and unperceived cause which may have been acting upon the planet."

Bouvard accordingly rejected the ancient observations, and constructed his tables exclusively on the basis of those comprehended between the dis-

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had been on one occasion determined by that astronomer. Burchardt, having made new researches into Flamstead's *Historia Celestis*, discovered four additional observations of the planet, one on the 2nd April, 1712, and the other three on the 4th, 5th, and 10th of March, 1715. Bouvard having carefully inspected all Lemonnier's observations found ten more positions of the planet. It is remarkable, that of six observations by that astronomer, two were made by him on two consecutive days, and the other four on four consecutive days. If it had only occurred to him to compare together his observations on either of these occasions, he could not fail to have anticipated Herschel in the discovery of the planet. Le Verrier, in his investigation of the theory of Uranus, rejects Flamstead's observation of the 5th March, 1712, and adopts another by the same astronomer dated the 29th April, 1712, without, however, mentioning the person who first covery of the planet in 1781 and the year 1820. These tables, like Delambre's, presented at first a very satisfactory accordance with observation, but after only a few years had elapsed they began to be sensibly erroneous, and they continued henceforward to deviate more and more from observation. The difficulty connected with the theory of the planet now assumed a more definite character, for it clearly did not depend on the ancient observations, since those of a more recent date were becoming equally irreconcileable with the tables.

Bouvard was in all probability the first person who was led to ascribe the irregularities of Uranus to the perturbations produced by an unknown planet. The passage from his tables already cited contains an obscure allusion to this hypothesis; but we should not be warranted on such grounds in awarding to him the merit of its original conception, if we did not possess abundant evidence from other sources that he had a strong persuasion of its truth. In fact, the venerable astronomer appears to have clung with tenacity to this explanation of the anomalies of the planet, down to the latest years of his life. Nor, indeed, was the idea of a planet exterior to the acknowledged boundaries of the solar system altogether new. Even as early as the year 1759, we find Clairaut speculating upon the probability of Halley's comet being exposed to the disturbing action of a planet too remote to be visible[*].

Dr. Hussey was unquestionably one of the first astronomers who had the sagacity to divine the true cause of the irregularities in the motion of Uranus. Writing to Mr. Airy, in 1834, he mentioned, that having carefully determined the position of Uranus on several occasions during the preceding year, he was led to examine closely Bouvard's tables of the planet, and he remarked that the strange inconsistency of the ancient and modern observations had suggested to him the possible existence of some disturbing body beyond Uranus[†]. He intimated, also, a design of sweeping for the planet with his large reflector, provided one or more positions of it were determined empirically. He added, in the same letter, that he had some conversation on the subject with Bouvard, who, in reply, asserted that he entertained a similar opinion with respect to the cause of the planet's irregularities, and had been corresponding with Hansen respecting it. Hansen replied, that one exterior planet would not account for the errors of the tables, and that two planets were necessary for this purpose.

As the errors of the tables continued to increase, the conviction of their real origin was constantly gaining ground among astronomers. M. Valz, of Marseilles, writing to M. Arago in 1835, on the subject of Halley's comet, made the following interesting remarks relative to the probable existence of a Trans-Uranian planet. "I would rather have recourse to an invisible planet situated beyond Uranus. Its period, according to the order of the distances, would be at least triple that of the comet, so that the perturbations of the latter would nearly recur at the close of every three revolutions, and the calculations made for four or five well-established cycles would enable us to trace them. Would it not be admirable to arrive thus at a knowledge of the existence of a body which cannot be perceived."*

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[*] See page 104 of this work.

[†] *Account of some Circumstances historically connected with the Discovery of the Planet exterior to Uranus, by G. B. Airy, Esq., Astronomer Royal.* This important collection of correspondence relating to the theoretical discovery of the planet Neptune was read before the Astronomical Society on the 13th November, 1846, and was published both in the *Monthly Proceedings,* and also in vol. xvi. of the *Memoirs* of the same

Guided by the same sagacious and philosophical views, but reasoning upon more legitimate grounds, Mrs. Somerville alludes in the following terms to the probable existence of an unknown planet in the third edition of her interesting little work on "*The Connexion of the Physical Sciences.*"† "The tables of Jupiter agree almost perfectly with modern observations; those of Uranus, however, are already defective, probably because the discovery of the planet in 1781 is too recent to admit of much precision in the determination of its motions, or that possibly it may be subject to disturbances from some unseen planet revolving about the sun beyond the present boundaries of our system. If, after the lapse of years, the tables formed from a combination of numerous observations should still be inadequate to represent the motions of Uranus, the discrepancies may reveal the existence, nay, even the mass and orbit of a body placed for ever beyond the sphere of vision." One more of such extracts will shew how strong the persuasion now was, respecting the existence of a Trans-Uranian planet. Professsor Mädler, in his "*Treatise on Popular Astronomy*," after remarking that Uranus might have been discovered by means of the perturbations it produces in the motion of Saturn, alludes in the following prophetic terms to the probability of discovering a planet exterior to Uranus. "Applying this conclusion to a body beyond Uranus, we approach a planet acting upon and disturbing it. We may, indeed, express the hope that analysis will one day or other solemnize this, her highest triumph, making discoveries with the mind's eye in regions where, in our actual state, we are unable to penetrate."

The steadiness with which the errors of the tables continued to increase rendered the necessity of their explanation more and more indispensable. In 1835, the computed geocentric longitudes of the planet exhibited a discordance with observation amounting to 30″; in 1838 the discordance amounted to 50″, and in 1841 to 70″. Nor were these errors simply the effects of errors in the heliocentric longitude of the planet. On the con trary, Mr. Airy clearly shewed, by means of his observations of the planet made at Cambridge in the years 1833–34–35, and at Greenwich in 1836, that the radius vector of the tables was also considerably in error.

The time had now arrived for the geometer to apply the powers of analysis to the investigation of these anomalies. The illustrious Bessel appears to have entertained the idea of attacking this important problem. On the occasion of his visit to England in 1842, he expressed to Sir John Herschel his conviction that the irregularities in the motion of Uranus were occasioned by the action of some unknown planet, and he added, that as soon as he should obtain leisure from other researches in which he was engaged he would undertake the determination of its actual position‡. It appears that, as a preliminary step to the inquiry, he had instructed M. Flemming, a young German astronomer, to reduce with great care all the observations of the planet. Flemming executed the task assigned to him; but, unfortunately, soon afterwards, Bessel was seized with the illness which proved fatal to him, and the inquiry was not prosecuted further§.

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* See the *Comptes Rendus*, tome xxiv. p. 35.

† Published in 1836.

‡ *Athenæum*, October 3rd, 1846.

§ M. Flemming died soon after he completed the calculations. The reduced observations are now in the possession of M. Schumacher of Altona. See Sir John Herschel's Notice of the life of Bessel, in the sixteenth volume of the *Memoirs of the Astronomical Society*.

The theory of Uranus was now about to be submitted to a severe examination by a young mathematician of England: Mr. J. C. Adams, while still pursuing his studies at St. John's College, Cambridge, had purposed investigating the cause of the strange irregularities in the motion of the planet. His attention was first drawn to the subject by Mr. Airy's allusion to those anomalies in his valuable Account of the Progress of Astronomy in the nineteenth century, contained in the Report of the British Association for 1832. Mr. Adams, on referring to his note-book at a subsequent period, found the following memorandum inserted in it under the date of the 3rd July, 1841:—"Formed a design in the beginning of this week of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for, in order to find whether they may be attributed to the motion of an undiscovered planet beyond it, and if possible thence to determine approximately the elements of its orbits, &c., which would probably lead to its discovery."*

Mr. Adams having taken his degree in January, 1843, proceeded to consider the subject of his future researches. The problem which offered itself to him for solution may be stated in the following terms:—Assuming that Uranus was disturbed by some unknown body, to determine the position of the latter, its mass, and the elements of its orbit, by means of the irregularities it produces in the motion of the disturbed planet.. This problem being the inverse case of the ordinary problem of planetary perturbation is manifestly of a very intricate character, and demands the resources of a very refined analysis for its successful treatment. Nay, it is even more complicated than at first it appears to be, for the elements of Uranus, which form the basis of the geometer's researches, are vitiated by the action of the unknown planet, and their rectification, however desirable, can only be effected by the actual solution of the problem. In order to acquire clear ideas upon this point, the reader must bear in mind that the fundamental or mean orbit of Uranus is an ellipse, in which it would constantly revolve if it was exposed solely to the attractive influence of the sun, and that the action of the other bodies of the system tends merely to occasion minute excursions of the planet now on one side and now on the other side of this orbit. It is manifest, then, that in determining the elliptic elements of a planet's motion by means of its positions as assigned by observation, the effects of perturbation must be subducted from the co-ordinates of each position, in order that the planet may be reduced to the place which it would occupy in the mean elliptic orbit. Now, when Delambre and Bouvard were severally engaged in determining the elements of Uranus, they applied the perturbations due to Jupiter and Saturn as corrections to the positions derived from observation; but, as they did not take into account the action of the unknown planet, it is manifest that, if the latter really existed, the final values of the heliocentric co-ordinates, as determined by each of these astronomers, must have failed to represent the elliptic positions of the planet. Hence, the elements deduced from these erroneous data were necessarily inaccurate, and it is manifest that this circumstance alone would have the effect of distorting the orbit of the planet, and thereby occasioning discordances between the observed and computed values of the longitude, latitude, and radius vector. The errors of the tables of Uranus were, therefore, attributable to two distinct causes, being due partly to the errors in the elements of the planet's own orbit, and partly to the disturbing action of the unknown planet. These two causes of error, although totally opposite in their natures, had both a common origin, and their effects on this account were so thoroughly entangled in each other, that it was impossible to investigate either independently. This circumstance, as may well be supposed, vastly increased the difficulty of the problem. If it had been possible to ascertain what portion of the irregularities of the planet was in each case due to the errors of the elements, the values of these errors might have been determined by the ordinary methods of astronomy; and when once the elements were thus corrected, it would have been undoubtedly a less arduous task to ascend from them to the elements of the unknown planet. But it clearly follows, from the remarks we have already made, that the elements of Uranus cannot be determined without taking into account the whole effects of planetary perturbation, and this again implies a knowledge of what is the very object of inquiry, namely, the elements and mass of the unknown planet. The problem, then, admits of being completely treated only by a method which shall embrace in one simultaneous investigation the corrections to the elements of Uranus and the elements and mass of the unknown planet. Now, the elements of a planet are six in number. The problem, when considered in all its generality, will therefore involve thirteen unknown quantities. As, however, it appeared from observation that the perturbations of Uranus in latitude were very small, the two planets might be supposed, for all the purposes of a first approximation, to revolve in the same plane, and by this simplification the number of unknown quantities was reduced to nine. These were:—the corrections to the mean distance, the eccentricity, the longitude of the perihelion, and the epoch or mean longitude of Uranus; and the absolute values of the same elements relative to the unknown planet together with its mass. Now, in the case of one planet acting upon another and disturbing its motion, the theory of perturbation enables the geometer to express the corrections to the co-ordinates of the disturbed planet in terms of these nine unknown quantities; and, if the hypothesis of an unknown planet was true in the present case, these corrections ought to account for the differences between the observed and computed positions of the planet Uranus. Hence, by comparing the analytical formulæ for the corrections to the co-ordinates of Uranus with the numerical errors of the tables, a number of equations of condition are formed, and the problem is finally reduced to the elimination of the unknown quantities from these equations. It may readily be imagined that this operation is attended with difficulties of no ordinary kind. In fact, only a thorough acquaintance with the nature of planetary perturbation, and a complete command of analysis, combined with consummate skill in its application, will suggest to the mathematician such artifices as may lead to the disengagement of the unknown quantities from the complicated expressions in which they are involved.

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* Mem. Ast. Soc. vol. xvi.; see also the Nautical Almanac for 1850.

Nor does the mere elimination of the unknown quantities constitute the only difficulty attending the solution of the problem. Unless the equations of condition be skilfully combined together, it may happen that the values of the unknown quantities, although deduced by a strictly legitimate process of reasoning, will prove totally erroneous when considered as physical results. This will be readily understood by the reader when he takes into account the mode in which the equations of condition are formed. In each case the error of the planet's place is expressed algebraically in terms of the nine unknown condition is formed by putting this expression equal to the actual error as determined by a comparison of the observed and computed places of the planet. Every tabular error furnishes a corresponding equation, and thus the number of equations of condition is limited only by the number of independent observations. Now, if the latter were mathematically accurate, the equations of condition would all be consistent with each other, so that any nine of them would suffice for the determination of the nine unknown quantities of the problem, and the values hence deduced would rigorously satisfy all the other equations. But, as all observations are necessarily more or less erroneous, the errors resulting from a comparison of the observed and calculated places of the planet will not be *wholly* dependent on the unknown quantities of the problem; and the equations of condition being vitiated in consequence, will prove to a certain extent incompatible with each other, so that every nine equations selected for the determination of the unknown quantities will conduct to different results. With a view to elude this source of error, the number of equations of condition is generally made to exceed in a great degree the number of unknown quantities, and the grand object, then, is to combine them together, so that the errors of observation may destroy each other, and a system of equations may finally be arrived at which will assign the true values of the unknown quantities.

Mr. Adams first proceeded to examine the perturbations produced in the motion of Uranus by the other planets, in order to assure himself beyond doubt that the errors of Bouvard's tables did not proceed from an erroneous application of the existing theory*. For this purpose he recomputed the principal perturbations due to Jupiter and Saturn, and introduced some new inequalities which had been first pointed out by Hansen†. He also took into account the correction to Jupiter's mass, to which recent researches had conducted Astronomers. Notwithstanding these improvements, the theory still failed to represent the motion of the planet. Two important advantages were, however, gained by these preliminary labours. In the first place, it was clearly established that the cause of the irregularities must be sought elsewhere than in the development of the actual theory. In the second place the application of the improvements had the effect of exhibiting the errors of the tables as residual facts wholly dependent on some extraneous influence, and consequently they now assumed a more precise and definite character than they had previously done. The first point to be decided in this inquiry was, the most suitable mode of exhibiting the irregularities in the motion of the planet. We have mentioned already that both the heliocentric longitude and radius vector had been discovered to be sensibly in error. Now it may be remarked that the problem admits of solution by employing as the basis of investigation an adequate number of the errors of either of these co-ordinates, independently of those of the other. In point of fact, however, the errors of the radius vector were too inconsiderable to be employed with safety in such an inquiry, although they might be subsequently used with advantage in verifying results otherwise derived. Mr. Adams, therefore, had recourse to the errors of heliocentric longitude, which in some instances amounted to 2′ or 3′, and extended over a period embracing more than a revolution and a half of

* Mem. Ast. Soc., vol. xvi.; Naut. Alm., 1850.

the planet round the sun. But here a difficulty occurs, the nature of which it is necessary briefly to explain. The earth being the place from which all observations are made, the position of a planet in the zodiac is determined by its geocentric longitude, or its angular distance from the vernal equinox relative to the centre of the earth. But, as the sun is the natural centre of the planet's motion, the theory of the latter assigns only the heliocentric longitude, or the distance from the vernal equinox relative to the centre of the sun. In order, then, to compare the observations of the planet with the results of theory, it is necessary to pass from the geocentric longitude, derived immediately from observation, to the corresponding value of the heliocentric longitude. This object may be effected by a simple process of trigonometry, if we know the length of the radius vector of the planet, and the position of the earth in her orbit, corresponding to the time of observation. The former of these data may be obtained from the tables of the planet, and the latter from the solar tables. It is manifest, however, that if the tabular radius vector of the planet be erroneous, the computation will be vitiated, and a false value will be assigned to the heliocentric longitude. The question then arises, how are we to pass from the geocentric longitude of the planet to the heliocentric longitude without complicating the result with the effect of the error of the radius vector. This object is accomplished *per se* for all observations made when the planet is in opposition, for the sun, the earth, and the planet being then in the same straight line, the geocentric and heliocentric longitudes will necessarily coincide, and they will both retain the same common value, whatever be the length of the planet's radius vector. Even if the observations should not be made when the planet is actually in opposition, the heliocentric longitudes may be obtained free from the errors of radius vector by skilfully combining together the observations made before and after opposition*. Mr. Adams in this manner deduced from the observations of Uranus twenty equidistant values of heliocentric longitude, for the period included between 1780 and 1840. Comparing these with the tabular values he obtained a corresponding series of errors in heliocentric longitude, which, with those derived from the ancient observations of the planet, formed the data of his final investigation of the problem.

Mr. Adams commenced his researches by supposing the orbit of the planet to be circular, and its mean distance from the sun double the mean distance of Uranus. It was probable from analogy that both these sup positions did not differ widely from the truth[*]. Without some assumed value of the mean distance, the operation of eliminating the unknown quantities would have been totally impracticable; but, when this object was once accomplished, a more accurate value might be afterwards arrived at, either by varying the hypothesis and comparing the results with those derived in the first instance, or by introducing into the second solution a correction to the assumed mean distance in the shape of a new unknown quantity. The results which Mr. Adams obtained by means of his first essay were sufficiently satisfactory to encourage him to pursue his researches. The observations which he employed were most defective throughout the period included between the years 1818 and 1826, and it was precisely in that period also that the errors of his theory were greatest. This circumstance induced him to apply to the Astronomer Royal, through his friend Professor Challis, for the Greenwich Observations of the planet then in process of reduction. Professor Challis' letter to the Astronomer Royal on this occasion is dated the 13th February, 1844[†]. He mentions in it that Mr. Adams, a young friend of his, was engaged in researches on the theory of Uranus. He then specifies the nature of his request, expressing also a desire to know whether, in comparing the observations of the planet with the theory, any alteration had been made in the tables of Bouvard, except that depending on the correction to the value of Jupiter's mass. The Astronomer Royal a day or two afterwards returned a suitable reply to Professor Challis' letter, and kindly transmitted, for the use of Mr. Adams, all the Greenwich Observations of the planet from 1750 to 1830 completely reduced[‡].

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* If we suppose the radius vector of the tables to be too small, then, before the earth arrives in opposition, the planet will appear behind its computed place; and, on the other hand, when the earth has passed opposition, it will appear in advance of its computed place. Hence it is not difficult to perceive that these opposite effects may be destroyed by a suitable combination of observations made before and after opposition. The same remark will manifestly apply, if the tabular radius vector be too large; the only difference being, that in this case the planet before opposition will appear in advance of its computed place, and after opposition will appear behind it. After Kepler made the memorable discovery that the orbit of Mars was not a circle, but retired within that curve towards the mean distance, his active imagination soon devised a theory in which the planet was supposed to move in an oval, the distance of which varied according to a certain law. This hypothetic orbit was, however, too much compressed in the sides to represent the true ellipse of the planet; and, accordingly, Kepler mentions that David Fabricius, to whom he communicated his theory, remarked to him that the observations of the planet before and after opposition indicated that the distances of the oval were all too short. "So nearly," says he, "did that astronomer anticipate me in the discovery of the true orbit of the planet." —*De Motibus Stellæ Martis*, cap. lv. p. 266.

Furnished with these valuable data, Mr. Adams was now in a condition to pursue his researches with the prospect of ultimate success. He accordingly undertook a more comprehensive investigation of the problem by considering the orbit of the disturbing planet to be elliptic. By repeating the approximation several times, introducing on each occasion more and more terms of the perturbation, he succeeded in gradually reducing the errors of Uranus, until its computed places finally exhibited a very general and satisfactory accordance with those derived from observation. In the autumn of 1845, he considered his researches were so far matured as to warrant the presumption that his results might be employed with a strong probability of success in searching for the new planet. Accordingly in the month of September, before leaving Cambridge, he placed in the hands of Professor Challis a paper containing numerical values of the mass, the epoch, the eccentricity, and the longitude of the perihelion of the disturbing planet, together with its geocentric longitude for the 30th of September, 1845[*]. Mr. Adams was also anxious to communicate his results to the Astronomer Royal, and for this purpose the kind offices of Professor Challis were again tendered. The letter of Professor Challis to the Astronomer Royal on this occasion is dated the 22nd September, 1845[†]. He mentions in it that Mr. Adams had completed his investigation relative to the supposed existence of a planet exterior to Uranus; that his researches were founded on the observations which Mr. Airy was kind enough to send him some time previously, and that he was desirous of communicating his results personally to him. He added, also, that from Mr. Adams' character as a mathematician, and his practice in calculation, he considered that his conclusions had been accurately deduced from his premises. Mr. Airy happened to be from home when Mr. Adams called at Greenwich, but a few days afterwards he wrote a letter to Professor Challis[‡], expressing the strong interest he felt in the subject of Mr. Adams' researches, and mentioning that he would be happy to hear by letter from him respecting them.

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[*] The following curious law regulating the mean distances of the planets from the sun was first announced by Bode, the German astronomer. It is said, however, that he was indebted for the original notion of it to Titian, a professor of Wurtemberg.

Mercury . . . . $= 4 = 4$
Venus . . . . $= 4 + 3.2^0 = 7$
The Earth . . . . $= 4 + 3.2^1 = 10$
Mars . . . . $= 4 + 3.2^2 = 16$
Ceres (as the mean of the smaller planets) $= 4 + 3.2^3 = 28$
Jupiter . . . . $= 4 + 3.2^4 = 52$
Saturn . . . . $= 4 + 3.2^5 = 100$
Uranus . . . . $= 4 + 3.2^6 = 196$

If we compare these numbers with the actual distances (supposing the earth's distance to be in both cases represented by 10), we shall find a very remarkable agreement. It is easy to perceive that as the planets recede from the sun, their successive distances become more nearly double each other.

[†] Airy's Historical Statement of circumstances connected with the discovery of the planet beyond Uranus, No. 6.

On one of the last days of October, 1845, Mr. Adams called at the Royal Observatory, and left a paper containing the elements of the new planet, and the errors resulting from a comparison of the improved theory of Uranus with a great number of observations of the planet from 1690 to 1840[§]. These errors, which, by Bouvard's tables, amounted frequently to 3′, were now for the most part little more than 1″. The following are the elements of the planet, as given by Mr. Adams in this important paper:—

Mean distance from the sun, the earth's distance being represented by unity .	38.4
Mean sidereal motion in 365.25 days .	1° 30′.9
Mean longitude, 1st October, 1845 . .	323°.34′
Longitude of perihelion	315.55
Eccentricity	.1610
Mass (that of the sun being unity) . .	.0001656

These elements give 7° 39′ for the mean anomaly of the planet on the 1st October, 1845, and 3° 3′ for the equation of the centre. Hence, by adding the latter to the mean longitude, we get 326° 37′ for the true heliocentric longitude of the planet. Now, if we compute the place of the planet for the same instant by means of the elements derived from observation, we shall obtain 324° 48′ for the true longitude[‖]. The difference, therefore, between the actual and theoretical places was only 1° 49′; and it is clear that if we had employed, as the epoch of comparison, the 23rd September, 1846 (the epoch of the actual discovery of the planet), the result would not have been materially different.

Thus it appears that, as early as the month of October, 1845, seven months before any other person had arrived at a similar conclusion, Mr. Adams had solved the inverse problem of planetary perturbation; that, by means of his solution, he had discovered, theoretically, the existence of a planet exterior to Uranus; and that he had assigned to

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[*] Challis' Report to the Syndicate of the University of Cambridge; see also Phil. Mag., 1847.

[†] Airy's Hist. Statement, No. 9.

[‡] Airy's Hist. Statement, No. 10.

[§] Ibid, No. 11.

[‖] The elements here employed are those deduced from observation by Professor the unknown body a place in the heavens which was subsequently found to differ little more than one degree from its actual place. All that was now wanting, therefore, to assure both to Mr. Adams and to his country the undivided honour of one of the noblest discoveries recorded in the annals of science, was some zealous observer to give effect to his results, by carefully searching the heavens in the vicinity of the place indicated by his theory as that occupied by the planet. If such a scrutiny had been undertaken, and prosecuted for some time, it would beyond all doubt have resulted in the actual discovery of the planet, and the name of Adams would have been alone associated with that remarkable triumph of science. Such a consummation was not destined to be the reward of Mr. Adams; but this circumstance does not detract in the slightest degree from the merit of his researches; for it is now universally admitted that he was the first theoretical discoverer of the planet, and that, as far as the task of the mathematician was concerned, he left no part of the problem relative to the determination of its actual position to be completed by others.

A few days after the Astronomer Royal received Mr. Adams' paper containing the elements of the new planet, he wrote a letter to that gentleman, thanking him for his communication, and stating that the results appeared to him very satisfactory*. He mentioned, however, that he was desirous of knowing whether the theory gave an equally satisfactory account of the errors of radius vector, directing Mr. Adams' attention to the fact of these errors having recently become very considerable.

Mr. Airy deemed the explanation of the errors of radius vector to be an *experimentum crucis*, which would decide, in his mind, the question of the legitimacy of Mr. Adams' researches, and he naturally enough waited for the reply of that gentleman before taking any active steps for the purpose of discovering the planet. Unfortunately, Mr. Adams did not consider it a matter of importance to transmit prompt information to Mr. Airy on this point; and it was not until nine months afterwards, when his results were confirmed by similar researches, prosecuted in another quarter, that any attempt was made to obtain their verification. It must not, however, be inferred, from the fact of Mr. Adams not having returned an immediate answer to Mr. Airy's letter, that he was prevented from doing so by any imperfection in his solution of the inverse problem of perturbation. When once the elements of the unknown planet, and the corrections to the elements of Uranus, have been deduced from the errors of longitude, the errors of radius vector may then be calculated by a direct process which does not involve any analytical difficulties beyond those already vanquished in the *Mécanique Céleste*. We shall again have occasion to advert to this point.

It is necessary to state, before proceeding further, that Mr. Adams' researches on the theory of Uranus were not hitherto made known to the public. The two astronomers whom we have had frequently occasion to allude to in connexion with him, and a few of his friends at Cambridge, appeared to have been the only individuals who were acquainted with the nature of his labours. We shall now give an account of the researches of the French astronomers in connexion with the same subject.

In the month of September, 1845, Eugene Bouvard, nephew of the astronomer Alexis Bouvard, presented to the Institute new tables of Uranus†. They were founded on the observations of the planet made subsequently to its discovery in 1781, and on the anterior observations of Bradley, Lemonnier, and Mayer. The actual motion of the planet was represented better by them than by the tables constructed in 1821, but the residual errors were still, in many instances, too great to be accounted for by any probable errors of observation. This was candidly admitted by the author of the tables, who intimated also his belief that the errors were of such a nature as to give countenance to the opinion entertained by his uncle that they were due to the perturbations produced by another planet*.

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* Airy's Hist. Statement, No. 12. This letter is dated the 5th November, 1845.

† Comptes Rendus de l'Académie, tome xxi. p. 524.

The failure of this laborious attempt to correct the tables of Uranus without the introduction of any new hypothesis, beyond doubt produced a strong impression on the minds of the French astronomers; and it was probably this circumstance which induced M. Arago to propose to his friend, M. Le Verrier, the theory of Uranus as a subject eminently worthy of attention. We have already had frequent occasions to allude to the labours of this talented mathematician. He had recently been occupied with the theory of comets, but, at the suggestion of the illustrious philosopher just mentioned, he temporarily laid aside his researches on that subject, and resolved to institute a searching scrutiny into all the circumstances affecting the motion of Uranus. An account of the first part of his researches appeared in the *Comptes Rendus*, of the Academy of Sciences, for the 10th November, 1845. It contained the result of a rigorous examination of the motion of the planet according to the principles of the actual theory. The only interior planets which produce sensible perturbations in the motion of Uranus are Jupiter and Saturn, and of these the latter exercises the more powerful influence, on account of his comparative proximity to the disturbed planet. Le Verrier determined the value of every inequality depending on the action of Saturn, that exceeded the twentieth part of a second, employing two distinct methods of calculation, in order to assure greater accuracy to his results†. He also computed all the perturbations occasioned by the action of Jupiter. Nor did he confine his researches to the inequalities of the first order of magnitude. All the principal inequalities depending on the squares of the disturbing forces were also taken into account by him‡. A great number of small terms were thus introduced, for the first time, into the heliocentric co-ordinates of the planet, the previous omission of which, he shewed, was alone sufficient to occasion sensible errors in the tables.

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* In a letter from Eugene Bouvard to Mr. Airy, dated the 6th October, 1837, we find also an allusion to the opinion entertained by his uncle, Alexis Bouvard, that the irregularities of Uranus were occasioned by a disturbing planet. After adverting to the inconsistency of the tables with observation, he then proceeds thus: "Cela tient-il à une perturbation inconnue apportée dans le movement de cet astre par un corps situé au-delà! Je ne sais, mais c'est du moins l'idée de mon oncle."—Airy's Hist. Statement, No. 3.

† One of these methods is that to which allusion has been made in page 115 of this work.

‡ When the mutual action of two planets is considered, their perturbations are first computed by supposing them to move in ellipses, and the resulting inequalities are of the first order with respect to the disturbing forces. In a second approximation the perturbations consequent upon the derangements of the motions of the two planets, as assigned by the first approximation, are taken into account, and the inequalities hence derived are of the second order with respect to the disturbing forces. It is important to remark that the inequalities of the second order may be materially modified by the derangements of both planets depending upon the action of a third planet. This happens when the theory of Uranus and Saturn is considered, the action of Jupiter exercising a very sensible influence on the mutual perturbations of those bodies. On account of his great mass and his comparative proximity to Saturn, Jupiter produces very extensive perturbations in the motion of that planet. Le Verrier, who resolved to verify everything by actual calculation, was induced by this circumstance to investigate a great part of the theory of Saturn, even although, in the present case, it was only with the ulterior view of computing the action of that planet upon Uranus.

An account of the second part of Le Verrier's researches appeared in the *Comptes Rendus*, for the 1st June, 1845. After having corrected the theory of the planet, he next proceeded to compare it with the observations, in order to ascertain whether the latter might be satisfied by applying suitable corrections to the elements, or whether it was impossible to account for them by any ellipse whatever, and the perturbations of the known planets. He selected, for this purpose, nineteen ancient observations of the planet, and 260 observations made at the Observatories of Greenwich and Paris, during the period comprised between the discovery of the planet in 1781, and the month of September, 1845. In order effectually to guard against every contingency of error, he undertook the laborious task of reducing anew all these observations. Having employed the results in computing the geocentric co-ordinates of the planet for each observation, he then proceeded to deduce from theory the corresponding values of the co-ordinates. It was necessary for this purpose to determine, in the first instance, the heliocentric co-ordinates of the planet. When these are known, and also the place of the earth in her orbit, a simple calculation, the converse, in fact, of that to which we have already alluded *, will suffice for passing from the heliocentric to the geocentric co-ordinates. Le Verrier therefore calculated the heliocentric longitude, latitude, and radius vector of the planet; and then, by means of these data and the places of the earth in her orbit, derived from the solar tables, he determined the values of the geocentric co-ordinates. Finally, subtracting the value of each co-ordinate from the corresponding value assigned by observation, he obtained the errors of his theory. The question which now offered itself was this: Can these errors be destroyed by applying suitable corrections to the elliptic elements of the planet? Now, it is easy to obtain the analytical expression for the error in geocentric longitude, in terms of the errors of the elements of the planet, these errors being exhibited in indeterminate forms. In the present case, the only elements which it is necessary to take into account are the mean distance, the epoch, the eccentricity, and the longitude of the perihelion; for the effects due to errors in the other two elements are of an inferior order of magnitude, and, in consequence, may be neglected. By putting the algebraic expression for the error of the theory equal to each of the numerical values which he had previously obtained by a comparison of the observed and calculated geocentric longitudes, Le Verrier formed a series of equations of condition, which ought to be satisfied if the discordances between theory and observation were really due to errors in the elements. Every observation gave him a corresponding equation of condition; so that the total number of equations thus formed by him amounted to two hundred and seventy-nine. Setting aside the equations of condition depending on the ancient observations, he formed four final equations, by a suitable combination of all the others, and from them deduced the corrections to the four elements. The motion of the planet, when calculated for a great number of places by means of the new ellipse, resulting from the application of these corrections to the tabular elements, still exhibited discordances with the actual motion, which were generally by far too great to be accounted for by any probable errors of observation.

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* See page 171.

Memoirs of the Academy of Sciences for 1824, he has shewn that the disturbing forces of the sun and moon cannot produce, in the variables which determine the relative position of the earth's axis, any secular inequalities which might ultimately become sensible. He also found that the velocity of rotation could not be sensibly affected by the same cause; whence it followed that the length of the sidereal day is not subject to any variation depending on the action of the sun or moon.

The conclusion at which Poisson arrived is fully borne out by an examination of ancient eclipses. It is clear that, if the diurnal motion of the earth be variable, the period comprised between two successive returns of a star to the same position relative to the horizon cannot constitute a fixed standard of time; and consequently the interval between the present time and any remote epoch, when expressed in terms of the sidereal day as determined by modern observation, will not correspond to the number of revolutions which the earth has actually accomplished, as indicated by historical records. It will follow also that if the diurnal motion constantly vary in the same direction, the difference between the computed and the historic epochs will increase with the lapse of time. We may therefore conclude that the places of the planets, when computed for any remote epoch by means of the modern value of the sidereal day, will differ from their actual places as assigned by the recorded observations of astronomers; and this difference will be more considerable for the moon than for any other body, on account of her rapid motion. Now, if the rotation of the earth is really invariable, the longitudes of the sun and moon, when computed for any ancient lunar eclipse, ought not to differ from 180° by a quantity greater than the sum of their semi-diameters, and the difference may naturally be expected to be in many cases much less. In the *Connaissance des Temps* for 1800, there is a paper by Laplace, containing calculations of this nature for 27 eclipses recorded by the Chaldeans, Greeks, and Arabians, and the results in all instances go to prove the invariability of the sidereal day. The greatest quantity by which the distance between the centres of the sun and moon differs from 180°, amounts to 27′ 41″, and relates to an eclipse which happened in the year 382 A.C. Even this difference, however, falls short of the sum of the solar and lunar semi-diameters, and, therefore, does not preclude the possibility of an eclipse having taken place. It is clear, then, that the length of the sidereal day is not subject to any sensible inequalities, since the conclusions deducible from the supposition of its being constant accord so well with observation. In order to illustrate this interesting fact more fully, Poisson assumed that the length of the day had diminished by a ten-millionth part since the most ancient of the Chaldean eclipses, which happened in the year 720 A.C.; and then, calculating the longitudes of the sun and moon for that epoch, he found them to differ from 180° by 34′. This quantity being greater than the sum of the semi-diameters of the two bodies, is incompatible with the occurrence of an eclipse, whence it follows that, during the lapse of 2500 years, the length of the sidereal day has not altered by so much as the ten-millionth part.

An interesting question which Laplace first considered in connexion with the length of the sidereal day, is that relating to the mean temperature of the earth. Various facts concur to strengthen the opinion that the earth was originally a fluid mass, which subsequently became solid by a process of cooling, which is even still going on. This gradual diminution of temperature being necessarily accompanied by a corresponding diminution of the earth's mass, the particles composing the latter will all in consequence approach nearer to the axis. Now, it follows immediately from a well-known principle in Mechanical Science, that when a body is endued with a rotatory motion, and is not exposed to the action of any extraneous forces, the principal moment of inertia, or, in other words, the sum of the products formed by multiplying each particle into its angular velocity and the square of its distance from the axis of rotation, is a constant quantity. The number of particles then remaining the same, if their distances from the axis be diminished, their angular velocities must be increased, and *vice versâ*. Hence, if the dimensions of the earth be in a state of contraction from cooling, the velocity of rotation must be increasing, and the length of the sidereal day must be diminishing. It is not difficult, however, to show that if the earth is really becoming cooler, the diminution of temperature must be proceeding at a very slow rate. We have seen that, during a period of 2500 years, the sidereal day has not been shortened by so much as the ten-millionth part. Now, the principles of Mechanics teach us that this result would ensue if the earth's radius experienced a diminution of only one twenty-millionth part of its length. It follows, then, that during the period which has elapsed since the earliest Chaldean observations, the mean temperature of the earth has not varied to such an extent as to cause the terrestrial radius to contract by one twenty-millionth part of its length.

The important question of the Tides has recently attracted considerable attention in this country. The *Encyclopædia Metropolitana* contains a valuable essay on the mathematical part of the subject, by Mr. Airy, founded on the theory of undulations. Sir John Lubbock and Dr. Whewell have been engaged during many years in determining the laws of the tides by observation, and in tracing their connexion with the places of the sun and moon. The results to which they have been conducted by their researches are contained in a series of admirable papers, which continue to be published from time to time, in the volumes of the Royal Society. These distinguished philosophers are now endeavouring to do for the theory of the tides what astronomers had done for the lunar theory previous to the establishment of the theory of Gravitation. Let us hope that their efforts will be attended with similar success, and that the day is not very remote when this important branch of Physical Astronomy will be in a condition to invite the researches of the geometer, and to reward him with a rich harvest of results.

It is clear that if the sun and moon by their action on the earth occasion oscillations in the waters of the ocean, they ought to produce a similar movement in the atmospheric fluid. Laplace investigated the theory of this subject upon a somewhat restricted hypothesis*, and Bouvard undertook an extensive series of observations of the height of the barometer, with the view of detecting periodical oscillations depending on the places of the disturbing bodies. The effects, however, were so very minute as to be almost entirely masked by irregularities arising from other causes, and no satisfactory conclusion could be deduced from the observations.

The oscillations of the atmospheric tide will manifestly be greatest near the equator, and the most favourable station for detecting them would admirable to arrive thus at a knowledge of the existence of a body which cannot be perceived."[*]

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* Méc. Cél. liv. iv. chap. iv., liv. xiii. chap. vii. The temperature of the atmosphere was assumed to be uniform, and the density at each point proportional to the compressing force.

Guided by the same sagacious and philosophical views, but reasoning upon more legitimate grounds, Mrs. Somerville alludes in the following terms to the probable existence of an unknown planet in the third edition of her interesting little work on "*The Connexion of the Physical Sciences.*"[†] "The tables of Jupiter agree almost perfectly with modern observations; those of Uranus, however, are already defective, probably because the discovery of the planet in 1781 is too recent to admit of much precision in the determination of its motions, or that possibly it may be subject to disturbances from some unseen planet revolving about the sun beyond the present boundaries of our system. If, after the lapse of years, the tables formed from a combination of numerous observations should still be inadequate to represent the motions of Uranus, the discrepancies may reveal the existence, nay, even the mass and orbit of a body placed for ever beyond the sphere of vision." One more of such extracts will shew how strong the persuasion now was respecting the existence of a Trans-Uranian planet. Professor Mädler, in his "*Treatise on Popular Astronomy,*" after remarking that Uranus might have been discovered by means of the perturbations it produces in the motion of Saturn, alludes in the following prophetic terms to the probability of discovering a planet exterior to Uranus:—"Applying this conclusion to a body beyond Uranus, we approach a planet acting upon and disturbing it. We may, indeed, express the hope that analysis will one day or other solemnize this, her highest triumph, making discoveries with the mind's eye in regions where, in our actual state, we are unable to penetrate."

The steadiness with which the errors of the tables continued to increase rendered the necessity of their explanation more and more indispensable. In 1835, the computed geocentric longitudes of the planet exhibited a discordance with observation amounting to 30″; in 1838 the discordance amounted to 50″, and in 1841 to 70″. Nor were these errors simply the effects of errors in the heliocentric longitude of the planet. On the contrary, Mr. Airy clearly shewed, by means of his observations of the planet made at Cambridge in the years 1833-34-35, and at Greenwich in 1836, that the radius vector of the tables was also considerably in error.

The time had now arrived for the geometer to apply the powers of analysis to the investigation of these anomalies. The illustrious Bessel appears to have entertained the idea of attacking this important problem. On the occasion of his visit to England in 1842, he expressed to Sir John Herschel his conviction that the irregularities in the motion of Uranus were occasioned by the action of some unknown planet, and he added, that as soon as he should obtain leisure from other researches in which he was engaged he would undertake the determination of its actual position[‡]. It appears that, as a preliminary step to the inquiry, he had instructed M. Flemming, a young German astronomer, to reduce with great care all the observations of the planet. Flemming executed the task assigned to him; but, unfortunately, soon afterwards, Bessel was seized with the illness which proved fatal to him, and the inquiry was not prosecuted further[§].

[*] See the *Comptes Rendus*, tome xxiv. p. 35.

[†] Published in 1836.

[‡] *Athenæum*, October 3rd, 1846.

[§] M. Flemming died soon after he completed the calculations. The reduced observations are now in the possession of M. Schumacher of Altona. See Sir John Herschel's Notice of the life of Bessel, in the sixteenth volume of the *Memoirs of the Astronomical Society*.

The theory of Uranus was now about to be submitted to a severe examination by a young mathematician of England. Mr. J. C. Adams, while still pursuing his studies at St. John's College, Cambridge, had purposed investigating the cause of the strange irregularities in the motion of the planet. His attention was first drawn to the subject by Mr. Airy's allusion to those anomalies in his valuable account of the progress of astronomy in the nineteenth century, contained in the Report of the British Association for 1832. Mr. Adams, on referring to his note-book at a subsequent period, found the following memorandum inserted in it under the date of the 3rd July, 1841:—"Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for; in order to find whether they may be attributed to the action of an undiscovered planet beyond it, and if possible, thence to determine approximately the elements of its orbit, &c., which would probably lead to its discovery."*

Mr. Adams having taken his degree in January, 1843, proceeded to consider the subject of his future researches. The problem which offered itself to him for solution may be stated in the following terms:—Assuming that Uranus was disturbed by some unknown body, to determine the position of the latter, its mass, and the elements of its orbit, by means of the irregularities it produces in the motion of the disturbed planet. This problem being the inverse case of the ordinary problem of planetary perturbation, is manifestly of a very intricate character, and demands the resources of a very refined analysis for its successful treatment. Nay, it is even more complicated than at first it appears to be, for the elements of Uranus, which form the basis of the geometer's researches, are vitiated by the action of the unknown planet, and their rectification, however desirable, can only be effected by the actual solution of the problem. In order to acquire clear ideas upon this point, the reader must bear in mind that the fundamental or mean orbit of Uranus is an ellipse, in which it would constantly revolve if it was exposed solely to the attractive influence of the sun, and that the action of the other bodies of the system tends merely to occasion minute excursions of the planet now on one side and now on the other side of this orbit. It is manifest, then, that in determining the elliptic elements of a planet's motion by means of its positions as assigned by observation, the effects of perturbation must be subducted from the co-ordinates of each position, in order that the planet may be reduced to the place which it would occupy in the mean elliptic orbit. Now when Delambre and Bouvard were severally engaged in determining the elements of Uranus, they applied the perturbations due to Jupiter and Saturn as corrections to the positions derived from observation; but, as they did not take into account the action of the unknown planet, it is manifest that, if the latter really existed, the final values of the heliocentric co-ordinates, as determined by each of these astronomers, must have failed to represent the elliptic positions of the planet. Hence, the elements deduced from these erroneous data were necessarily inaccurate, and it is manifest that this circumstance alone would have the effect of distorting the orbit of the planet, and thereby occasioning discordances between the observed and computed values of the longitude, latitude, and radius vector. The errors of the tables of Uranus were, therefore, attributable to two distinct causes, being due partly to the errors in the elements of the planet's own orbit, and partly to the disturbing action of the unknown planet. These two causes of error, although totally opposite in their natures, had both a common origin, and their effects on this account were so thoroughly entangled in each other, that it was impossible to investigate either independently. This circumstance, as may well be supposed, vastly increased the difficulty of the problem. If it had been possible to ascertain what portion of the irregularities of the planet was in each case due to the errors of the elements, the values of these errors might have been determined by the ordinary methods of astronomy; and when once the elements were thus corrected, it would have been undoubtedly a less arduous task to ascend from them to the elements of the unknown planet. But it clearly follows, from the remarks we have already made, that the elements of Uranus cannot be determined without taking into account the whole effects of planetary perturbation, and this again implies a knowledge of what is the very object of inquiry, namely, the elements and mass of the unknown planet. The problem, then, admits of being completely treated only by a method which shall embrace in one simultaneous investigation the corrections to the elements of Uranus and the elements and mass of the unknown planet. Now, the elements of a planet are six in number. The problem, when considered in all its generality, will therefore involve thirteen unknown quantities. As, however, it appeared from observation that the perturbations of Uranus in latitude were very small, the two planets might be supposed, for all the purposes of a first approximation, to revolve in the same plane, and by this simplification the number of unknown quantities was reduced to nine. These were:—the corrections to the mean distance, the eccentricity, the longitude of the perihelion, and the epoch or mean longitude of Uranus; and the absolute values of the same elements relative to the unknown planet together with its mass. Now, in the case of one planet acting upon another and disturbing its motion, the theory of perturbation enables the geometer to express the corrections to the co-ordinates of the disturbed planet in terms of these nine unknown quantities; and, if the hypothesis of an unknown planet was true in the present case, these corrections ought to account for the differences between the observed and computed positions of the planet Uranus. Hence, by comparing the analytical formulæ for the corrections to the co-ordinates of Uranus with the numerical errors of the tables, a number of equations of condition are formed, and the problem is finally reduced to the elimination of the unknown quantities from these equations. It may readily be imagined that this operation is attended with difficulties of no ordinary kind. In fact, only a thorough acquaintance with the nature of planetary perturbation, and a complete command of analysis, combined with consummate skill in its application, will suggest to the mathematician such artifices as may lead to the disengagement of the unknown quantities from the complicated expressions in which they are involved.

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* Mem. Ast. Soc., vol. xvi.; see also the Nautical Almanac for 1851 (*Appendix*).

Nor does the mere elimination of the unknown quantities constitute the only difficulty attending the solution of the problem. Unless the equations of condition be skilfully combined together, it may happen that the values of the unknown quantities, although deduced by a strictly legitimate process of reasoning, will prove totally erroneous when considered as physical results. This will be readily understood by the reader when he takes into account the mode in which the equations of condition are formed. In each case the error of the planet's place is expressed algebraically in terms of the nine unknown quantities of the problem, and the equation of condition is formed by putting this expression equal to the actual error as determined by a comparison of the observed and computed places of the planet. Every tabular error furnishes a corresponding equation, and thus the number of equations of condition is limited only by the number of independent observations. Now, if the latter were mathematically accurate, the equations of condition would all be consistent with each other, so that any nine of them would suffice for the determination of the nine unknown quantities of the problem, and the values hence deduced would rigorously satisfy all the other equations. But, as all observations are necessarily more or less erroneous, the errors resulting from a comparison of the observed and calculated places of the planet will not be *wholly* dependent on the unknown quantities of the problem; and the equations of condition being vitiated in consequence, will prove to a certain extent incompatible with each other, so that every nine equations selected for the determination of the unknown quantities will conduct to different results. With a view to elude this source of error, the number of equations of condition is generally made to exceed in a great degree the number of unknown quantities, and the grand object, then, is to combine them together, so that the errors of observation may destroy each other, and a system of equations may finally be arrived at which will assign the true values of the unknown quantities.

Mr. Adams first proceeded to examine the perturbations produced in the motion of Uranus by the other planets, in order to assure himself beyond doubt that the errors of Bouvard's tables did not proceed from an erroneous application of the existing theory*. For this purpose he recomputed the principal perturbations due to Jupiter and Saturn, and introduced some new inequalities which had been first pointed out by Hansen†. He also took into account the correction to Jupiter's mass, to which recent researches had conducted Astronomers. Notwithstanding these improvements, the theory still failed to represent the motion of the planet. Two important advantages were, however, gained by these preliminary labours. In the first place, it was clearly established that the cause of the irregularities must be sought elsewhere than in the development of the actual theory. In the second place, the application of the improvements had the effect of exhibiting the errors of the tables as residual facts wholly dependent on some extraneous influence, and consequently they now assumed a more precise and definite character than they had previously done. The first point to be decided in this inquiry was, the most suitable mode of exhibiting the irregularities in the motion of the planet. We have mentioned already, that both the heliocentric longitude and radius vector had been discovered to be sensibly in error. Now it may be remarked, that the problem admits of solution by employing as the basis of investigation an adequate number of the errors of either of these co-ordinates, independently of those of the other. In point of fact, however, the errors of the radius vector were too inconsiderable to be employed with safety in such an inquiry, although they might be subsequently used with advantage in verifying results otherwise derived. Mr. Adams, therefore, had recourse to the errors of heliocentric longitude, which in some instances amounted to 2′ or 3′, and extended over a period embracing more than a revolution and a half of to its discovery in 1781, and on the anterior observations of Bradley, Lemonnier, and Mayer. The actual motion of the planet was represented better by them than by the tables constructed in 1821, but the residual errors were still, in many instances, too great to be accounted for by any probable errors of observation. This was candidly admitted by the author of the tables, who intimated also his belief that the errors were of such a nature as to give countenance to the opinion entertained by his uncle that they were due to the perturbations produced by another planet*.

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* Mem. Ast. Soc., vol. xvi.; Naut. Alm., 1851.

† These inequalities are of the order of the squares of the disturbing forces, and are by far the most considerable of the class to which they belong.

The failure of this laborious attempt to correct the tables of Uranus without the introduction of any new hypothesis, beyond doubt produced a strong impression on the minds of the French astronomers; and it was probably this circumstance which induced M. Arago to propose to his friend, M. Le Verrier, the theory of Uranus as a subject eminently worthy of attention. We have already had frequent occasions to allude to the labours of this talented mathematician. He had recently been occupied with the theory of comets; but, at the suggestion of the illustrious philosopher just mentioned, he temporarily laid aside his researches on that subject, and resolved to institute a searching scrutiny into all the circumstances affecting the motion of Uranus. An account of the first part of his researches appeared in the *Comptes Rendus* of the Academy of Sciences, for the 10th of November, 1845. It contained the result of a rigorous examination of the motion of the planet according to the principles of the actual theory. The only interior planets which produce sensible perturbations in the motion of Uranus are Jupiter and Saturn, and of these the latter exercises the more powerful influence, on account of his comparative proximity to the disturbed planet. Le Verrier determined the value of every inequality depending on the action of Saturn, that exceeded the twentieth part of a second, employing two distinct methods of calculation, in order to assure greater accuracy to his results†. He also computed all the perturbations occasioned by the action of Jupiter. Nor did he confine his researches to the inequalities of the first order of magnitude. All the principal inequalities depending on the squares of the disturbing forces were also taken into account by him‡. A great number of small terms were thus introduced, for the first time, into the heliocentric co-ordinates of the planet, the previous omission of which, he shewed, was alone sufficient to occasion sensible errors in the tables.

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‡ When the mutual action of two planets is considered, their perturbations are first computed by supposing them to move in ellipses, and the resulting inequalities are of the first order with respect to the disturbing forces. In a second approximation the perturbations consequent upon the derangements of the motions of the two planets, as assigned by the first approximation, are taken into account, and the inequalities hence derived are of the second order with respect to the disturbing forces. It is important to remark that the inequalities of the second order may be materially modified by the derangements of both planets depending upon the action of a third planet. This happens when the theory of Uranus and Saturn is considered, the action of Jupiter exercising a very sensible influence on the mutual perturbations of those bodies. On account of his great mass and his comparative proximity to Saturn, Jupiter produces very extensive perturbations in the motion of that planet. Le Verrier, who resolved to verify everything by actual calculation, was induced by this circumstance to investigate a great part of the theory of Saturn, even although, in the present case, it was only with the ulterior view of computing the action of that planet upon Uranus.

An account of the second part of Le Verrier's researches appeared in the *Comptes Rendus*, for the 1st of June, 1846. After having corrected the theory of the planet, he next proceeded to compare it with the observations, in order to ascertain whether the latter might be satisfied by applying suitable corrections to the elements, or whether it was impossible to account for them by any ellipse whatever and the perturbations of the known planets. He selected, for this purpose, nineteen ancient observations of the planet, and 260 observations made at the Observatories of Greenwich and Paris, during the period comprised between the discovery of the planet in 1781 and the month of September, 1845. In order effectually to guard against every contingency of error, he undertook the laborious task of reducing anew all these observations. Having employed the results in computing the geocentric co-ordinates of the planet for each observation, he then proceeded to deduce from theory the corresponding values of the co-ordinates. It was necessary for this purpose to determine, in the first instance, the heliocentric co-ordinates of the planet. When these are known, and also the place of the earth in her orbit, a simple calculation, the converse, in fact, of that to which we have already alluded *, will suffice for passing from the heliocentric to the geocentric co-ordinates. Le Verrier therefore calculated the heliocentric longitude, latitude, and radius vector of the planet; and then, by means of these data and the places of the earth in her orbit, derived from the solar tables, he determined the values of the geocentric co-ordinates. Finally, subtracting the value of each co-ordinate from the corresponding value assigned by observation, he obtained the errors of his theory. The question which now offered itself was this: Can these errors be destroyed by applying suitable corrections to the elliptic elements of the planet? Now, it is easy to obtain the analytical expression for the error in geocentric longitude, in terms of the errors of the elements of the planet, these errors being exhibited in indeterminate forms. In the present case, the only elements which it is necessary to take into account are the mean distance, the epoch, the eccentricity, and the longitude of the perihelion; for the effects due to errors in the other two elements are of an inferior order of magnitude, and, in consequence, may be neglected. By putting the algebraic expression for the error of the theory equal to each of the numerical values which he had previously obtained by a comparison of the observed and calculated geocentric longitudes, Le Verrier formed a series of equations of condition, which ought to be satisfied if the discordances between theory and observation were really due to errors in the elements. Every observation gave him a corresponding equation of condition; so that the total number of equations thus formed by him amounted to two hundred and seventy-nine. Setting aside the equations of condition depending on the ancient observations, he formed four final equations, by a suitable combination of all the others, and from them deduced the corrections to the four elements. The motion of the planet, when calculated for a great number of places by means of the new ellipse, resulting from the application of these corrections to the tabular elements, still exhibited discordances with the actual motion, which were generally by far too great to be accounted for by any probable errors of observation.

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* See page 171.

It appeared, therefore, impossible to avoid the conclusion that some extraneous cause must be constantly acting upon the planet, the influence of which had not hitherto been taken into account in calculating its motion. In order, however, to establish, beyond all doubt, that the discordances were not due to the particular values of the elements employed by him, he had recourse to a general method by which he formally demonstrated the incompatibility of the existing theory of the planet with the results of observation. Pursuing a process which it would be out of place to explain here, he obtained a final equation, one part of which was a numerical quantity amounting to 356″, while the other part was an analytical expression involving eleven mean errors of observation in indeterminate forms, these mean errors being in each case generally the resultant of a great number of individual errors of observation. Now, if the discordances between the computed and the actual motion of the planet were really due to errors of observation, this equation ought to be rigorously true. The question, therefore, which now presented itself to M. Le Verrier was this: is it possible, by ascribing to the errors of observation any admissible values, to render the indeterminate part of the equation equal to the numerical part? With the view of arriving at a definite solution of this question, he gave to each error a value considerably greater than was probably due to it; he further supposed that the errors all took place in the same direction, and also that they had in all cases attained their maximum values. These assumptions, although very improbable, were all favourable towards rendering the equation identically true; still, notwithstanding this circumstance, the resulting value of the indeterminate part of the equation amounted only to 92″, and consequently of the 356″, which formed the constant part, there still remained 264″ unaccounted for. Le Verrier, therefore, finally concluded that the theory of Uranus in its existing state was absolutely incapable of representing the actual motion of the planet; and that the 264″ which formed the residual part of the equation must be attributed to some extraneous cause acting upon the planet, and hitherto unknown to astronomers.

Le Verrier next examined the various hypotheses which had been advanced with the view of accounting for the irregularities in the motion of Uranus. Some persons imagined that the solar force did not diminish in intensity according to the exact ratio of the inverse square of the distance; and they suspected that, for a body so remote as Uranus, the deviation from the Newtonian law might become sensible. Le Verrier justly considered all such hypotheses to be inadmissible, until it was found that the anomalies of the planet could not be accounted for in accordance with the recognised principles of the theory of gravitation. Again, others were disposed to ascribe the irregularities to the influence of a resisting medium pervading the regions of space. This explanation he considered to be equally unworthy of credit; since the effects of a resisting medium had not been found to be sensible, even in cases much more favourable for the manifestation of its presence.

Were the errors, then, due to a large satellite which accompanied Uranus in its orbit, and constantly disturbed its motion? This explanation was likewise untenable; for the perturbations produced by a satellite would pass through their values in short periods, whereas the actual irregularities of the planet appeared to develope themselves with extreme slowness. Besides, the satellite would require to be very large, and in

In the next place, were the irregularities due to a comet which came into collision with the planet, and suddenly changed the direction and quantity of its motion? The effect of such a collision would be to throw the planet out of the ellipse in which it was moving, into another ellipse having different elements. Its motion, therefore, previous to the epoch of collision, and during an indefinite lapse of time afterwards, would be satisfied by two distinct systems of elliptic elements, and consequently in neither case ought there to appear any outstanding irregularities, when once the elements were duly corrected by means of observations relating exclusively to the ellipse under consideration. This hypothesis acquired some degree of plausibility from the fact, that Bouvard succeeded in representing with tolerable accuracy, by means of an elliptic orbit, all the observations of the planet comprised between the years 1781 and 1820. In order, however, to justify its adoption, it would be necessary that this ellipse should tally either with the observations anterior to 1781, or with those subsequent to 1820, conditions both of which had been found by astronomers to be incompatible with the actual motion of the planet.

It was manifest, then, that the irregularities must be occasioned by some cause acting continuously upon the planet, and varying slowly in the intensity of its influence. This can be no other than a planet, and the question then arises: is it possible to assign the place in the heavens which it ought to occupy? In the first place, it could not be situated within Saturn, for, if so, it would disturb that planet to a greater extent than it disturbs Uranus; but this was contrary to fact, the perturbations which it produces in the motion of Saturn being insensible. Secondly, was it situated between Saturn and Uranus? In that case, it would require to be nearer Uranus than Saturn, and as its mass, in consequence, must necessarily be very inconsiderable, the perturbations produced by it would be sensible only in conjunction. But the periodic times of the two planets would be very nearly equal. It followed, therefore, that the perturbations would be sensible only once during the period including all the existing observations of Uranus. This conclusion, however, was at variance with the actual character of the irregularities of the planet. The disturbing body must, therefore, be situated beyond Uranus. It must not be very near that planet, for its mass would then be inconsiderable, and its perturbations, as in the preceding case, would exhibit a character inconsistent with observation. Nor must it be situated very far beyond Uranus; for, if it were so, its respective distances from Saturn and Uranus would be comparable with each other, and it would be impossible to account for the irregularities of Uranus without developing similar irregularities of the same order of magnitude in Saturn; but of these there did not exist any sensible traces. It manifestly followed, therefore, that the planet must be situated at some moderate distance beyond Uranus. Now, according to Bode's law, the successive distances of the more remote planets are nearly double each other. Le Verrier, therefore, assumed as the basis of a first approximation, that the disturbing planet was twice as distant from the sun as Uranus, and, arguing similarly from analogy, he concluded that it revolved very nearly in the plane of the ecliptic. He was thus led to propound the subject of his researches in the following form:—"*Is it possible that the irregularities of Uranus may arise from the action of a planet situated in the ecliptic at a mean distance double that of Uranus? If such be the case, what is the actual position of this planet? What is its mass? and what are the ele-*

ments of the orbit which it describes?" The problem being thus enunciated, Le Verrier next proceeded to explain his method of solution. It was impossible to consider the disturbing planet without also taking into account the errors in the elements of Uranus; for the irregularities of the latter planet, which formed the data of the problem, depended on both these causes; nor was it possible to eliminate the effects due to either cause without a knowledge of those due to the other. The problem, therefore, admitted of solution only by a method which would embrace in one simultaneous investigation the elements of the disturbing planet, and the corrections due to the elements of Uranus. It was necessary for this purpose to compute the heliocentric co-ordinates of the latter planet by means of the actual ellipse, and the perturbations of the known planets, and then to apply to the results 1°, the expression for the perturbations of the unknown planet, in terms of its mass and the elements of its orbit, 2°, the expression for the correction due to the false ellipse of Uranus, in terms of the errors of the elements. Each of the two last-mentioned expressions contained four unknown quantities, and therefore the complete value of any of the heliocentric co-ordinates would contain eight unknown quantities. Equating this result to the corresponding value of the co-ordinate derived from observation, an equation of condition was formed between the eight unknown quantities, and the solution of the problem was reduced to the elimination of these quantities from a system of such equations.

Le Verrier employed as the basis of his researches eighteen errors of heliocentric longitude*. Three of these errors depended on observations of the planet made by Flamstead, in 1690, 1712, and 1715. The other fifteen extended over the period embraced between 1747 and 1845, and were separated by equal intervals of seven years. By means of these eighteen errors he formed an equal number of equations of condition. He remarked that the corrections to the elements of Uranus might be readily eliminated from the equations of condition; and hence the resulting equations would contain only four unknown quantities, namely, the mass, the epoch, the eccentricity, and the longitude of the perihelion of the disturbing planet. Now, it was easy to obtain expressions for the mass, the eccentricity, and the longitude of the perihelion, in terms of the epoch. The reader who possesses a knowledge of the elements of mathematics will hence perceive that by means of these expressions an equation might finally be formed involving the epoch alone; and upon its elimination the solution of the problem would manifestly depend. This element, however, presented itself in such a complicated form, that Le Verrier did not consider it advisable to attempt the solution of the problem by means of such an equation. He proposed to attain the same end by assigning to the epoch a succession of numerical values, and, having determined by means of it the corresponding values of the other elements, then ascertaining which system of values agreed best with the observations.

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* He had previously employed these errors in demonstrating the incompatibility of the actual theory of Uranus with the results of observation; but, in order to avoid unnecessary complication, we did not make any allusion to them on that occasion. As the errors were all subject to the condition of being equidistant, it was impossible to determine them generally except by a process of interpolation; and, as the ancient observations were separated from each other by considerable intervals, it is manifest that in some instances the numerical values of the errors could not be relied upon for extreme accuracy. Besides, the probable effect of the error of radius vector was not taken into account. Le Verrier clearly saw that the errors arising from both these causes were too insignificant to deserve notice when the object merely was to obtain an approximate value of the mean longitude of the disturbing planet. In his final solution he completely eluded both sources of

Le Verrier eliminated the corrections to the elements of Uranus from the equations of 1715, 1775, 1810, and 1845; and, setting aside those of 1690 and 1715, he grouped the remaining twelve into three mean equations corresponding to the years 1758, 1793, and 1828. Each of these equations contained the mass, the eccentricity, the longitude of the perihelion, and the epoch of the disturbing planet. Now, by assigning any particular value to the epoch, it was easy to determine the values of the other three unknown quantities. These values might then be employed in computing the place of Uranus corresponding to any given observation; and, by a comparison of the observed and computed places, the error of the theory depending on the assumed value of the epoch might be ascertained. Le Verrier proposed, by this means, to compare his theory with the observations of 1690 and 1747 for a great number of values of the epoch, with the view of discovering whether the errors in any case were so small that it might be fairly presumed they were due to errors of observation. This was the criterion by which he resolved to test the legitimacy of his theory; and its suitableness for this purpose may be readily understood, for it is manifest that any errors committed in the calculation of the elements of the disturbing planet by means of the equations of condition, founded mainly on the modern observations, could not fail to produce sensible effects when the theory was compared with the more remote observations of 1690 and 1747*. In order to confine his labours within as narrow a sphere as possible, he proceeded to inquire what values of the epoch were really admissible, for it was useless to employ any values that did not tally with the conditions of the problem. Now, it was clear that those values of the epoch which assigned negative values to the mass ought to be rejected, for all such values implied that the disturbing planet exerted on Uranus a *pushing* force, and not an attractive force, conformably to the theory of gravitation. Le Verrier found by an elaborate and skilful scrutiny of the form of the algebraic expression for the mass that it had a positive signification for all those values of the epoch comprised between 96° 40′ and 189° 55′, and also for those between 263° 8′ and 358° 41′; and that it was negative for all the remaining values comprised within the circuit of the ecliptic. Again, it was evident that those values of the epoch which made the mass immoderately large were inadmissible; for, in all such cases, the planet would produce sensible perturbations in the motion of Saturn; but this conclusion was at variance with observation. Rejecting all such values, Le Verrier succeeded in bringing still nearer to each other the limits he had previously found. The arc which had 96° 40′ and 189° 55′ for its limits now extended only between 108° and 162°; while the arc which was bounded by 263° 8′ and 358° 41′ was now wholly included between 297° and 333°.

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* Some of our readers may be disposed to conclude, by similar reasoning, that it would be more advantageous to test the theory by the equations of 1712 and 1690 than by those of 1747 and 1690. This is, no doubt, true in an absolute sense; but it must be borne in mind that the equation of 1715 was employed for the purpose of eliminating the corrections to the elements of Uranus, and was on this account invariably equal to zero. It is easy, then, to perceive, when we take into account the close proximity of the observations of 1712 and 1715, that the equation of 1712 would, in all cases, be exceedingly small, and consequently it could be of little value in testing the accuracy of the

It only now remained for him to compute the values of the mass, the eccentricity, and the longitude of the perihelion of the disturbing planet, for a number of particular values of the epoch contained between those limits which he had found to include all the admissible values, and then to determine the corresponding errors of the theory in 1690 and 1747. This calculation he executed for a great number of equidistant values of the epoch, but he was mortified to find that the errors were in all cases so considerable that they could not be accounted for by any probable errors of observation, and the conclusion seemed to be inevitable, that it was absolutely impossible to represent the irregularities of Uranus by the hypothesis of a disturbing planet. Thus it appeared to him that he was all the while engaged in pursuing a phantom, or, to use the words of the illustrious Kepler on a similar occasion, "all his labours vanished in smoke."*

Notwithstanding this unexpected conclusion, Le Verrier was still reluctant to abandon the hypothesis of an exterior planet; for, in such abstruse and complicated inquiries, there may exist just reasons for supposing that the final results have been influenced in an inordinate degree by some hidden cause, depending either on the method of solution or on the nature of the problem itself. He did not therefore despair of rendering his theory consistent with observation by the detection of some peculiarity of this kind; nor did his genius fail to come eventually to his aid in this perplexing emergency. He discovered, in fact, that the form of the analytical expression for the mass was such, that a very small error committed in the observations of Uranus would affect, to an enormous extent, its numerical value corresponding to any given value of the epoch; whence it followed that the errors of the theory in 1690 and 1747, which had been computed by means of the values of the mass, and the other elements of the disturbing planet obtained by supposing the observations to be absolutely correct, had not been fairly represented by the results relative to those quantities at which he had finally arrived. It appeared to him, therefore, that no legitimate conclusion could be deduced from the equations of condition until the supposition of possible errors in the observations was introduced into them. He now resumed the consideration of the three mean equations of 1758, 1793, and 1828, assuming that all the observations which entered into their composition were affected with indeterminate errors. By a simple inspection of their forms he discovered that these equations could only be affected to a sensible extent by the errors of the observations of 1715 and 1775†. In addition, therefore, to the four unknown quantities, relative to the disturbing planet, each equation contained these two errors represented by appropriate symbols. He now resolved to assign a succession of values to the epoch in the two equations of

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* Itaque causæ physicæ, cap. xlv. in fumos abeunt, *De Motibus Stellæ Martis*, cap. lv. Such are the terms in which Kepler announces the failure of his oval theory to account for the motions of Mars, a theory to which he long continued to cling with the most absolute conviction of its truth, and upon which he expended an almost incredible amount of ingenious reasoning and toilsome calculation. While pondering in great perplexity on the cause of the failure, a happy inspiration of his genius revealed to him the grand truth that the orbit of the planet is an ellipse.

† The errors of observation for 1810 and 1845 were also similarly calculated to affect the equations; but, as they were in all probability very small, Le Verrier considered

1793 and 1828, and then to eliminate from them the eccentricity and the longitude of the perihelion of the disturbing planet. From what we have already said, relative to the composition of these equations, it is manifest that each of the eliminated elements would be expressed in terms of three unknown quantities, namely, the mass, and the two indeterminate errors of observation. He now proposed, by means of them, to determine the errors of the theory in 1690, 1747, and 1758, in order to ascertain whether they might be made sufficiently small for any value of the epoch. These errors no longer appeared in the form of simple numerical results, as they did by his first method. They now contained the mass, and the two errors of observation in indeterminate forms; and it was only by a discussion founded on the limits, within which each of these three quantities might possibly vary, that the numerical value of the error of the theory could be in any case arrived at.

Le Verrier proceeded to apply this method of investigation, purposing to extend it to the whole of the ecliptic. For this purpose he made the epoch of the disturbing planet vary for every 9° from zero to 360°, and computed the corresponding errors of the theory in 1690, 1747, and 1758. Examining the expressions for these errors, he found that, for all values of the epoch from zero to 189°, no admissible values of the mass and the other two indeterminate quantities could render the results in all the three cases so small that they might reasonably be attributed to errors of observation. As the epoch increased from 189°, the errors in each of the three cases began to diminish, and they all became very small, when it attained a value equal to 243° or 252°, or any of the intermediate values. From 252° the errors began to increase and soon became inadmissible; nor did they present any diminution throughout the remaining part of the circumference of the ecliptic. Le Verrier therefore came to the conclusion that *there is only one region of the ecliptic in which it is possible to place the disturbing planet so as to account for the movements of Uranus, and that the mean longitude of this planet, on the 1st of January,* 1800, *must be included between* 243° *and* 252°.

The next point to ascertain was, whether a planet actually situated in the region indicated by his researches would account generally for the irregularities of Uranus; for, although he had found that a planet whose mean longitude was somewhere about 252°, and whose eccentricity and longitude of perihelion were determined by the equations of 1793 and 1828, would also satisfy the equations of 1690, 1747, and 1758, it did not follow as a necessary consequence that all the equations of condition depending severally upon the individual observations of the planet would also be satisfied by the same supposition. In order to establish this point, he resolved to compute the errors of the theory corresponding to the eighteen individual observations which formed the basis of his researches. There was this advantage, however, gained by his previous labours, that it was no longer necessary to extend his investigation to the whole circuit of the ecliptic, it being merely sufficient to examine the region in which he had already found that the planet, if really existing, must be situated, at the commencement of the year 1800. With this view he formed eighteen expressions for the errors of the theory for five equidistant values of the epoch comprised between 234° and 270°, the corresponding values of the eccentricity and the longitude of the perihelion being derived, as before, from the two mean equations of 1793 and 1828. The result of this investigation afforded a complete confirmation of his previous researches. He now found that, when the epoch was made equal to 252°, the eighteen errors of the theory could be so diminished by assigning to the mass, and the two indeterminate errors of observation, values within their prescribed limits, that the residual quantities might reasonably be supposed to arise from errors of observation. As the epoch diverged on each side of 252°, the errors continued small for the first 9°; but afterwards they began to increase, and became inadmissible before it reached 234° or 270°. He therefore concluded that the observations of Uranus might generally be satisfied by the hypothesis of a disturbing planet, whose mean longitude at the commencement of the year 1800 was nearly equal to 252°.

It still remained for him to determine the approximate value of the true longitude, a knowledge of which was indispensable to the discovery of the planet. For this purpose, it was necessary for him to investigate the values of the eccentricity and the perihelion corresponding to the value of the epoch adopted by him as the most probable approximation to the truth. Now the expressions for these elements were obtained by eliminating them from the mean equations of 1793 and 1828, after substituting for the epoch the numerical value 252°, and consequently each of them contained three unknown quantities; namely, the mass and the two indeterminate errors of observation. By a skilful discussion of the errors of the theory corresponding to the eighteen fundamental equations of conditions, Le Verrier had already arrived at approximate values of these three quantities*. He therefore readily determined the numerical values of the eccentricity and the longitude of the perihelion†; by means of which, and the other two elements already known, he computed the approximate value of the true longitude. He finally concluded that the true longitude of the disturbing planet for the 1st of January, 1847, was 325°, and that the probable error did not exceed 10°.

Le Verrier's paper contained the earliest account of researches respecting the hypothetical planet that was given to the world, and, as may well be conceived, it was read with delight by all the astronomers of Europe. Mr. Airy received the *Comptes Rendus* for the 1st June, on the 23rd of the same month. Finding that the place assigned by Le Verrier to the disturbing planet did not differ more than a degree from that which Mr. Adams had assigned to it eight months previously, he felt a strong persuasion of the accuracy of the labours of both mathematicians. He was still, however, anxious to ascertain whether the action of the hypothetic planet would account for the errors in the radius vector of Uranus as satisfactorily as it accounted for the errors in longitude, and he now applied to M. Le Verrier for information on this point, as on a similar occasion he had applied to Mr. Adams. In his letter to the French mathematician, he remarks, that the tabular radii vectores of Uranus were too small in recent years; and he asks whether this would result from the disturbance of an exterior planet occupying the position indicated by the theory. "I imagine," says Mr. Airy, "that it would not be so, because the principal term of the inequality would probably be analogous to the moon's variation, or would depend on sine $2(v-v')$; and in that case the perturbation in radius vector would have the sign — for the present relative position of the planet and Uranus. But this analogy is worth little until it is supported by proper symbolical computations."

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* Le Verrier, indeed, does not give any account of the method by which he assigned to the errors of observation their most probable values; but it is probable that he effected this object by a careful comparison of the residual errors of the theory, which in all cases were expressed in terms of the mass, and the two indeterminate quantities denoting the errors of observation. It is not impossible also, that, in order to assure greater accuracy to his results in the present instance, he took into account the effect of the errors of observation for 1810 and 1845; for, although the presumed insignificance of these errors allowed their rejection when the object was merely to ascertain the region of the ecliptic in which the planet was to be found, still, when the question referred to the determination of its actual position, it is not difficult to perceive the desirableness of taking them into account. In fact, Le Verrier shews, at page 202 of his Memoir, that they exercise [illegible] very sensible influence on the ultimate values of the errors of the theory.

† The paper in the

The reply of Le Verrier to the Astronomer Royal was at once prompt and satisfactory. He stated that his theory accounted for the errors of radius vector as well as those of longitude; and he explained the circumstances which caused them to disappear. We have already mentioned that the irregularities of Uranus, if due to a disturbing planet, must have proceeded partly from the errors in the elements of the planet's own orbit, and partly from the perturbations occasioned by the undiscovered planet. Now, Le Verrier found that the errors of radius vector almost wholly vanished when he applied to the elements the corrections derived from the errors of longitude. It was clear, then, that the errors directly dependent on perturbation were quite insignificant, compared with those arising from the distortion of the orbit, and this circumstance afforded a satisfactory explanation of the apparent inconsistency between theory and observation, alluded to by the Astronomer Royal in his letter to Le Verrier. The latter, in fact, found that both the eccentricity and the longitude of the perihelion ought to be increased; and it happened, in consequence of the actual position of the planet, that the application of these corrections had the effect of increasing the tabular value of the radius vector almost to the whole extent required by the observations[*].

The results of Mr. Adams, which received so remarkable a confirmation from those of M. Le Verrier, appeared now to the Astronomer Royal to be established beyond all doubt by the satisfactory communication he received from the latter of these mathematicians. At a meeting of the Board of Visitors of the Observatory of Greenwich, held on the 29th June, 1846, Mr. Airy mentioned the near agreement of the results obtained by Le Verrier and Adams relative to the supposed existence of a planet exterior to Uranus; and he suggested in strong terms the desirableness of some Observatory devoting its resources to a systematic search for the undiscovered body. In pursuance of this view he wrote a letter to Mr. Challis, dated the 9th July, recommending an examination of the heavens with the Northumberland refractor, and offering to supply him with an assistant if he should not have leisure to superintend the operation personally[†]. On the 13th of the same month he wrote a second letter to Mr. Challis on the same subject, inclosing in it a paper headed, "Suggestions for the Examination of a portion of the Heavens in search of the New Planet." He proposed taking a sweep of the heavens in the direction of the ecliptic 30° long, and 10° broad, the centre being the place indicated by the theory as the locus of the planet. The paper contained the details of a plan for conducting the examination. In his letter he says, "I only add at present that in my opinion the importance of this inquiry exceeds that of any current work which is of such a nature as not to be totally lost by delay."* Professor Challis declined Mr. Airy's offer of an assistant, having himself formed the resolution (as he had previously intimated to Mr. Adams †) of searching for the planet on the occasion of the approaching opposition. Having received a paper from Mr. Adams, containing instructions relative to the theoretical place of the planet, he commenced his observations for this purpose on the 29th July ‡. The plan contemplated was to sweep over the region of the zodiac selected for examination at least three times, completing each sweep before the commencement of the following one. It was concluded that to accomplish this object three hundred hours of observing would be required. The discovery of the planet would be effected by finding that one of the stars in the examined region had not the same position in each sweep. Mr. Challis continued to prosecute his search till the end of September. We shall find that his observations contained more than one place of the planet, and that their subsequent comparison, *which was included in the plan of operations*, would have infallibly led to its discovery.

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[*] Airy's Hist. Statement, No. 14.

[†] Airy's Hist. Statement, No. 15. It is perhaps right to mention, for the use of some of our readers, that at Greenwich the object of the observations is to determine with the utmost possible precision the apparent positions of the celestial bodies, rather than to acquire a knowledge of the physical structure of the heavens. Telescopes of great power are not, therefore, required at the Royal Observatory. In the present instance it was necessary to observe all the stars, in the part of the heavens appointed for examination, down to the tenth magnitude, but at Greenwich there were no telescopes sufficiently powerful for this purpose. The instrument alluded to by Mr. Airy is a magnificent refractor pre-

We now return to Mr. Adams. On the 2nd September, 1846, he transmitted a second paper to the Astronomer Royal, containing an account of his further researches on the Trans-Uranian planet. A comparison of his original results with the observations of Uranus had induced him to suspect that the mean distance was somewhat too great. He therefore diminished it to the extent of $\frac{1}{30}$th, assuming it to be equal to 37.5, and then repeated his previous solution. He hoped that by this means the theory would be brought to agree better with the observations of recent years, and that a smaller value would be obtained for the eccentricity, which appeared to him too large, as it resulted from his first hypothesis. In his communication to the Astronomer Royal, he gave the elements of the planet by the two hypotheses of the mean distance, and appended a list of residual errors of longitude formed by a comparison of his theory with a great number of observations of the planet included between the years 1712 and 1840. These errors were smaller by the second hypothesis than by the first, and the eccentricity was also considerably diminished. Mr. Adams was of opinion that by continuing to diminish the mean distance the theory might be rendered still more accordant with observation, and he was induced to conclude, from an examination of the residual errors of recent years, that, by assigning to the mean distance a value equal to 33.6, a very near approximation to the truth would be obtained. This surmise has been fully borne out by the results derived from actual observations of the planet; and, while it is highly creditable to Mr. Adams' sagacity, it also shews the thorough insight he had obtained into the mutual bearings of the various parts of the intricate problem with which he was engaged. The following are the elements he deduced from his second hypothesis of the mean distance:—

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* Airy's Hist. Statement, No. 16.

† Challis' Report to the Syndicate of the University of Cambridge.

‡ Ibid. It must be borne in mind that at this time Le Verrier had obtained an approximate value of the position of the planet, but had not assigned determinate values to the mass or the elements of the orbit. A knowledge of the mass was necessary for the purpose of ascertaining the class of stars among which it might be expected that the planet would be found. Mr. Adams, guided by his theoretical results, had mentioned to Professor Challis that the planet would be equal to a star of the ninth magnitude. This circumstance induced Mr. Challis to note the positions of all stars down to the tenth magnitude. The actual discovery of the planet has shewn that the range of search adopted was at once necessary and sufficient.

Mean longitude, October 1, 1846	323° 2′
Longitude of the perihelion	299° 11′
Eccentricity	.12062
Mass	.00015003

Mr. Adams gave examples of the correction to the tabular value of the radius vector. The correction for 1834 almost coincided with that which Mr. Airy had deduced from observation. The corrections for later years were not quite so satisfactory; but in this respect also the second hypothesis presented a better agreement with observation than the first did. Mr. Adams mentioned that he was then engaged in determining the inclination and place of the node, and that he hoped to complete his investigation in a few days.

The corrections to the tabular radius vector transmitted, on this occasion by Mr. Adams to the Astronomer Royal, naturally suggest a few remarks. We have already mentioned that the latter attached much value to this part of the theory, as affording a criterion for testing the accuracy of the results derived from the researches on the motion in longitude. We can easily imagine that some of our readers will be slow to concur with the views of the Astronomer Royal on this point. If indeed the cause of the irregularities of Uranus was doubtful, it is not difficult to perceive that the explanation of the errors of radius vector by the perturbations of an assumed planet would possess much weight in establishing the legitimacy of such an hypothesis; for, granting that the irregularities were due to some other cause, although it is conceivable that the theory of gravitation might be made to represent the errors in longitude by a suitable evaluation of the constants of the problem, it is utterly improbable that the same constants and the same values of them would also account for the errors of radius vector, with which they had no physical connexion. But the necessity of computing the errors of radius vector in addition to those of longitude does not appear so obvious, if it be admitted that the irregularities of Uranus are really due to a disturbing planet, and that the sole point to be decided was the accuracy of the results. In this case it was known *à priori* that the constants which entered into the expressions of the errors, both of longitude and radius vector, were the *bonâ fide* representatives of the physical elements of the problem. It was also known that these expressions were legitimately deduced from established principles, and consequently were not mere empyrical formulæ. Hence it might be presumed that results which accounted so satisfactorily for the errors in longitude throughout a period embracing more than a revolution and a half of the planet could hardly fail to account with equal fidelity for the errors of radius vector. This reasoning involves the tacit assumption that, if certain values of the unknown quantities of the problem satisfy the errors of one of the co-ordinates of the planet, they must either be absolutely correct, or must constitute very near approximations to the truth. This principle, however, is at variance with the result of researches in physical astronomy, for it has been found in the case which we are actually considering, that the equations of condition involving the errors of longitude may assign to the elements of the disturbing planet, and to the corrections of the elements of Uranus, values which shall account for these errors with sufficient accuracy, but still may differ widely from the true values. Now, if any such system of erroneous values was employed in computing the errors of radius vector, it manifestly does not follow as a necessary consequence that, because the motion in longitude had been satisfied by a mutual compensation of errors, the results in this case also would accord equally well with observation. We are therefore led to conclude that, even if the existence of an exterior planet had been already placed beyond all doubt, the explanation of the errors of radius vector would prove extremely valuable in testing the accuracy of results derived from the errors of longitude. It may be remarked, however, that the Astronomer Royal, in his letter to M. Le Verrier, expressed in very explicit terms his doubts respecting the accuracy of the final results of that geometer, without indeed going so far as to reject *in toto* the hypothesis of a disturbing planet. In his letter to Mr. Adams, on the contrary, he does not suggest any such doubts, but simply inquires whether the errors of radius vector were accounted for by his theory as faithfully as those of longitude. We may suppose, then, that Mr. Adams, who had for many years been strongly impressed with the existence of a Trans-Uranian planet, and who had already been conducted to such satisfactory results by his researches on the motion in longitude, may not have duly appreciated the importance attached by the Astronomer Royal to the explanation of the errors of radius vector. It is to be regretted, however, that he was so tardy in replying to Mr. Airy, especially as he could not have experienced any analytical difficulty in complying at once with his request *.

An account of the third part of Le Verrier's labours on the theory of Uranus appeared in the *Comptes Rendus* for the 31st August, 1846. The main object of the second part of his researches, as announced in the *Comptes Rendus* for the 1st June, was to obtain an approximate value of the epoch or mean longitude of the hypothetic planet at a given instant. When this was once accomplished, the true value might be investigated by applying to the approximate value an indeterminate correction, and then deducing it from the conditions of the problem simultaneously with the other unknown quantities. There was this advantage gained by a first approximation to the epoch, that, as the correction to the true value might be presumed to be small, it was possible so to conduct the investigation that it would not be necessary to take into account any terms beyond those involving the first and second powers of the correction. For a similar reason a more accurate value of the mean distance might be obtained by supposing the approximate mean distance to be affected with an indeterminate error, and then treating the latter as one of the unknown quantities of the problem. There were, therefore, five quantities to be determined relative to the disturbing planet; namely, the corrections to the mean distance and the epoch, and the absolute values of the mass, the eccentricity, and the longitude of the perihelion. These five quantities, together with the corrections to the mean distance, the epoch, the eccentricity, and the longitude of the perihelion of Uranus, formed nine unknown quantities, upon the determination of which the solution of the problem depended.

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* We have maintained that it does not necessarily follow, because the errors of longitude are satisfied, that the errors of radius vector are satisfied also. In the present instance, however, it happens that such is really the case. Mr. Adams, in his Memoir on the Perturbations of Uranus, has given an expression for the correction to the radius vector involving the correction to the mean longitude and its differential, together with the eight unknown quantities of the problem; and he has shown that by far the most considerable part of the expression is due to the term involving the differential of the correction to the mean longitude. Hence it manifestly follows that, if the tabular errors of longitude be satisfied, the errors of radius vector will be satisfied also. This result, however, depends upon the particular values obtained for the unknown quantities, and could not be predicated by any *à priori* reasoning.

In the investigation of an approximate value of the epoch, Le Verrier employed as the basis of his reasoning a select number of errors of heliocentric longitude. These were obtained by a comparison of the theory with observations made when the planet was in opposition. It would have been impossible to deduce accurately the errors of heliocentric longitude from all the observations, because many of the latter were made when the planet was near the quadratures, and in that case the process of passing from the geocentric to the heliocentric longitude could not be rigorously effected without a knowledge of the error of radius vector; but this was altogether uncertain. He now had recourse to the errors in geocentric longitude, which could be readily computed without a knowledge of the errors of either of the heliocentric co-ordinates, and were, therefore, deducible with equal accuracy from all the observations. In the first part of his researches he had carefully computed the errors in geocentric longitude corresponding to two hundred and seventy-nine observations of the planet. Now the analytical expression for these errors contained the nine unknown quantities of the problem. Putting it, therefore, equal to each numerical error in succession, Le Verrier formed two hundred and seventy-nine equations of condition, and by means of these he proposed to obtain the solution of the problem. With this view he grouped them into thirty-three mean equations, twenty-six of which depended on the modern observations of Uranus, and the remaining seven on those made previous to its recognition as a planet in 1781. He eliminated without difficulty six of the unknown quantities in terms of the three others, these last being the mass of the disturbing planet, and the corrections to the epoch and the mean distance. Pursuing a process which it would be out of place to attempt explaining here, he formed three final equations, involving these three quantities, and then determined their values by successive approximation. This object being once accomplished, it was easy for him to obtain the values of the other six quantities which he had first eliminated. The following are the results relative to the disturbing planet at which he finally arrived :—

Semi-axis of the orbit	36.154	
Sidereal revolution	217.387	years.
Eccentricity	0.10761	
Longitude of the perihelion . . .	284° 45′	
Mean longitude, 1st January, 1847 . .	318° 47′	
Mass	$\frac{1}{9300}$	
True heliocentric longitude, 1st Jan., 1847	326° 32′	
Distance from the sun	33.06	

Having determined the precise position of the disturbing planet, by assuming that the observations of Uranus were rigorously accurate, Le Verrier next proceeded to investigate the limits within which it must be included, allowing each of the observations to be affected with an error as great as could possibly be due to it. He found that the mean distance could not be greater than 37.90, nor less than 35.04, and hence he concluded that the limits of the periodic time were 207 years and 233 years. Adopting a given value of the mean distance, and supposing all the other elements to vary, he found that the recorded positions of Uranus might still be represented within the limits assigned by the errors of observation. At length the error in one of the positions having become equal to the greatest possible value assigned to the error of observation, the values of the elements were henceforth restricted by the condition that the error in this position should retain its maximum value, and the locus of the disturbing planet for the 1st January, 1847, corresponding to the different systems of elements derived from this hypothesis, was a continuous curve. By continuing the variation of the elements, the error in another of the positions became as great as possible, and, the values of the elements being henceforth restricted by it, the locus of the planet was transformed into a different curve. Proceeding in this manner, Le Verrier found that the space within which the planet must be included, corresponding to the assumed value of the mean distance, was a curvilinear polygon whose sides were discontinuous, and for different values of the mean distance he obtained different polygons. The amplitude of the polygons diminished as the value of the mean distance approached its extreme limits, and when it actually attained either of them the polygon became a point. This circumstance indicated that there was only one position of the planet that could satisfy the observations. When a sufficient number of such polygons was constructed, they might then be all circumscribed by one continuous curve, and tangents drawn to it would necessarily include the disturbing planet. Le Verrier found that the longitude of the tangent, drawn from the sun to the east side of the bounding curve, was 321°, whence, as he had already obtained 326° 32′ for the most precise value of the planet's longitude, it followed that the space to be explored in this direction extended only to about $5\frac{1}{2}$°. The western limit was much more remote from the precise longitude, but he remarked that its amplitude might be reduced considerably by assigning a probable limit to the eccentricity. Assuming that the value of the eccentricity did not exceed .125, he obtained 335° for the limiting longitude in this direction. He also found that the value of the mass could not be greater than $\frac{1}{4700}$, nor less than $\frac{1}{14500}$.

We have mentioned that Le Verrier assigned 326° 32′ as the true heliocentric longitude of the disturbing planet on the 1st January, 1847. This determination, to use his own words, placed the planet about 5° to the east of the star δ of Capricorn. Only twelve days had elapsed since it was in opposition, and on this account strong hopes were entertained by him that astronomers might succeed in discovering it before it plunged again into the rays of the sun. With the view of directing attention more strongly to this point, he made some interesting remarks relative to the apparent magnitude and visibility of the planet. The apparent magnitude depends on the volume of the body and its distance from the sun. The volume, again, is deducible from the mass and density. Now, he had already found that the mass of the planet was two and a half times greater than that of Uranus. There remained, therefore, only the density to be ascertained. He had no established principles to guide him in coming to course to analogy. Now it appears, from a comparison of the relative densities of the various planets, that, in general, the density is less according as the planet is more distant from the sun. Le Verrier assumed that the density of the new planet was equal to the density of Uranus. This hypothesis was manifestly less favourable to its visibility than if he had assumed, according to the strict suggestions of analogy, that the density was somewhat less than that of Uranus. Knowing the density and the mass, he obtained the volume, and finally, by means of the latter and the distance from the sun, which he had found to be equal to thirty-three times the earth's distance, he determined the apparent magnitude of the planet. In this manner he found, that at the instant of opposition it would subtend an angle of 3″.3; and, considering in connexion with this fact the light which it would be capable of reflecting, he concluded that it would not only be visible in good telescopes, but that it would be distinguishable from the fixed stars by its disc. "This," he remarked, "is a very important point. If the object of discovery is liable to be confounded with the fixed stars, it will be necessary, in order to distinguish it from them, to observe all the small stars situated in the region of the heavens assigned for examination, and to establish in one of them a proper motion. This would be a long and troublesome operation. But, on the other hand, if the planet has a disc of sufficient amplitude to prevent it from being confounded with the stars; if we may substitute, instead of a rigorous determination of all the luminous points, the simple study of their physical appearance, the search will proceed with greater rapidity."*

The elaborate character of Le Verrier's researches, and the confidence with which he predicted the discovery of the planet, was calculated to produce a strong impression on the minds of astronomers. This remark applies more especially to those who were cognizant of the simultaneous researches of Mr. Adams, and who could appreciate the probability in favour of a result in Physical Astronomy that had been deduced from two independent investigations. The existence of a Trans-Uranian planet appeared now to such astronomers to be placed beyond all doubt, and its actual discovery was expected to be not very distant. It was under the impression of the near approach of this great event that Sir John Herschel used the following memorable words in an address to the British Association, at Southampton, on the 10th September, 1846:—"The past year has given to us the new planet Astrea; it has done more, it has given us the probable prospect of another. We see it as Columbus saw America from the shores of Spain. Its movements have been felt trembling along the far-reaching line of our analysis with a certainty hardly inferior to ocular demonstration."†

Before proceeding further with our account, it may not be out of place to consider shortly what were the means which astronomers possessed for the discovery of a body such as theory indicated in the present case to exist. Now there are two peculiarities which generally distinguish planets from the fixed stars, and which serve the purpose of their detection. These are:—1st, their apparent magnitudes, in virtue of which, when observed through telescopes, they exhibit well-defined discs; 2nd, their motion in the zodiac. Le Verrier, by means of reasoning of a very probable character, had come to the conclusion that the planet would have a sensible disc. The apparent diameter, however, which he assigned to it was so extremely small that it was manifest only the most powerful telescopes would suffice to distinguish it from the fictitious discs exhibited by the fixed stars. The probability, therefore, of discovering the planet by its physical appearance was confined to a limited number of European Observatories. With respect to the method of discovering the planet by its proper motion, the use of the telescope in this case is mainly to render the star visible as a luminous point, and, in consequence, it is more generally practicable than the method just referred to. On the other hand, the operation of carrying it into effect is extremely laborious, unless the astronomer already possesses a map of the region of the heavens which he purposes to examine, including all the stars down to the magnitude of the body he is in search of. With such a guide, however, nothing can be more simple than to ascertain whether the region which he is engaged in exploring can possibly afford any indication of the planet. For this purpose the astronomer has only to compare the actual appearance of the heavens on any night with the map. If the stars in both cases correspond, it follows that no change has occurred since the construction of the map, and, as all the objects whose positions were recorded must in consequence have been of a stellar nature, the comparison of the heavens with the map cannot afford any clue to the existence of a planet. If, however, the map contains a star which is not in the heavens, it is clear that the missing star must have been a planet which wandered out of the region under examination during the period that elapsed since the construction of the map, and its discovery may be expected to result from a careful scrutiny of the heavens in the vicinity of the stars designated on the map. On the other hand, if a star appear in the heavens which is not contained in the map, it clearly indicates the entrance of a planet into the designated region subsequently to the construction of the map. In order to conduct a search for the Trans-Uranian planet after this manner, it was necessary to possess a map on which were designated all the stars in the part of the heavens assigned for examination, down to the tenth order of magnitude inclusive. No such map had hitherto been executed for the region comprehending the theoretical locus of the planet, and the only method of search which remained to be adopted was that already in course of being carried into effect by Professor Challis, and which was indeed tantamount to the actual construction of a map.

On the 18th September Le Verrier addressed a letter to the astronomers of the Berlin Observatory, announcing to them the result of his researches, and requesting their co-operation in searching for the planet. By a singular instance of good fortune the Berlin astronomers possessed an advantage in effecting this search which was not yet available to the other astronomers of Europe. For some years past a series of star maps had been in course of publication, under the auspices of the Berlin Academy of Sciences, comprehending different portions of the region of the heavens which extends 15° on each side of the equator, and designating the positions of all stars down to the tenth magnitude*. Just as the accounts respecting Le Verrier's researches reached Berlin, the map of Hora XXI., the part of the heavens containing the theoretical place of the planet—which had been executed with great care by Dr. Bremiker—was engraved and published. The Berlin astronomers received Le Verrier's letter on the 23rd September. On the same evening Dr. Galle compared the appearance of the heavens with the map, and found that the latter did not contain a star of the eighth magnitude which was situate very near the place indicated by Le Verrier as the locus of the disturbing body. The observations of the following evening decided that this was the Trans-Uranian planet. It was then retrograding with a daily motion in right ascension amounting to 62″. The following is a comparison of the results of observation and theory.

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* Many astronomers in other countries of Europe, as well as Germany, have lent their aid in the construction of these maps. One of them was executed by Dr. Hussey, the

Observed Right Ascension 23rd September, $12^h\,0^m\,15^s$ M. T. Berlin	328° 19′ 16″
Observed Declination South	13° 24′ 8″·2

Whence—

Geocentric Longitude	325° 53′
Parallax of the Orbit	1° 4′
True Heliocentric Longitude	326° 57′
Longitude for the same instant, assigned by Le Verrier's theory	326° 0′
Difference between Observation and Theory	57′

Thus it appears that the place assigned by Le Verrier to the disturbing body did not differ by so much as one degree from its actual place as indicated by observation. Nor was the agreement less striking with respect to the apparent diameter of the planet. Le Verrier had predicted that it would be equal to 3″.3; the micrometrical observations of the Berlin astronomers gave 3″ as the real value.

The accounts of the discovery of the Trans-Uranian planet were received with admiration and delight by all who felt any interest in the cause of science, and the name of Le Verrier was henceforth associated with those illustrious philosophers who have stamped the age in which they lived with the impress of their genius. We shall now give a brief account of the labours of Professor Challis, who had undertaken a very laborious examination of the heavens in search of the planet. We have mentioned that he commenced his observations on the 29th July. His plan was to divide the region to be explored into zones of 9′ in declination, this being the breadth of the field of view of the telescope when a magnifying power of 166 was employed, and to note the positions of all the stars in each zone down to the eleventh magnitude. On the 4th August his observations were made wholly in declination, for the purpose of obtaining a number of stars as reference points. On the 12th of the same month he noted the positions of all the stars in a zone which he had already examined on the 30th July. He compared to a certain extent the observations of the two evenings, and, having discovered their complete accordance, he felt assured that his method of search might be relied upon. He continued his observations throughout the months of August and September. On the 1st October he was made acquainted with the discovery of the planet by Galle. He had then recorded the positions of 3150 stars, and was making preparations to map them. The necessity for this operation having ceased, he proceeded to discuss the observations, with the view of ascertaining whether they had secured the discovery of the planet. Having resumed the comparison of the observations of the 30th July and the 12th August, which both related to the same zone, he found that a star, marked No. 49 in the series of the 12th August, was wanting in the series of the 30th July. It followed, as a necessary consequence, that this was the planet: it had wandered into the zone during the period that elapsed between the two observations. He also easily ascertained, by means of the observation of the 12th August, that the planet was included in the stars observed as reference points on the 4th of the same month*. Thus, although the 12th August was only the fourth day of observing, two positions of the planet were already secured. "This is entirely to be attributed," says Professor Challis, "to my having on those days directed the telescope towards the planet's theoretical place, according to instructions given in a paper Mr. Adams had the kindness to draw up for me."†

It is a remarkable fact that before receiving intelligence of the discovery of the planet by Galle, Professor Challis had also obtained a position of the planet by pursuing the plan of observation recommended by Le Verrier. On the 29th September he received the *Comptes Rendus* for the 31st August, containing the account of that geometer's researches on the hypothetic planet. Struck with the author's conclusions relative to the limits of the planet's position and the magnitude of its disk, he resolved to attempt the discovery of the body by means of its physical appearance. He possessed an advantage in prosecuting a search of this kind which few astronomers enjoyed in an equal degree, from having at his command the magnificent equatorial belonging to the Observatory. On the evening of the 29th September he examined a zone comprised between the limits of right ascension, within which Le Verrier had fixed the position of the planet. Among 300 stars which passed through the field of view of his telescope, one especially attracted his attention by its disk. This proved to be the planet; it shone with the lustre of a star of the eighth magnitude.

The search commenced by Professor Challis on the 29th July, and prosecuted with so much energy and perseverance during the two following months, is deserving of attention, both on account of its forming the only systematic attempt to detect the planet that had been made previous to the evening of its actual discovery, as well as on account of the results which were obtained by the subsequent discussion of the observations. It appears that not only was the theoretical discovery of the planet first effected at Cambridge, but two positions of it were also secured at the same place six weeks before a telescope was directed to the heavens in search of it at any other observatory in Europe. In estimating how nearly Professor Challis had arrived at the actual discovery of the planet, it must be borne in mind that the contemplated search for it extended only to a definite region of the heavens, and that it would have been completed within a definite lapse of time. When the observations came to be discussed, the discovery of the planet would infallibly have resulted from a comparison of the observation of the 30th July with that of the 12th August.

The earliest announcement of Mr. Adams' researches through the

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* The following are the positions of the planet in right ascension and declination, as assigned by these observations :—

	M.T. Greenwich.	Right Ascension.	North Polar distance.
August 4 . .	$13^h\ 26^m\ 25^s$ . .	$21^h\ 58^m\ 14^s.70$ . .	$102^\circ\ 57'\ 32''.2$
„ 12 . .	13 3 26 . .	21 57 26.13 . .	103 2 0.2

† Report to the Syndicate of the University of Cambridge.

medium of the press was contained in a letter from Sir John Herschel to the editor of the *Athenæum*, which appeared in the number of that Journal for the 3rd October, 1846. The illustrious philosopher, after referring to the words he used at the meeting of the British Association held at Southampton in the previous month, relative to the probable existence of a planet exterior to Uranus and the prospect of its speedy discovery by the aid of analysis, remarked that he would not have expressed himself in such confident terms on that occasion if he had not been already aware that Mr. Adams, a young mathematician of Cambridge, had been prosecuting researches similar to those in which M. Le Verrier was engaged, but at the same time totally independent of them, and had arrived at results respecting the actual position of the disturbing body which almost coincided with those deduced by the French geometer*. This announcement was followed by a letter from Professor Challis, which appeared in the *Athenæum* of the 17th of the same month. The writer gave a brief account of Mr. Adams' labours and of the systematic search which had been undertaken at Cambridge with the object of discovering the planet by its zodiacal motion. He also stated that the observations had been discussed, subsequently to the actual discovery of the planet, at Berlin, whence it was found that the discovery of the disturbing body had been secured by means of the observations of the 30th July and 12th of August. He concluded by intimating that the details of Mr. Adams' researches would shortly be published. The statements of Sir John Herschel and Professor Challis were corroborated at the same time by Mr. Airy in a letter addressed to M. Le Verrier.

In France the announcement of Mr. Adams' researches on the Perturbations of Uranus gave rise to a strong manifestation of national feeling. Nor can it be denied that the occasion chosen for preferring the claims of the English mathematician was unfavourable to their ready reception in that country. Amid the universal applause so justly excited by the brilliant researches of M. Le Verrier, it could hardly be expected that the announcement of similar researches prosecuted independently of them in another country, terminating in similar results, and therefore claiming by implication an equal degree of credit, would be received without some degree of reluctance, or discussed with a total absence of passion, by a people especially sensitive on points of national glory. But although this circumstance may account for the absurd violence with which a portion of the French press assailed the eminent astronomers who first announced the labours of Mr. Adams, it does not by any means explain, far less does it justify, the ungenerous aspersions that were cast upon the researches of the English geometer—while the means of ascertaining their real character had not yet been laid before the public—by persons whose position in the scientific world ought to have served as a guarantee for greater discretion. It was at once *assumed* that Mr. Adams' solution of the inverse problem of perturbation was a crude essay which could not endure the test of rigorous examination, and it was urged with equal precipitancy, that as his researches had not been duly published, it would be impossible to establish their claim to originality. On the other hand, as their merits were attested by the first astronomers of England, it was obvious that they could not be treated with indifference. Under such circumstances the course obviously suggested by a due regard to the sacred claims of right and the dignity of science, would have been to suspend judgment on the question altogether, pending the publication of Mr. Adams' researches which were announced as speedily forthcoming. M. Arago, actuated, beyond doubt, by a laudable desire to defend the rights of his countryman from what he conceived to be an unjustifiable aggression, took a different view of the matter, and at once undertook a critical examination of the merits of Mr. Adams in regard to the theoretical discovery of the Trans-Uranian planet. The paper which he drew up on this occasion is inserted in the *Comptes Rendus* for the 19th October, 1846*. We shall not here make any allusion to the assumptions which M. Arago so unwarrantably employs in the absence of acknowledged facts, with the view of preserving the consistency of his reasoning. Their fallacy was effectually exposed a few weeks afterwards, without any controversy, by the publication of the admirable researches of Mr. Adams, and of the important mass of documentary correspondence in the possession of Mr. Airy, relative to the discovery of the planet. We shall merely make a few remarks on the main principle laid down by him, and the conclusion which he seeks to draw from it. He assumes, as the basis of his reasoning, that the only rational mode of writing the history of the sciences is to rely exclusively on publications, the dates of which are well ascertained, and he hence infers that Mr. Adams has no right to be mentioned in connexion with the discovery of the Trans-Uranian planet, either by a detailed citation, or by any allusion whatever. It will at once occur to the reader that the force of this proposition depends entirely on the precise meaning applied to the term *publication.* If it be restricted to *printed* documents, we conceive that the principle advanced by M. Arago is totally untenable, and that it essentially vitiates any conclusion that may be derived from it. To limit the test of decision in disputed questions of scientific discovery to such a species of evidence would be at once repugnant to reason and at variance with the common practice of mankind in all ages. Are communications transmitted to learned societies, or documents of an authentic character, addressed to persons of acknowledged probity and occupying official positions, to be set at nought as so much waste paper, merely because they have not passed through the ordeal of the printing press? To admit such a monstrous proposition would be striking at the root of those unalterable principles of common sense upon which our primary notions of evidence are founded, and to introduce in their stead an arbitrary standard of decision which, so far from defining clearly the rights of rival claims, would itself perpetually form the subject of acrimonious controversy. We have no reason however to suppose that the illustrious philosopher, who drew up the paper in question, was really of opinion that the evidence which could be of any utility in establishing claims to scientific research rested on so narrow a basis. On the contrary, we are disposed to infer from the acknowledged acuteness of his discriminating powers, and his enlightened zeal in the cause of truth, that his sentiments as above expressed, demand a more liberal interpretation, and that in the present instance, had he been in possession of all the facts relating to the disputed question, he would have arrived at a conclusion more consistent with reason and justice than that to which he was conducted by the imperfect statements then accessible to him *.

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* Sir John Herschel was present at the meeting of the Board of Visitors, held at the Observatory of Greenwich on the 29th June, 1846 (see p. 184), when Mr. Airy gave an account of the researches of Messrs. Adams and Le Verrier.

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* This paper contains a view of certain communications of Professor Challis which tends at first appearance to lead to a conclusion totally inconsistent with the real statements of the English astronomer. M. Arago asserts that Professor Challis, writing to him on the subject of the planet, uses the following words:—"I became acquainted on the 29th September with the final researches of M. Le Verrier; I conformed strictly to the suggestions of that astronomer, and confined my search within the limits assigned by him." He then remarks that Professor Challis in his letter to the *Athenæum*, announces that he was guided in his search for the planet by a paper which Mr. Adams drew up for him. Having placed these two apparently contradictory statements in juxtaposition, M. Arago simply contents himself with the following comment upon them: "I will not seek to reconcile these two versions. I will leave to Mr. Challis the task of explaining how the name of Adams, which did not figure in his first communication, is become so prominent in his second." Now, what are the real facts of the case? In the *Comptes Rendus*, tome xxiii. p. 764, there appears a letter from Professor Challis to M. Arago, dated the 5th October, 1846, in which the writer gives an account of his search for the planet with the Northumberland telescope, and mentions his having detected it by its disk on the evening of the 29th September. In this letter he alludes to M. Le Verrier in the terms quoted by M. Arago as above stated. Again, in a letter to the editor of the *Athenæum*, dated the 15th October, 1846, (see *Athenæum*, October 17,) Professor Challis gives an account of the laborious search undertaken by him, having for its object to detect the planet by its zodiacal motion. In this letter he states that he was guided in his search by a paper which Mr. Adams drew up for him; that he commenced his observations on the 29th July; and that a comparison of the observations of the 30th July and 12th of August, instituted subsequently to the receipt of the accounts of Galle's discovery, shewed him that he had secured the planet. The reader will at once perceive from the foregoing statements that the two communications of Professor Challis referred to two distinct modes of search prosecuted on two distinct occasions. In his letter to M. Arago he mentions the result of observations pursued with the view of detecting the planet by its *physical aspect*, professing to have been guided by the instructions of M. Le Verrier. In his letter to the *Athenæum* he communicates the result of observations anterior to the preceding that had been made with the view of discovering the planet by its *zodiacal motion*, asserting to have been guided on this occasion by Mr. Adams. It was impossible that the instructions contained in M. Le Verrier's paper of the 31st August, and first made known to Professor Challis on the 29th September, could have been of any service to that astronomer in a search for the planet prosecuted by him in the beginning of the former of these months. A mind much less acute than M. Arago's will, therefore, see no inconsistency in the two passages which that illustrious philosopher appears to despair of reconciling together. How justly may we remark in his own words, "L' amitié est souvent aveugle, et se laisse fasciner."

The question of Mr. Adams' merits in connexion with the theoretical discovery of the planet exterior to Uranus, was soon placed in a clear light. On the 13th of November, 1846, a paper was read by Mr. Airy before the Astronomical Society, entitled "An Historical Statement of Circumstances connected with the Discovery of the Planet beyond Uranus." This valuable communication, besides containing a large mass of interesting materials bearing more or less on the discovery of the planet, exhibits, in one unbroken chain of correspondence, the progress of Mr. Adams' researches, from the date of Professor Challis' first letter to the Astronomer Royal on the subject in February, 1844, down to the acknowledgment at Greenwich of Mr. Adams' paper, dated the 2nd September, 1846. The independent character of Mr. Adams' researches was now placed beyond all doubt or equivocation, and his claims to the theoretical discovery of the planet established on the incontrovertible evidence of authentic documents. Mr. Adams' Memoir on the Perturbations of Uranus was read before the Astronomical Society on the same day on which Mr. Airy communicated his statement, and was subsequently published in the sixteenth volume of the Memoirs of the Society, and also in the *Nautical*

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* See in connexion with these remarks the note at the foot of page 214, in the next chapter.

Almanac for 1850. If anything was wanted to complete the vindication of Mr. Adams' claims, it was amply supplied by this Memoir. Apart from all consideration of the brilliancy of the final result in relation to the planetary system, it exhibits an admirable specimen of the application of analysis to one of the most difficult subjects of physical astronomy, being planned with a sagacious appreciation of the difficulties peculiar to such abstruse researches, and pursued throughout its details with exquisite mathematical skill.

The impression produced by the communication of Mr. Airy to the Astronomical Society was such as always naturally ensues when truth is presented in the form of a plain statement of facts, stripped of all ingenuity of reasoning or flourish of rhetoric, and relying for acceptance solely on the authenticity and innate strength of the evidence by which it is supported. The most illustrious philosophers, of Europe, while justly acknowledging the originality and brilliancy of M. Le Verrier's researches on the perturbations of Uranus, have cordially concurred in awarding a similar tribute to Mr. Adams, and the names of both these geometers are now imperishably associated with the theoretical discovery of the planet by which these perturbations were produced.

It has been asserted that as M. Le Verrier's researches alone were instrumental in leading to the actual discovery of the planet by Galle, Mr. Adams, whose labours exercised no influence on the observations of the German astronomer, cannot justly be placed in so high a position as his illustrious contemporary. This opinion we imagine to have originated in an imperfect discrimination of the respective functions of the geometer and the observer. The geometer, relying upon the firmly-established principles of the theory of gravitation, recognises in the irregularities of one of the planets unequivocal evidence of the existence of an unseen member of the solar system, and, by a successful application of analysis, he arrives at a knowledge of its position. The observer, guided by the instructions of the geometer, searches the heavens, and succeeds in discovering *optically* what the geometer had previously discovered *theoretically*. Each has his peculiar duties, totally distinct from those of the other; and we should be acting in opposition to the plainest maxims of justice by ascribing to the one any credit on account of duties performed by the other. The *éclat* which so justly surrounds the *optical* discovery of the Trans-Uranian planet must be shared severally by the Berlin Academy of Sciences under whose auspices the charts of the zodiac have been constructed, by Dr. Bremiker who so skilfully designated the region of the heavens in which the planet was moving, and by Dr. Galle, who so promptly availed himself of the publication of Dr. Bremiker's chart. If we would look for the real grounds of M. Le Verrier's renown, we must abandon the glorious spectacle of the heavens and enter the solitary chamber of the geometer. It is there that he justly becomes the object of our admiration, as he advances step by step along the intricate maze of his researches, vanquishing each successive difficulty by the ready resources of his genius, and cheerfully executing calculations which are almost appalling to contemplate; until a bright flood of light is finally diffused over his labours, and the distant member of our system, which human eye has not yet seen, discloses itself to his purely intellectual scrutinies with all the certainty of demonstrative reasoning. If this be the take a similar view of the equally admirable labours of Mr. Adams. Nor can the award of equal merit be withheld, on the ground that Mr. Adams reserved the secret of his researches to himself, while M. Le Verrier openly promulgated his results to the world, and thereby put astronomers in possession of the means of actually discovering the planet. This objection is untenable, for the simple reason that it is directly at variance with acknowledged facts. Mr. Adams communicated his results to two of the most influential astronomers of England, whose co-operation, as we have already had occasion to remark, sufficed to *secure* the optical discovery of the planet. That the laborious search undertaken at Cambridge was anticipated as regards the final result by the more simple procedure adopted at Berlin, cannot, without a flagrant violation of the rules of justice, be considered derogatory to the merits of Mr. Adams. It was sufficient that he had communicated his results to competent astronomers. It would be totally inconsistent with reason to hold his fame responsible for any ulterior proceedings. We have already mentioned the peculiar advantage enjoyed by the Berlin astronomers in searching for the planet. If a similar facility of search had been accessible at the Observatory of Cambridge, it is admitted by all persons that the planet could not fail to have been first discovered there*. This is a question, however, which concerns Professor Challis, but does not, in the remotest degree, affect the purely theoretical labours of Mr. Adams.

We have been induced to submit to the reader the foregoing remarks, because we conceive that the brilliant result achieved by the Berlin astronomers has had a tendency to foster erroneous ideas relative to the merits of the various persons, whether mathematicians or astronomers, whose labours have been more or less associated with the discovery of the Trans-Uranian planet. It is only, however, by persisting to confound things totally dissimilar in their nature, that any such notions can retain a permanent hold on the mind.

Soon after the discovery of the planet, an attempt was made in this country to exhibit in an unfavourable light the conduct of the astronomers to whom Mr. Adams had communicated his results towards the close of the year 1845. They were charged in vehement terms with lukewarmness in the cause of science, as well as indifference to the honour of the country and the reputation of Mr. Adams, on the ground that they did not institute an *immediate* search for the planet which theory had indicated so clearly to exist. It was alleged that the execution of this task devolved more especially upon them, inasmuch as they were the official astronomers of the country, and that, by neglecting to carry into effect the proposed search, time was allowed for a rival to step into the field and to divide with Mr. Adams the honour attached to the theoretical discovery of the planet.

It is hardly necessary to state that this accusation is the offspring of indiscriminating zeal or personal prejudice, rather than the result of a dispassionate examination of facts. We are confident that the simple perusal of the foregoing account will be amply sufficient to convince any unbiassed mind of the justness of this assertion. As, however, the charge is not destitute of plausibility, and as some of our readers, who have not bestowed sufficient attention on the subject, may have been misled to some extent by this groundless clamour, we deem it not altogether out of place to submit a few remarks here in relation to it. We have not indeed the slightest doubt that if a diligent search of the heavens had been instituted in the month of November, 1845, it would have resulted in the discovery of the planet. But, while fully conceding this point, we are by no means prepared to admit that existing circumstances would have warranted the immediate appropriation of the resources of any official Observatory to such an object. It must be borne in mind that Mr. Adams' results were the fruits of the first solution of the problem of planetary perturbation which the geometer, aided by analysis, had yet arrived at. It was reasonable then to suppose that the position of the planet was not assigned with a high degree of accuracy, and that, in order to secure its discovery, a considerable region of the heavens would require to be submitted to a careful examination. In carrying this operation, however, into effect, no aid could be derived from any previous labours in Uranography, since a star map had not yet been constructed for the part of the zodiac in which the planet was then moving. The search, therefore, could only be accomplished by the prosecution of an extensive course of observations, similar to that which Mr. Challis undertook in the following year. Under such circumstances it was imperative on the part of the astronomer, charged with official duties, to exercise due discretion in selecting the most favourable period for devoting the resources at his disposal to a systematic search for the planet. This line of conduct was more especially prescribed in the present case, as two months had already elapsed since the planet was in opposition, and the chance of effecting its discovery before it was lost in the rays of the sun had in consequence considerably diminished. The subsequent history of the circumstances connected with the discovery of the planet confirm this view of the subject. Although M. Le Verrier announced the existence of the undiscovered body as early as the 1st June, 1846, and confidently asserted, as the result of a careful analysis, that the error in the position he assigned to it did not exceed 10°, it does not appear that a single telescope was directed to the heavens in search of the disturbing body at any Observatory on the Continent of Europe previous to the night of its actual discovery towards the close of the month of September of the same year. Even at the Royal Observatory of Paris, which is under the direction of the illustrious philosopher who originally suggested to M. Le Verrier the subject of the perturbations of Uranus, and where it might have been expected that the remarkable results obtained by the geometer would have excited peculiar interest, no steps appear to have been taken towards the actual discovery of the planet, although a month had elapsed between M. Le Verrier's announcement of his final results on the 31st August, 1846, and the receipt of the intelligence from Berlin, containing the accounts of Galle's discovery. That a search for the disturbing body would eventually have been undertaken at more than one Observatory is quite certain; but it was not the opinion of any astronomer, that without the aid of a star map, this object could be successfully accomplished, except by an extensive and systematic course of observation. "Had it not been for the infinitely favourable circumstance," says M. Encke, "of possessing a map whereon one might be sure to find the positions of all the fixed stars down to the tenth magnitude, I do not think that we should have found the planet."* These remarks will appear superfluous to those who have any practical knowledge of astronomy. They are addressed especially to the general reader, who might be led by misrepresentation to form inaccurate views on the subject. Men of science have all been delighted and astonished that the planet was discovered *so soon.* Only those who make the cause of science subservient to the miserable gratification of personal feeling have sought to indulge in opprobrious language, because the discovery did not take place *sooner.* Such persons, true to their instincts, would have been the first to hold up to public ridicule the credulity of the planet hunters, and to raise the charge of a wasteful expenditure of the country's resources, if the speedy discovery of the disturbing body had not indicated a different, though equally unjustifiable, ground of obloquy.

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* "Si M. Challis s'en fût servi (une carte des étoiles) au lieu de suivre une marche plus penible, il n'eût pas manqué une découverte qui échappa en pareille circonstance à Lemonnier et à Lalande."—Extract of a Letter from M. Valz to M. Arago, *Comptes Rendus*, tome xxiv. p. 880.

It would be an invidious task to institute a comparison between the respective merits of Le Verrier and Adams in connexion with the immortal discovery, of which we have endeavoured to give some account in the preceding pages. We are of opinion that the labours of both these geometers are equally calculated to excite admiration. The annals of science do not contain a brighter page than that which records the progress of M. Le Verrier's labours as he advances from the ill-defined irregularities of Uranus to the precise position of the undiscovered planet. The care with which he scrutinizes every fact; the vigilance he exhibits in detecting every imaginable source of error, and the thorough manner in which he sifts all its parts; the ingenuity and conclusiveness of his methods, and his indomitable perseverance in calculation—indicate, in a high degree, the possession of those qualities which constitute the main elements of success in all researches relating to the physico-mathematical sciences. Nor does a review of Mr. Adams' labours offer a less pleasing picture. We see the obscure undergraduate, while his attention is yet distracted by the routine of academic discipline, seizing with the happy intuition of genius the true theory of Uranus, and forming the bold resolution of tracing it to its final results. The constancy with which he afterwards struggles to effect this object, notwithstanding the manifest disadvantages of his position, is equalled only by the masterly character of his analytical researches and the brilliant termination to which he conducts them. It is gratifying to reflect that the labours of both Le Verrier and Adams, in connexion with the perturbations of Uranus, are so completely dissociated, that no danger of a mis-statement of facts can exist in the discussion of their relative merits. Differences of opinion on this last point will, no doubt, always prevail; but we are confident that future ages will concur with the present in awarding to each geometer the tribute of unqualified admiration.

The discovery of the planet Neptune (for such is the name by which Astronomers have agreed to distinguish the Trans-Uranian member of the Solar System) marks an important epoch in the history of physical astronomy. Hitherto the object of the geometer had been to unfold by a deductive process the principles of perturbative influence, and to explain by them the various phenomena of the planetary motions. It was only in the determination of the masses of the planets, and in assigning the ratios of their polar and equatorial axes, that the order of inquiry was reversed, and an accurate knowledge of the disturbing influence was sought to be established by reasoning upwards from its observed effects. In each of these cases, however, the equations of condition are of extreme simplicity, and the value of the final results is dependent much less upon the skill of the geometer than upon the accuracy of the fundamental observations. The idea of submitting to a similar treatment the problem of three bodies does not appear to have occurred to any of the great geometers whose names are associated with the development of the theory of gravitation. Nor is this circumstance calculated to excite surprise, for the actual state of Physical Astronomy had not yet demanded such an advanced step. It was only when all the consequences resulting from the mutual action of the planets already known had been fully deduced, and the outstanding irregularities had assumed the form of residual phenomena depending on some foreign influence, that further speculation suggested the expediency of inverting the usual order of investigation. It is manifest from this circumstance, that the complete establishment of the formulæ of planetary perturbation must have preceded any attempt to ascend from the effects produced by an unseen planet to the determination of its actual position. The accomplishment of this latter object is therefore an indication of the highly-advanced state of physical astronomy, since it implies not only that the difficulties peculiar to the inverse problem of perturbation have been successfully overcome, but also that the irregularities occasioned by the mutual action of the planets have been deduced from the principles of the Newtonian theory, and have, in all instances, been found to accord with the results derived from observation. This remark does not, of course, apply to the planet which theory has recently revealed to us, since a considerable time must elapse before an accurate knowledge of the inequalities of its motion can be obtained. It will then be an interesting point to ascertain whether these inequalities do not in their turn afford indications of the existence of a still more remote member of the solar system. The astronomer is thus led to speculate on the *theoretical* discovery of planets, reflecting too feeble a light on account of their immense distance from the sun, to be ever visible, even by the aid of the most powerful telescopes. It is to be hoped, that notwithstanding the abundant harvest, which has been already reaped in Celestial Mechanics, that magnificent region is destined still to afford more profitable fields for the application of the resources of analysis than that which the imagination here suggests.

CHAPTER XIII.

The Elements of the Planet Neptune deduced from Observation.—They are found to be Discordant with the Results of Theory.—The cause of Discordance assigned.—The Planet observed by Lalande.—Theory of its Perturbations.—Researches on the Value of its Mass.—Uncertainty respecting this Element.—Researches of M. Hansen on the Lunar Theory.—Conclusion of the History of Physical Astronomy.

As soon as astronomers received intelligence of the discovery of the Planet Neptune, the new member of the solar system was regarded with intense interest, and accurate observations of it were made both in Europe and America. When the elements of the orbit were calculated from these observations, a comparison of the results with those assigned by the theories of Le Verrier and Adams led to rather unexpected conclusions. Although the orbits assigned by these geometers to the disturbing planet did not differ materially from each other, they both, on the other hand, exhibited a very marked discordance with the real orbit, as indicated by observation. It was found that the orbit in which the planet actually revolved was much smaller than either of those deduced from theory, and that, instead of being very eccentric, it approached very nearly to a circular form. The following are the elements calculated from observation by Mr. Walker of Washington, U.S.:

Mean distance	30.0363
M. Long. January 1, 1847; M. T. Greenwich	328° 32′ 44″.20
Eccentricity	.00871946
Long. of Perihelion	47° 12′ 6″.50
Long. of Ascending Node	130 4 20 .81
Inclination	1 46 58 .97
Mean Daily Motion	21″.55448
Periodic Time	164.6181 trop. years.

Elements of the planet's orbit were also calculated by other astronomers, and the results were found to agree very nearly with those above given. It appears that the mean distance, instead of being nearly double the mean distance of Uranus, amounts only to about two-thirds of it. The law of Bode, therefore, which is so remarkably applicable to the other members of the planetary system, totally fails in this case. The general discordance of the elements with those severally assigned by the two geometers who were led to the theoretical discovery of the planet, at first occasioned considerable surprise, and it was suspected that some difficulty would be experienced in rendering a satisfactory account of its origin. A little reflection, however, served to arrive at clearer views on the subject. In order that the reader may understand how elements so remote from the truth as those of Le Verrier and Adams, could have sufficed to effect the theoretical discovery of the planet, it is necessary to form a distinct conception of the nature of the problem, by the solution of which these geometers were conducted to their respective results. The data of this problem were the observed perturbations of Uranus, and the main object to be accomplished was to determine the position in the zodiac occupied at any assigned instant by the disturbing body so as to arrive at its actual discovery. Now, the derangement occasioned in the motion of any planet by the action of another planet upon it depends on the intensity and direction of the disturbing force at each instant; and these again depend on the mass of the disturbing body, and on its distance and longitude with respect to the sun. It is manifest, therefore, that only those elements which will *accurately* assign the distance and longitude of the disturbing body will render a *complete* account of the perturbations of Uranus. But if the values of the heliocentric co-ordinates should not be absolutely correct, still, if they approach with tolerable approximation to the true values, it is not difficult to perceive that by a due adjustment of the mass, the intensity and direction of the disturbing force will be represented with a corresponding degree of precision. Under such circumstances the anomalies of the disturbed planet will be accounted for with nearly as great fidelity as if the disturbing planet were in its true place, for the error of perturbation is obviously of an order inferior to the error in the place of the disturbing body. Now, although it is impossible *permanently* to represent, even with tolerable accuracy, the heliocentric co-ordinates of a body revolving in an elliptic orbit by means of any elements which differ from the true elements, still, when the question refers only to a *section* of the orbit, this object may be accomplished by employing indefinite combinations of elements, differing very considerably from each other. This will be readily understood when it is borne in mind that the theory of elliptic motion assigns four arbitrary constants, which may be modified in a variety of ways, so as to answer the purpose of mutual correction; and that on account of the smallness of the arc described by the body, the outstanding errors, which inevitably exist in all such cases, are not allowed time to develope themselves to any serious extent. In the case of Neptune disturbing Uranus, the perturbations are sensible only a little before and after conjunction. Throughout the whole period embraced between 1690, the year of the earliest observation of Uranus, and the commencement of the present century, the action of the disturbing planet has been quite inappreciable; and consequently the tabular errors of Uranus for that period may be considered as wholly explicable by the errors of the elliptic elements. The last conjunction of the two planets took place in the year 1822, and the action of the disturbing planet was sensible only during about twenty years anterior to that event, and the same number of years subsequent to it. It is manifest, therefore, that any elements which will afford a pretty accurate representation of the heliocentric co-ordinates of Neptune during the present century, will account with sufficient fidelity for the perturbations of Uranus during the same period. Conversely, if the perturbations are faithfully accounted for, we may conclude that the theory is capable of representing the co-ordinates of the disturbing planet with considerable precision, and that they may be employed with confidence for the purpose of its actual discovery. That the theories of Le Verrier and Adams were capable of so representing the co-ordinates of Neptune during the whole period when its action was sensible, may be seen from the following table, which exhibits the actual and theoretical values of the longitude and radius vector for the beginning of each of the years specified between the years 1800 and 1860:—

	Planet Neptune.		Theory of Le Verrier.		Theory of Adams[*].			
					1st Approximation.		2nd Approximation.	
Year.	Longitude.	Rad. Vec.	Longitude.	Rad. Vec.	Longitude.	Rad. Vec.	Longitude.	Rad. Vec.
1800	226° 4′	30.30	231° 34′	33.57	236° 46′	36.31	238° 9′	34.90
1810	247 20	30.28	251 10	32.80	254 13	34.75	256 39	33.92
1820	268 52	30.23	271 28	32.35	273 11	33.45	276 5	33.25
1830	290 31	30.55	292 8	32.29	293 27	32.56	295 54	32.96
1840	312 17	30.06	312 36	32.63	314 30	32.22	316 10	33.11
1850	334 12	29.96	332 25	33.32	335 36	32.48	335 50	33.67
1860	356 14	29.87	351 17	34.26	356 1	33.30	354 39	34.57

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[*] Mr. Adams' theory is founded upon a method of successive approximation. A certain value of the mean distance is first assumed, and by a comparison of the results calculated by its aid with those derived from observation, an indication is obtained of the direction in which the error in the mean distance lies, and also of its probable magnitude. The solution is then repeated with a new value of the mean distance, suggested by the original solution, and a further approximation is obtained. This is the method of

It is manifest that any two corresponding co-ordinates of either of the theories contained in the above table might have been employed with success in searching for the planet. Even twenty years before conjunction the error of its position as assigned by Adams' first approximation amounts to little more than 10°, a quantity which falls considerably within the range of search proposed by the Astronomer Royal to Professor Challis, in the month of July, 1846. The reader will not fail to remark, that the longitudes are generally represented with greater accuracy than the distances. This circumstance admits of easy explanation. The intensity of the disturbing force in any given configuration of the two planets depends on the mass and distance of the disturbing planet. Now, if the distance be made too great, the disturbing force will be enfeebled in a corresponding degree; but this effect may be obviated by a suitable increase of the mass. This is precisely what happens in the theories of Le Verrier and Adams. The distances are all too great in both theories; but, on the other hand, the mass in each case is considerably enlarged beyond its true value. The perturbations of Uranus may therefore be accounted for, in so far as the *intensity* of the disturbing force is concerned, even although the radius vector of the theoretical planet should be considerably erroneous. The mass, however, exercises no influence in determining the *direction* of the disturbing force; and if the latter element be erroneous to any great extent, the perturbation will be necessarily erroneous also, since it cannot derive compensation from any other source. Hence arises the necessity of a comparatively higher degree of accuracy in the representation of the longitudes of the theoretical planet.

When the elements of Neptune were determined with a degree of precision sufficient to enable astronomers to trace its motion through the anterior part of its orbit, attempts were made to ascertain whether it had been observed on any occasion previous to its discovery as a planet by Dr. Galle. It was soon found by Dr. Petersen, of Altona, and Mr. S. Walker, of Washington, that a star in the *Histoire Celeste* of Lalande, observed May 10, 1795, and since missing, could be no other than the planet Neptune. The place of the star being marked doubtful, the French astronomers were induced to examine the original manuscripts of the *Histoire Celeste*, which are deposited in the Royal Observatory of Paris. An inspection of the observation established the identity of the planet with the recorded star, and disclosed an additional fact of extreme interest. It appeared that the planet was also observed on the 8th May, 1795, and that its right ascension and declination were regularly recorded, although they were not subsequently inserted in the printed catalogue. The discordance of the two positions May 8–10, in a case where identity was looked for, beyond doubt suggested to Le François Lalande a suspicion of the accuracy of his observations, and induced him to suppress altogether the observation of May 8. A comparison of the two observations clearly

investigation generally employed in physical astronomy. In his first solution, Mr. Adams assumed that the mean distance of the hypothetic planet was equal to 38.4; in his second solution he made it 37.5; and in his communication to the Astronomer Royal, dated September 2, 1846, he stated, as the result of further discussion, that 33.6 would probably be a very near approximation. The actual mean distance in fact is 30.04. Thus we see that Mr. Adams was fairly on the track of the true orbit. The method employed by M. Le Verrier is of a more ambitious character, but, unfortunately, it is not adequate to meet all the difficulties of such abstruse enquiries, and in the present instance it had the effect of betraying M. Le Verrier into error with respect to the limits of the mean distance and the other elements of the orbit.

exhibits the retrograde motion of a planet, and assigns differences of right ascension and declination, agreeing almost exactly with those indicated by the motion of the planet Neptune.

An examination of the mutual action of Uranus and Neptune illustrates some very interesting points in the theory of planetary perturbation. In consequence of the mean motion of the former of these planets being only a small fraction less than twice the mean motion of the latter, the perturbations which recur in every synodic revolution of the two planets, assume a resemblance to those which take place in the theories of the first and second, and of the second and third satellites of Jupiter. This near commensurability of the mean motions, in fact, introduces into the orbit of each planet a considerable eccentricity, dependent wholly on perturbation, and distinguishable from the permanent eccentricity of the orbit by the constant coincidence of the line of apsides with the line of conjunction of the two planets. In the case of Neptune disturbing Uranus, the aphelion of the movable ellipse is constantly turned towards the point of conjunction. On the other hand, when the action of Uranus upon Neptune is considered, it is the perihelion that is turned towards the same point. In another respect the perturbations of the two bodies resemble those of various other primary members of the planetary system. The near commensurability of the mean motions, combined with the permanent eccentricities of the orbits, gives rise to an inequality of long duration, similar to that in the theories of Jupiter and Saturn, or any of the other long inequalities to which we have had occasion to allude. The magnitude and duration of this inequality cannot be ascertained with precision until astronomers arrive at an accurate determination of the elements of the two planets. This remark applies more especially to the mean distances, a very small error in their relative values producing an enormous influence on the final results. If we adopt 30.2026 for the mean distance of Neptune, being one of the earlier values deduced from observation, the duration of the inequality will be 6820 years. On the other hand, if we employ Mr. Walker's determination of the mean distance, which differs from the preceding value only by $\frac{1}{182}$, the duration of the inequality will only extend to 4051 years. The *magnitude* of the inequality will vary in a still greater proportion with the change of mean distance *.

The mass of Neptune is an element which, in the present state of the theory of the planet, is still involved in great uncertainty. A satellite which has been discovered by Mr. Lassel fortunately affords the means of determining its value independently of the perturbative action of the planet, but no satisfactory results on this point have been elicited by the researches of astronomers. M. Otto Struve, by means of his own observations, makes the period of the satellite $5^d\ 21^h\ 15^m$, and the semi-major axis of the apparent orbit $17''.89$. These numerical values indicate a mass equal to $\frac{1}{14494}$, the sun's mass being represented by unity. On the other hand, Professor Pierce of Harvard College, U.S., guided by similar observations of his countryman, Mr. Bond, has obtained $\frac{1}{19840}$ for the value of the planet's mass. This geometer has executed a detailed calculation of the perturbations which Neptune produces on Uranus, and has found that the mass deduced from Mr. Bond's observations of the satellite accounts better for the irregularities of the disturbed planet than any other value which has been assigned. When the difficulty of making observations of the satellite is taken into consideration, it is manifest that some time must elapse before the value of this element can be established with a degree of precision conformable to the present condition of Physical Astronomy.

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* It is worthy of remark that the inequality is greater in the former of these cases than in the latter, although the disturbing body is then more distant. Both the magnitude and duration of the inequality increase *ad infinitum* as the mean distance increases towards a

In one of the preceding chapters allusion has been made to the long inequality in the moon's epoch, which was the cause of so much embarrassment to astronomers in the present century, but of which the existence was placed beyond all doubt, and its true nature clearly established, by a discussion of the Greenwich lunar observations consequent upon their recent reduction. The complete explanation of this inequality by the theory of gravitation, was the reward of Professor Hansen's researches on the subject. Mr. Airy, by the discussion of the same observations, detected also two small periodical inequalities in the moon's motion, neither of which had hitherto been recognised by theory. The greater of these inequalities was in the moon's latitude. It varied with the cosine of the moon's true longitude, amounted at its maximum to 2″.17, and was additive. The other inequality was in the longitude. It varied with the cosine of the longitude of the node, had a maximum value equal to 0″.97, and was subtractive. These quantities are indeed very small, but such is the high degree of precision which characterizes modern observation, and such is the mathematical refinement attained in the correction of the lunar or planetary elements by the discussion of masses of observations, that the existence and true form of the inequalities was established beyond all doubt by Mr. Airy's researches. Observation and theory were therefore once more at direct issue, and it remained for the geometer to reconcile their discordance by demonstrating the existence of the discovered inequalities, as necessarily due to perturbative action. This important object has recently been accomplished by the same geometer to whom the explanation of the long inequality in the epoch of the moon is due. Professor Hansen, by a profound investigation of the lunar theory, has discovered two inequalities similar to those indicated by observation, and very nearly agreeing with them in their maximum values. The inequality in latitude was found by him to amount to 1″.38. It arises from this circumstance, that the plane to which the lunar orbit is inclined at an invariable angle is not the plane of the ecliptic as has been hitherto supposed, but a plane the position of which Professor Hansen represents by the following conception:—"Take the plane of the earth's orbit in the position in which it was about three years ago; let the nodes of this plane revolve backwards on the present ecliptic through 90°, without change of inclination; the plane so found is the plane to which the uniformity of the inclination of the moon's orbit is to be referred."* The same train of investigation gave also the inequality in longitude. M. Hansen obtained 0″.50 for its maximum value. The discordance between M. Hansen's results, and the values assigned to the inequalities by Mr. Airy, are not greater than such as may fairly be ascribed to errors of observation. Thus the clouds which for a moment obscured the Newtonian theory of gravitation have been effectually dissipated, and a fresh conquest has been added to the long list of triumphs which adorn its history.

With the foregoing brief allusion to the recent researches of M. Hansen on the lunar theory, our imperfect attempt to trace the history of physical astronomy is now brought to a close. A review of the progress of this sublime department of science from its origin to its present advanced state exhibits three periods, severally characterized by distinctive features, and separated by well-defined outlines. The first of the seperiods extends from the commencement of Newton's career to about the middle of the eighteenth century. It comprehends the establishment of the theory of gravitation by that immortal philosopher, the general propagation of its principles, and the efforts pursued by geometers with a view to its future development. The appearance of the *Principia* in 1689, is one of those conspicuous landmarks in the annals of science which enable the reader to group together a multitude of subordinate events, and to pursue his way with comparative ease along the intricate path of physical research. Guided by the maxims of a cautious philosophy in establishing his fundamental principles, and relying on the conclusions of a sublime geometry, the offspring of his own genius, Newton has in that work unfolded the mechanism of the material universe, and traced with complete success the agency of gravitation in all the grand phenomena of the planetary movements. The dim perceptions of Copernicus, the more searching and comprehensive but equally ineffectual speculations of Kepler, the sober conjectures of Borelli, and the acute surmises of Hooke, were now proved by demonstrative reasoning to be so many indistinct and scattered glimpses of one all-pervading principle which the author had the glory of first revealing to the world. The multitude of complex and apparently unconnected phenomena, which astronomers had detected by means of persevering efforts, prosecuted throughout a long succession of ages, now assumed the character of subordinate truths flowing from one common source. The sagacious generalizations of Hipparchus and Ptolemy among the ancients, and the equally admirable efforts of Tycho Brahe and Kepler among the moderns, were finally emancipated from the empiricism which characterizes mere mathematical inductions, and now stood forth invested with all the grandeur of natural laws. The general adoption of the theory of gravitation which took place towards the close of the period we are considering, affords a lesson at once interesting and instructive. Slow at first to admit principles which presented themselves in the austere garb of the ancient geometry, and violated the cherished creations of fancy, the human mind, notwithstanding, was unable effectually to resist the force of demonstrative reasoning, and finally was constrained to bow before the simple majesty of truth. By a rational exposition of their more salient features, the principles of the theory of gravitation were brought home to the understandings of persons unused to processes of scientific inquiry with a power of conviction which the more refined investigations of modern times are calculated only in a small degree to strengthen. With respect to the geometer, the impression produced on his mind by the discoveries of Newton was of a totally different character. Far from recognising a theory already complete in all its parts, he discovered, in the pages of the *Principia*, an inexhaustible mine of profound research, and already descried, in the dim perspective of futurity, the magnificent expansion which the sublime principles therein expounded were destined to acquire. But, to use the language of an illustrious living philosopher, the geometry of Newton was like the bow of Ulysses, which none but its master could bend *. The efforts of geometers during this period were therefore mainly directed to the improvement of the transcendental analysis, the only instrument that could be expected in future to afford any efficient aid in the abstruse researches relating to the system of the world, its applications being confined to problems of pure mechanics—the branch of science which forms the connecting medium between analysis and physics. The prodigious mass of original matter that had been presented in the *Principia* in a synthetic form supplied geometers with an inexhaustible field of delightful speculations of this nature, and gave birth to a multitude of analytical inventions which proved of incalculable service in the future development of the theory of gravitation.

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* Letter from Professor Hansen to the Astronomer Royal, Monthly Not. Ast. Soc., June, 1849.

The second period in the history of physical astronomy extends from the commencement of the labours of Euler and his contemporaries on the lunar theory to the close of the eighteenth century. Less fertile in original ideas than the period to which we have been referring, it notwithstanding exhibits a refinement of conception in fundamental principles, combined with a power of generalization in the methods of research, to which no scientific investigations in any other age can offer a parallel. These brilliant efforts of genius, when applied to Celestial Mechanics, conducted the geometer to new solutions of all the great problems of the system of the world, more comprehensive in design, and more fertile in detail, than those derived from the synthetic methods of the *Principia.* Formulæ were assigned exhibiting the oscillations of the planetary movements throughout indefinite ages both past and future. The sublime truths relating to the stability of the planetary system were established by Lagrange and Laplace upon the basis of a rigorous analysis. The accurate computation of the motion of the lunar apogee, the equally satisfactory account of the hitherto inexplicable irregularities of Jupiter and Saturn, and of the secular acceleration of the moon's mean motion, the solution of the great problem of the precession of the equinoxes by the strictest principles of mechanical science, and the complete establishment of the theory of Jupiter's satellites, may be ranked among the most brilliant conquests achieved during this period. The actual state of astronomical science was also now brought into more intimate connexion with the researches of the geometer than it had been in the preceding period. The phenomena of the celestial motions discovered by Hipparchus and his successors down to Kepler inclusive, were too broadly traced out in the heavens to excite any doubts respecting their real character. It only remained, therefore, to explain their dependence upon physical principles, a task which devolved exclusively on the geometer. But when the latter proceeded to consider the more recondite parts of the theory of gravitation, it became indispensable, in order to obtain a confirmation of his results by an appeal to observation, that the corresponding phenomena of the heavens should be exhibited in a clear form by the astronomer. The objects of research, being now, however, of a more minute character than those hitherto accounted for by theory, were appreciable to the astronomer only by a closer scrutiny of facts and greater delicacy of observation. It was thus that astronomy and abstract science reflected a light upon each other's paths, which conduced materially to their mutual progress. The results achieved by the critical acumen of Halley, the sagacity of Bradley, and the unrivalled practical skill of Maskelyne, advanced at an equal pace with the analytical researches of the geometers on the Continent, and are imperishably associated with them in the magnificent triumphs which illustrate the history of physical astronomy during this period. The latter aptly closes with the publication of the *Mécanique Celeste*, a work embodying many of the most important methods of Euler, Clairaut, D'Alembert, and Lagrange; but in respect to which it has been beautifully remarked by an eminent philosopher* that the immortal author might be amply justified in using the words of the prince of antiquity, "*et quorum pars magna fui.*"

The third period, although distinguished less by intellectual triumphs than by the profusion of its accumulated treasures, notwithstanding exhibits some results which may vie in splendour with the most brilliant efforts of genius in any age. The sublime theory of the variation of arbitrary constants was carried by Lagrange to a state of perfection which will command the admiration of geometers to the latest posterity, and was subsequently applied with success to all the great problems of the system of the world. The vast treasures of analysis which had been amassed by the geometers of the eighteenth century supplied inexhaustible stores for the improvement and extension of the several parts of the theory of gravitation; and, as the objects of research were now of a still more evanescent character than they had been in the preceding period, the labours of the astronomer were characterized by a suitable degree of refinement. The annals of science do not contain the record of more delicate operations than those which have been conducted in the present age for the purpose of determining the ellipticity of the earth by means of the pendulum. The elements of the planetary orbits have also been determined with unexampled precision, a circumstance which has been attended with the twofold advantage of improving the tables of astronomy, and exhibiting, in bold relief, the existence of outstanding irregularities. By these means the long inequalities in the earth and moon, besides various phenomena of minor interest, have been detected and accounted for by the theory of gravitation. A similar process led to a clear definition of the anomalies of Uranus, and suggested those immortal investigations which established the existence of an exterior planet.

The future prospects of physical astronomy are in accordance with its past triumphs. The theories of the smaller planets and comets, and the inverse problem of planetary perturbation, still continue to offer to the geometer extensive subjects of interesting research. The theories of the secondary systems are also still in an imperfect condition. It is true that the moon and the system of Jupiter's satellites have formed the subjects of elaborate research, and that their complex perturbations have been studied with a degree of success which leaves little further to be desired; but so intricate are the various parts of physical astronomy, and so difficult is it for the geometer to bring them within the reach of his analysis, that methods of investigation devised for any particular problem become totally useless when applied to others apparently similar to it. In the secondary systems of Saturn, Uranus, and Neptune, difficulties will doubtless occur to the geometer, which can only be vanquished by methods of analysis peculiarly adapted to each specific case. But the planetary system does not hold out the exclusive prospect of future advancement in the study of Celestial Mechanics. Already the sublime truth announced by Newton, that every particle of matter in the universe attracts every other particle with a force varying reciprocally as the squares of their mutual distances, has been realised in the motions of those vast bodies which roll in space at an inconceivable distance beyond the limits of the solar system. The recent researches of astronomers on the motions of Double Stars have established this important fact beyond all doubt. An unlimited field of speculation is here presented to the contemplative mind. Whether it is allotted for the geometer to transport his analysis effectually to those myriads of bodies which twinkle in the starry heavens, and to calculate the perturbations which the solar system may one day experience as, in the course of its motion through space, it approaches some of the great masses of the universe, are questions which cannot fail to occur to the thoughtful inquirer, but are of the class which time alone can solve. To use the language of an eminent philosopher it would be rash to be too sanguine; it would be unphilosophical to despair.

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* M. Biot.

Still more shrouded in mystery is the question relative to the nature and mode of operation of the principle which is thus found to exercise an incessant influence over the constituent particles of matter. That it has a close affinity to heat, light, electricity, and the other imponderable agents of the material creation,—nay, that all may be only so many distinct manifestations of some more general principle, is the prevailing opinion of those who have devoted much attention to physical researches. This opinion may one day ripen into an established truth, and views of nature unexampled in magnificence and splendour may be reserved for future generations. In the present state of our knowledge, however, all attempts at generalizations of this kind must be regarded as premature. A multitude of difficulties occur in every subject of physical enquiry, the explanation of which must precede any extensive induction of general principles such as that referred to, but which can only be expected to result from scientific researches prosecuted throughout a long succession of ages. Whether gravitation is a quality inherent in, and necessarily co-existent with, matter, or whether it is a principle essentially distinct from it and operating merely on its constituent parts, is a question which, in all probability, is destined for ever to prove irresolvable to the most penetrating inquiries of the human mind. It is when he thus passes the boundary that circumscribes the province really accessible to his researches, and seeks with prying interest to penetrate into the illimitable region of the unknown, that man with all his boasted philosophy is reminded of his nothingness. He has decomposed the subtle light into its primitive elements, and determined with mathematical rigour the amazing velocity of its transmission through space; he has measured the distances of the celestial bodies, and traced the laws of their complicated movements—but the fall of a decayed leaf suggests to him problems, whose solution transcends the loftiest powers of his understanding, and in the physiology of the humblest moss that presents itself to his contemplation he encounters mysteries that prove impenetrable to his most searching scrutinies. To examine, arrange, and classify the countless varieties of physical phenomena—to define the character, and unfold the admirable beauty, of the principles that unite them together, and to advance by successive inductions to relations of a more and more extensive order in the economy of the material universe—such are the magnificent enterprises which science proposes to the well-directed efforts of persevering thought—such are those alone whose realization is attainable by human research. Ennobled with elevating conceptions of creative grandeur, and fraught with sentiments of pure enjoyment, is the mind of the philosopher, who, while prosecuting the study of nature in the true spirit of rational inquiry, regards his vocation, not as a pursuit designed to gratify his curiosity or minister to his ambition, but as a glorious privilege offered to him, by rightly availing himself of which he is enabled to advance the condition of his fellow creatures, and to discover innumerable illustrations of the power, wisdom, and goodness of the Supreme Being.

CHAPTER XIV.

Researches on the Solar Parallax.—Modern Determinations of this Element.—Discovery of the Solar Spots.—Consequences deduced from this Discovery.—Period of the Sun's Rotation.—Theories of the Solar Spots.—Wilson.—Herschel.—Researches on the Lunar Parallax.—Ellipticity of Mercury.—Researches on the Rotation of Venus.—Discovery of the Ultra Zodiacal Planets.—Micrometrical measures of Jupiter's Satellites.—Micrometrical measures of Saturn, and of his Ring.—Discovery of the eighth Satellite of Saturn.—Researches on the Satellites of Uranus.—Lassel's Discovery of the Satellite of Neptune.—Researches on Comets.—Halley's Comet.—Comet of 1843.

THE determination of the distance from the sun to any of the planets revolving round him, is one of the most important problems of astronomical science. When this object is effected in any individual instance, it is then possible by means of Kepler's third law to ascertain the distances of all the planets from the sun, and hence, also, their distances from the earth corresponding to any assigned instant. An accurate knowledge of the latter is indispensable in reducing the apparent positions of the planets to the true positions which they would occupy if seen from the centre of the earth. These results may obviously be all derived from the solar parallax, which is expressed by the reciprocal of the sun's distance from the earth. The real value of this element has, in all ages, formed an interesting subject of enquiry. Aristarchus of Samos, by observing the angular distance between the sun and moon, when the latter was dichotomized, inferred that the sun is nineteen times more distant from the earth than the moon is. Ptolemy assumed this result to be true, and, combining it with the value of the lunar parallax as determined by his own observations (or rather perhaps those of Hipparchus), he obtained 3′ for the amount of the solar parallax. This value was adopted by all his successors down to Tycho Brahé inclusive. Kepler, while engaged in his celebrated researches on the motions of Mars, availed himself of the accurate observations of Tycho Brahé to institute a searching scrutiny into the value of the solar parallax. The conclusion he came to was, that it did not exceed 1′, and in all probability fell very short of that quantity. The researches of Cassini reduced the superior limit considerably below this value. He attacked the problem not by direct investigation, but by means of researches on the parallax of Mars. When this planet is in opposition, it is much nearer to the earth than the sun is, and consequently its parallax is then a much more appreciable quantity than that of the sun. But if the parallax of any planet is once determined, the parallax of the sun or that of any other planet, becomes known by means of Kepler's third law. The method proposed by Cassini for determining the parallax of Mars, was to make simultaneous observations of the planet when in opposition at two places of the earth considerably distant from each other, and then, by a comparison of the results, to ascertain the amount of displacement arising from the difference of position. For this purpose, Richer was sent by the Academy of Sciences to Cayenne, in Africa, while Cassini, Roemer, and Picard observed the planet at different places in France. A comparison of the positions of the planet as determined by these observations shewed that the effect of parallax was insensible; whence it was inferred that at the utmost it did not exceed 25″. This gave 10″ as the greatest value which could be assigned to the solar parallax. Cassini fixed it at 9″.5, a result which formed a very important approximation to the true value. When Lacaille was at the Cape of Good Hope, he made observations of Mars for a similar purpose. A comparison of his results with those assigned by similar observations in Europe gave 10″. for the amount of the sun's parallax.

The transits of Venus across the sun's disk in the years 1761 and 1769 supplied astronomers with a more accurate method of determining the solar parallax than that to which we have been just alluding. This method was first pointed out by Halley, who, in 1716, suggested to astronomers to carry it into effect on the occasion of the approaching transit of the planet, earnestly imploring them not to neglect so valuable an opportunity of determining so important an element. Astronomers did not fail to appreciate the value of Halley's proposal; and on their recommendation the principal Governments of Europe fitted out expeditions to various parts of the world, both in 1761 and 1769, for the express purpose of observing the transit of the planet. The transit of 1761 was imperfectly observed, and the result did not prove satisfactory. The transit of 1769, however, was observed with complete success. A comparison of the results assigned by the various observations gave 8″.7 and 8″.5 as the most probable limits within which the parallax is confined. Delambre assigns 8″.6 as the most accurate value, a result which places the sun at the distance of 95 millions of miles from the earth. When we consider the ingenuity of the method employed in arriving at this determination, and the refined nature of the process by which it is carried into effect, we cannot refrain from acknowledging it to be one of the noblest triumphs which the human mind has ever achieved in the study of physical science.

A comparison of the value of the solar parallax as assigned by observations of the planet Mars, with that obtained by the more accurate method of the transit of Venus, did not lead to so close an agreement as might have been expected from the advanced state of astronomy. With the view of arriving at a more satisfactory result by the former of these methods, the late Mr. Henderson made observations of Mars at the Cape of Good Hope, on the occasion of the opposition of the planet in 1832. Simultaneous observations of the planet were made at Greenwich, Cambridge, and Altona. A discussion of all the observations gave for the solar parallax a mean value equal to 9″.125. This result forms a nearer approximation to the generally acknowledged value, than either of those assigned by Cassini or Lacaille, but still it has not altogether satisfied the desire of further research on the subject. A method for determining the solar parallax by means of observations of Venus and Mars has recently been proposed by Dr. Gerling of Marburg. A suggestion which was made to the Government of the United States to lend its co-operation in carrying this method into effect, has been favourably received; and, in pursuance of this object, an expedition has been despatched to the State of Chili, in South America, in the month of July of the present year*. It is contemplated to observe Mars at the oppositions of 1849–52; and Venus at the inferior conjunctions and stationary points of 1850–52. The conduct of the observations has been assigned to Lieutenant Gilliss, a young astronomer who has already signalized himself in a most praiseworthy manner. It is to be hoped that the result of this expedition will prove to be as conducive to the advancement of astronomical science as the expedition itself is creditable to the Government under whose auspices it has been undertaken.

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* 1849.

When Galileo first directed his telescope to the sun, he was surprised to find that the surface of that luminary, instead of presenting a uniform appearance, according to the opinion universally entertained by philosophers, was diversified with a number of dark spots exceedingly irregular both in form and magnitude. When watched attentively for some days these phenomena were perceived to be in a state of constant change. In some instances two or three spots would unite together and form one large spot; and on the other hand it happened not unfrequently that a spot of considerable magnitude would break up into two or three smaller ones. In the latter case the spots generally diminished with more or less rapidity, and finally disappeared altogether, when they were succeeded by others of the same irregular and fleeting nature. This remarkable discovery inflicted one of the most effective blows which had yet been dealt against the ancient philosophy by the progress of scientific research; for it was one of the fundamental doctrines of Aristotle that the heavens are incorruptible and immutable; and, therefore, that the surfaces of the celestial bodies are destitute of any physical changes analogous to those which characterize the operations of nature on the surface of the earth. The announcement of the existence of spots on the sun was therefore received by the adherents of that philosophy with feelings of deep mortification, and various attempts were made to demonstrate its fallacy. By some the spots were alleged to be spurious phenomena arising from the impurities in the glasses with which the observations were made. By others they were maintained to be planets revolving round the sun at small distances from his surface, for when watched some days they were all found to have a motion on his disk from east to west. Observations made with different glasses sufficed to disprove the former of these assertions. With respect to the latter, a fatal objection to it consisted in the irregular aspect and changeful character of the spots.

Although the name of Galileo is more intimately associated with the discovery of the solar spots than that of any other philosopher, it would appear that one of his contemporaries at least arrived also at a knowledge of these interesting phenomena by original observation. Nay, it has even been alleged that the illustrious Italian was anticipated on this occasion by an astronomer of Germany. It is incontestable that the earliest publication which contains an account of the solar spots is due to John Fabricius, a nephew of David Fabricius, the astronomer and intimate friend of Kepler. The dedication of this work* is dated June 13, 1611. The author asserts that the solar spots were observed by him from the commencement of the current year. The work contains internal evidence that some of the observations of these phenomena must have been made at least three months anterior to the date of dedication. Another of Galileo's contemporaries who asserted that he had discovered the spots by his own observations was Christopher Scheiner, a German Jesuit, who was Professor of Mathematics in the University of Ingolstadt. This individual published an account of his observations in three letters addressed to Wilser, the chief magistrate of Augsburg, under the anonymous signature of *Apelles latens post tabulam.* The first of these letters is dated the 12th November, 1611. The author states that he first discovered the spots seven months previously. This assertion would carry back his observations to the month of April, 1611. He mentions that, upon resuming his observations in the month of October, he suspected that the appearances arose from some defect in the glasses with which he viewed the sun, but that after he had prosecuted his observations for a short time he finally became convinced of the actual existence of the spots*. With respect to his alleged discovery of the spots as early as April, 1611, it is clearly inadmissible, inasmuch as it rests solely on his own assertion. Nor, indeed, even if supported by sufficient evidence, do his observations of that period appear to possess any real merit, since we find him in October still doubting the actual existence of the spots. Galileo first alluded to his discovery of the solar spots in the commencement of his "Dissertation on Floating Bodies," published at Florence in 1612†. A detailed account of his researches on these phenomena is contained in three letters addressed to Welser in reply to the letters addressed to the same individual by Scheiner, whose views of the nature of the solar spots were at variance with those of the Italian philosopher. These letters were published at Rome in January, 1613, under the auspices of the celebrated Lyncean Society‡. The first letter is dated the 4th May, 1612. The author asserts in it that eighteen months had elapsed since he originally observed the spots. This statement carries back his discovery to about the beginning of November, 1610. As, however, he has not cited any observations of so early a date, nor mentioned any individuals to whom he communicated his discovery, it is impossible to admit his assertion as an historical fact§. Evidence of a more reliable character goes to shew that he observed the spots as early as the month of April, 1611. On some day in that month he announced the existence of the solar spots at a conference of savans, held in the garden of Cardinal Bandini at Rome. This fact was afterwards adduced in support of his claims to the original discovery of the phenomena, and was corroborated by several persons who were present at the conference. A fourth individual to whom the discovery of the solar spots has been attributed is Hariot, the celebrated mathematician of England. De Zach, who visited this country towards the close of the last century, obtained access to the manuscripts of Hariot, and from an inspection of them he came to the conclusion that the author had observed the solar spots as early as the month of December, 1610. The late Professor Rigaud, of Oxford, however, having carefully examined the same manuscripts, found that the observations contained in them could not bear the interpretation put upon them by De Zach. It turns out, in fact, from his researches, that Hariot did not commence his observations of the spots before the month of December, 1611. With respect to the other three individuals whose names are associated with the discovery of the phenomena, it is pretty evident that the claims of two at least are well founded. Fabricius and Galileo appear to have both perceived the spots about the same time. The observations of Scheiner were of a later date, but it is not impossible that they may have been quite independent of those of his contemporaries.

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* Johannis Fabricii Phrysii de Maculis in Sole Observatis, &c. Witteburgi, 4to, 1611.

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* An amusing incident is related in connexion with Scheiner's observations, which indicates the fatal effect with which the authority of Aristotle succeeded in maintaining its ascendancy over men's minds, even when the dogmas of the illustrious Stagyrite were opposed to the testimony of the senses. As soon as Scheiner had assured himself of the actual existence of the solar spots, he communicated his discovery to the Provincial of the Order of Jesuits, but the latter, who was a zealous Peripatician, positively refused to give credit to his assertion. "I have read Aristotle's writings from end to end many times," says he to Scheiner, "and I can assure you that I have nowhere found in them anything similar to what you mention. Go, my son, and tranquillize yourself; be assured that what you take for spots in the sun are the faults of your glasses or your eyes." The inflexible provincial would not allow him to publish his observations and opinions under his own name. He only consented to an anonymous publication of them, as mentioned in the text.

† Discorso intorno alle Cose che stanno in su l'Acqua. Firenze, 1612.

‡ Istoria et Dimostrazioni intorno alle Macchie Solari et loro accidenti dal Signor Galileo Galilei, &c. Roma, 13 gennaro, 1613.

§ M. Arago, in his valuable "Analysis of the Life and Discoveries of Sir William Herschel," published in the *Annuaire* for 1842, has discussed with great ability the claims of the various individuals to whom the discovery of the solar spots has been attributed. He rightly refuses to Galileo the credit of having discovered the spots eighteen months previous to his first letter to Welser, on the ground that the claim to the discovery rests on the bare assertion of its author. He, however, readily admits that if Galileo's letters contained the records of any observations made in the preceding year, such records should be considered as fully substantiating his claim to the discovery. The following are the words of M. Arago in reference to this point:—"Le meilleur moyen de trancher toute difficulté sur la date de la découverte des taches eût été de rapporter de veritables obser-

The discovery of the solar spots soon conducted astronomers to the important conclusion that the sun has a rotatory motion round a fixed axis. Fabricius found that all the spots had a common motion on the sun's disk from east to west. Their motion was greatest when they were in the centre of the disk, and it thence gradually diminished until they reached the western limb, when they disappeared from observation. In the course of ten days afterwards the spots reappeared on the eastern limb. Their motion at first was slow, but it continually increased until they reached the centre of the disk, when it again attained its maximum rate. He also found that the magnitude of the spots appeared to be greatest when they were on the centre of the disk, and least when they were near either of the limbs. He remarked, that according to the principles of perspective, all these appearances would ensue on the supposition that the spots were attached to the surface of the sun, the latter being supposed at the same time to revolve round an axis with a uniform motion from east to west. He does not appear, however, to have formed an adequate conception of the importance of this conclusion, for he did not pursue any further researches in connexion with it.

Galileo reasoned on the subject with his usual sagacity. The various circumstances connected with the phenomena of the spots soon revealed

vations. Qui aurait osé concevoir des doutes sur la sincerité d'une declaration de Galilée conçue en ces terms: Tel jour, en 1611, je vis une tache près du bord oriental du Soleil; tel autre jour elle etait au centre du disque; à telle troisième date je fus témoin de la disparition de la tache derrière le bord occidental? On trouve des observations de ce genre dans les lettres que l'illustre physicien écrivit à Welser d'Ausbourg, mais elles sont toutes des mois d'Avril et de Mai, 1612." (*Annuaire*, 1842, p. 469.) From the above passage we infer that if M. Arago had been acquainted with Mr. Airy's "Historical Statement of Circumstances connected with the Discovery of the Planet exterior to Uranus" at the time when he was engaged in drawing up the critical remarks on that subject, which appeared in the *Comptes Rendus* for the 19th October, 1846, he would have conceded *substantially* to Mr. Adams his right to the discovery of the Trans-Uranian planet, as unhesitatingly as in the present instance he concedes *hypothetically* to Galileo his right to to his acute perception the fact of the sun's motion round a fixed axis. In the remarks on the spots, which appear in the beginning of his "Dissertation on Floating Bodies," he makes the period of rotation to be about a lunar month *. In his third letter to Welser his language is more precise. He states that, having observed a great number of spots with much attention, he came to the conclusion, that the time during which they remained on the disk was somewhere about fourteen days †. This assigns rather more than twenty-eight days to the period of the sun's *apparent* rotation. Modern observation makes it $27^{d}\ 8^{h}$. He also remarked that the axis of rotation is not perpendicular to the plane of the ecliptic, but that it deviates from the pole of that circle only by a small angle. Although Scheiner formed very erroneous views of the nature of the solar spots, he studied with great assiduity all the circumstances connected with their appearance. In 1630 he published his *Rosa Ursina* ‡, an immense work, devoted exclusively to the subject of the solar spots, and comprehending observations of these phenomena, which extended over a period of eighteen years. He determined the period of the sun's rotation to be between 26 and 27 days. He also assigned 6° and 8° as the two extreme limits of the angle at which the pole of rotation is inclined to the pole of the ecliptic. Modern observation makes it 7° 20′.

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* Che (il sole) in un mese lunare in circa finisce il suo periodo. Opere di Galileo, Edit. Pad. tome i. p. 189.

† M. Arago, while engaged in discussing the relative merits of Galileo and Scheiner, with reference to their respective researches on the subject of the sun's rotation (*Annuaire*, 1842), appears to have been hurried, by the ardour of dispute, into the commission of an act of injustice against the illustrious Italian. The following is a brief statement of the facts connected with this question. Scheiner, in one of his letters, had contended that the spots could not be attached to the surface of the sun, alleging in support of his assertion, that the time occupied by a spot in traversing the sun's disk was greater in some cases than it was in others. Galileo denied the existence of any difference in the duration of the spots on the disk, and proceeds in the following terms to state the grounds of his disbelief:—"Perche havendo io circa questo particolare fatte molte, et molte diligentissime osservazioni non ho trovato incontro alcuno, onde si possa concluder altro, se non che le macchie tutte indifferamente dimorano sotto 'l solar disco tempi eguali che al mio giudizio sono qualche cosa più di giorni 14." (Istoria, &c., alle Macchie Solari, p. 116.) It appears by this passage, and also by that cited in the foregoing note, that as early as 1612 Galileo had watched with great assiduity the various circumstances connected with the solar spots, and that *as nearly as he could possibly judge* the period of the sun's rotation was 28 days. In 1630, Scheiner published his *Rosa Ursina*, in which he fixed the period of rotation between 26 and 27 days. Let the reader compare these facts with the following assertion of M. Arago's. "Galileo has never assigned the period of the sun's rotation, whether apparent or real, otherwise than in a vague manner. With respect to the apparent period he fixed it at about a month (nello spazio quasi d'un mese, Dialogues)....Besides, the Dialogues did not appear until 1632, two years after the publication of the *Rosa Ursina* of Scheiner." Thus it appears that M. Arago entirely overlooks the scientific statement made by Galileo in his letter to Welser, of December, 1612, and assumes, as the basis of his reasoning, a remark on the same subject made by the illustrious philosopher in his "Dialogues on the System," a work in which the author avowedly expounds his views rather in the language of familiar explanation than in the strict phraseology of science. Galileo's estimate of the period of rotation is also presented in a very unfavourable point of view by placing it *after* the publication of Scheiner's work, and twenty years subsequent to the stricter statement made by him in his letter to Welser. It may be urged that the language of the Italian philosopher is by no means consistent with the precision due to such researches. This is, no doubt, true, but it certainly does not lose, in this respect, by comparison with the language of Scheiner. Indeed, when we take into account the discordances of the modern determinations of the period of rotation (amounting to seven or eight hours), we cannot refrain from the conclusion that too much reserve cannot be employed in advancing the charge of vagueness against the original explorer of the heavens with the telescope, whose observations were made with a little instrument that magnified only thirty-three times.

‡ Rosa Ursina, sive Sol ex admirando facularum et macularum suarum phenomeno varius, &c. Alluding to this enormous work, Delambre says, "There are few books so diffuse and so void of facts. It contains 784 pages: there is not matter in it for 50 pages."—*Hist. Ast. Mod.* tome i. p. 690.

The subject of the solar spots is calculated, in a strong degree, to attract the attention of those engaged in the study of physical science. When it is considered that the sun exercises so commanding an influence over the operations of nature on the earth, it is impossible to repress an intense desire of arriving at some knowledge of those mysterious changes which are perpetually taking place at his surface. Notwithstanding its manifest importance, however, there is, perhaps, no department of astronomical science in which less real progress has been made than in that relating to the solar spots. In order that the reader may form a clearer conception of the various theories which have been devised with a view to explain the origin of these interesting phenomena, we shall give a brief statement of the principal facts relating to them which have been established by the observations of astronomers.

When a spot on the sun's disk is closely examined, it is found not to be uniformly obscure. The central part is characterised by intense blackness, but it is surrounded on all sides by a contour of appreciable breadth, which exhibits a semi-luminous appearance. The more obscure part of the spot is termed the nucleus; while that which is visible by a faint light has been denominated the penumbra. The nucleus and the penumbra do not shade into each other, but are separated by a well-defined boundary. This important characteristic of the solar spots was first remarked by Scheiner, and has been fully established by the observations of subsequent astronomers. A similar remark holds good with respect to the exterior part of the penumbra; the bounding line between it and the wholly luminous region around it being in general distinctly visible. Sir William Herschel made a series of photometrical experiments with a view to determine the relative quantities of light emitted by the nucleus, the penumbra, and the wholly luminous part of the sun. The conclusion he came to was that, if the full light of the sun be represented by a thousand, the brightness of the penumbra will be represented by four hundred and sixty-nine, and that of the nucleus by seven.

The formation of a spot is generally indicated by the appearance of a very black pore, which gradually enlarges on all sides. This enlargement of the spot is effected by a simultaneous enlargement of the nucleus and penumbra. When two or more spots appear very close together, they frequently expand towards each other and form one large spot. When a spot is diminishing, previous to its disappearance, the process of diminution is accomplished by an irregular encroachment of the penumbra upon the nucleus. This circumstance causes the form of the nucleus to be very irregular, and not unfrequently leads to its breaking up into two or more distinct nuclei. When a spot is disappearing, the nucleus generally vanishes before the penumbra.

Sometimes a spot is observed in which the penumbra is wanting. Small spots are generally destitute of these appearances. On the other hand, a penumbra without a nucleus is occasionally seen. Scheiner, and the ancient observers of the spots, were of opinion that the exterior boundary of the penumbra never contained any sharp angles, however irregular the boundary on the side of the nucleus might be. The accurate observations of Sir William Herschel have shewn that this is not universally true. On the 18th February, 1801, that astronomer observed a spot on the sun's disk, from the nucleus of which there issued a branch, jutting out sharply upon the penumbra, while *on the same side of the spot* there appeared a similar projection of the penumbra upon the luminous region around it. In the course of a little more than two hours he found that the nucleus had thrown out three branches; and, in this case also, he found that there were three exactly corresponding branches of the penumbra. A conclusion manifestly deducible from these curious facts was, that the cause which acted upon the nucleus acted also in a similar manner upon the penumbra.

The magnitude of some of the solar spots is immense. In 1754, Mayer perceived a spot, the dimensions of which amounted to $\frac{1}{20}$th of the sun's apparent diameter. This gives above 45,000 miles for the absolute diameter of the spot. In 1779 there appeared an immense spot on the sun which Herschel was enabled to discern with the naked eye. When observed by him with a seven feet reflector, and a high magnifying power, it was found to be divided into two parts. The larger of the two measured 1′ 8″ in diameter, which indicates an absolute length of 31,000 miles. The rapid changes which these phenomena undergo is very astonishing. Herschel states that, while engaged in observing a spot on the 19th February, 1800, he fixed his attention on several places, but on looking off, *even for a moment*, the spots he had marked could not be found again. Sir John Lubbock, in a recent communication, states that he has observed spots visible to the naked eye, of which, on the following day, not a trace could be found, even with the aid of a good telescope *.

A remarkable circumstance connected with the solar spots is their constant appearance near the equator. Galileo remarked that their distance from that circle never exceeded 29°. Scheiner found by his own observations, that they were all confined to a zone extending 30° on each side of the equator, which was termed by him, on this account, the royal zone. Subsequent astronomers have enlarged the region of the spots, so as to embrace a zone of about 35° north and south of the equator. Occasionally, indeed, spots are observed in the regions exterior to this zone. A large spot, which appeared on the sun's disk in 1783, was found by Mechain to be distant about 41° 30′ from the solar equator. This is, however, an instance of very rare occurrence. Sir John Herschel carefully observed the spots on the sun's disk, towards the close of the year 1836 and the beginning of 1837; and he has remarked that, during the whole period embraced by his observations, the most unpractised eye could not fail to perceive by the mere allineation of the spots, the situation of the poles and equator of the sun, without watching from day to day their progress across the disk †.

Besides the phenomena of the spots, the telescope has disclosed other interesting appearances on the sun's disk. Some parts are perceived to be brighter than the rest of the surface, and hence have derived the appellation of faculæ. These phenomena were first noticed by Galileo in his third letter to Welser on the spots ‡. The illustrious philosopher, with admirable tact, cites the existence of these phenomena as an irrefragable proof that the spots are attached to the surface of the sun, instead of being planets, as Scheiner, in his "Letters to Welser," supposed them to be; for, on the latter supposition, he remarks, that the faculæ, in virtue of their motion from east to west, ought sometimes to appear as bright spots *beyond* the limb of the sun, a conclusion which was totally at variance with observation. The faculæ generally present an extended luminous appearance, but occasionally they exhibit a round form. On the 3rd December, 1800, Sir William Herschel observed a facula, which measured 2′ 45″.9 in apparent length, and therefore extended over a linear space of 75,000 miles. Faculæ are for the most part seen in the neighbourhood of spots, but sometimes they appear alone. In the latter case they are generally the precursors of spots which appear on the disk the following day. Messier was frequently enabled by this circumstance to predict the appearance of spots twenty-four hours before they actually presented themselves on the disk. The faculæ are always brightest on the sun's limb, and generally disappear as they approach the centre of the disk. When they enter the disk on the eastern limb they generally continue to be perceived for two or three days, after which they cease to be visible until they reappear on the opposite side of the disk, when they are again perceptible during two or three days before disappearing at the western limb.

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* Phil. Mag. vol. xxxii. p. 171.

† Results of Astronomical Observations at the Cape of Good Hope, &c., &c. London, 1847.

‡ Posso aggiungere che nella medisima faccia del sole si veggono tal volta alcune piazzette più chiare del resto. Istoria, &c., delle Macchie Solari, p. 132.

The faculæ always appear in the region of the spots. Observers, however, have discovered that the whole surface of the sun is diversified with minute luminous specks of different degrees of brightness and irregular streaks of light of extreme tenuity, bordered by more obscure parts. These phenomena are termed luculi. They are always perceptible on the sun's disk, and cause the whole surface to assume a mottled appearance. Sir William Herschel compares the corrugated state of the sun, arising from this cause, to the roughness of an orange. These phenomena are found to be in a state of constant change. The more obscure parts, when submitted to a close examination by Sir William Herschel, presented small pores as black as the nuclei of the spots.

We now proceed to notice the various explanations of the solar spots which have been advanced by different astronomers. Galileo supposed them to be clouds, of a greater or less degree of opaqueness, which are constantly floating in the solar atmosphere, and, by their occasional interposition, prevent the observer from viewing the luminous surface of the sun. This theory, however, is incompatible with certain appearances (to be noticed presently) which indicate that the spots are depressions in the luminous surface of the sun. It is also defective, inasmuch as it does not explain the constant presence and well-defined outline of the penumbra, nor give any account of the existence of faculæ. Scheiner, after he was convinced that the spots could not be planets, adopted the opinion that they were the indications of tumultuous movements occasionally agitating the ocean of liquid fire of which he supposed the sun to be composed. La Hire imagined the sun to be a fluid mass, which contained within it numerous opaque bodies. Occasionally the latter approached the surface and gave rise to the appearance of spots. When floating on the surface they attracted all the particles of a similar nature around them; and, consequently, when they disappeared the places that had been occupied by them were brighter than the other parts of the surface: hence originated the solar faculæ. This explanation agrees with a remark of Cassini's, to the effect that when a spot disappears it is generally succeeded by a facula

It gives no account, however, of the numerous faculæ which frequently are seen in the vicinity of a spot previous to its disappearance. It is also manifestly defective in many other particulars. Derham was of opinion that the spots are volcanos in the sun, that the nucleus represents the smoke, and that the faculæ, which subsequently appear, are the glowing flames of the eruption. A fatal objection to this hypothesis is, that the appearance of the solar faculæ, for the most part, *precedes* the appearance of the spots.

The theory of the solar spots, propounded by Dr. Wilson of Glasgow, is far more worthy of consideration than any that we have hitherto noticed. This ingenious astronomer maintained that the sun is an opaque mass surrounded by a luminous atmosphere, and that the spots are excavations in the luminous matter, by means of which the observer is enabled to see the dark body of the sun. The reasoning by which he established the important fact that the spots are depressions below the luminous surface of the sun is of a purely inductive character, and is founded upon observations of the great solar spot which appeared in the year 1769. The account he gives of these observations is exceedingly interesting*. He first perceived the spot on the 22nd November. It appeared below the equatorial diameter, and was not far from the western limb. On the 23rd he observed it again, and found that a remarkable change had taken place. The penumbra, which on the previous day was equally broad on all sides of the nucleus, was now very much contracted *on the side which lay towards the centre of the disk*, while the other parts retained nearly their former dimensions. On the 24th he again observed the spot. The distance from the limb was now only 24″, and the contracted side of the penumbra had entirely vanished. The breadth of the nucleus on the same side, also appeared to be more suddenly impaired than it ought to have been by the motion of the sun across the disk. Dr. Wilson demonstrated, by strict geometrical reasoning, that these are the appearances which would necessarily ensue on the supposition that the spot was a vast excavation, of which the nucleus was the bottom, and the penumbra the sloping sides. If it really was an excavation, a similar succession of changes in a reverse order should take place when the spot reappeared on the eastern limb. This was, in fact, what occurred to the observation of Dr. Wilson. On the 11th December the spot appeared on the opposite side of the disk. It was then distant about 1′ 30″ from the eastern limb. The side of the penumbra, next to the limb which formerly vanished, was now wholly visible, while that turned towards the centre of the disk appeared to be wanting. On the 12th December it came into view, and he saw it distinctly, although narrower than the other side. He did not see the spot again until the 17th December, when it had passed the centre of the disk, and the penumbra now appeared to surround the nucleus equally on all sides.

The interesting facts first announced by Dr. Wilson on this occasion, have been fully established by the observations of Sir William Herschel and all subsequent astronomers who have directed their attention to the phenomena of the solar spots. The conclusion to which they unavoidably lead cannot be any other than that which suggested itself to Dr. Wilson, namely, that the spots are not on the same level with the rest of the solar surface, but are depressions below it, formed by the partial removal of the luminous matter which envelopes the dark body of the sun. This was an important step in the progress of researches on the solar spots, and it recommended itself more especially to the attention of astronomers. inasmuch as it was not due to any arbitrary assumptions, but was established by a rigorous process of inductive reasoning. With respect to the physical cause which determines the generation of the spots, and the various phenomena connected with them, Wilson candidly acknowledged his inability to advance an adequate explanation ; but he propounded his views on the subject in the form of queries, some of which are not unworthy of attention. In order to account for the formation of the spots, he suggests whether an elastic vapour might not be generated in the interior of the sun, which, by escaping at the surface, would diffuse itself in all directions, and, forcing a passage through the luminous envelope, would expose to the eye of an observer the dark body of the sun. This hypothesis, he remarked, would satisfactorily explain the circumstance of the boundary between the nucleus and the penumbra being always distinctly visible and well defined. When the action of the vapour diminished in intensity, the luminous matter, in obedience to the solar gravitation, would flow into the excavation, causing it to contract equally on all sides, and thereby giving rise to the appearance presented by the diminution of a spot. In order to account for the gradual process by which the changes of the spots appeared to be effected, Dr. Wilson supposed the luminous matter of the sun to possess rather the consistency of a thick fog than the mobility of a fluid or gaseous envelope. The penumbra indicated the sloping sides of the excavation, which were supposed to be less bright than the rest of the surface, on account of a diminution of lustre experienced by the luminous matter when it diffused itself over them. With respect to the faculæ, Dr. Wilson imagined them to be, in all probability, merely phenomena of light and shade, arising from tumultuous movements in the luminous matter, which were occasioned by the intense action of the excavations. He adduced in support of this explanation, the remarkable fact, that the faculæ appear always in the vicinity of spots, and are never observed in the polar regions of the sun.

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* Phil. Trans., 1774.

The foregoing speculations of Dr. Wilson, on the origin of the solar spots, are very ingenious ; but as they involve the assumption of a gaseous vapour, of whose existence we have no proof, they are inadmissible into the rank of physical truths deduced by legitimate reasoning from established facts, and, therefore, cannot be recognised as really forming a part of astronomical science. But apart from the objection that the principle to which the phenomena of the spots are ascribed is not a *vera causa*, the hypothesis of Wilson, when submitted to close examination, will be found to afford a very inadequate explanation of these phenomena. One important defect we shall mention, and it will be unnecessary to point out more ; for unless a theory be capable of representing all the details of a phenomenon, it cannot be considered as a true embodiment of natural facts. Although the boundary between the nucleus and the penumbra is sufficiently well accounted for, the case is very different with respect to the exterior boundary of the latter. It is impossible to conceive how a homogeneous vaporous substance, such as the luminous matter of the sun is assumed to be composed of, could experience so striking a change of lustre as observation indicates, by a mere change in the inclination of its surface to the visual ray. That the transition is abrupt and not gradual from the nucleus to the penumbra, and from the penumbra to the wholly luminous region around it, is a feature of the solar spots which has been remarked by all astronomers who have observed these phenomena, from the period of their discovery down to the present day. "This want of graduation," says Sir John Herschel, "this sharply-marked suddenness of transition, is altogether opposed to the conception of a susceptibility of indefinite and easy mixture, in the luminous, non-luminous, and semi-luminous constituents of the solar envelope. . . . . There is no gradual melting of the one shade into the other—spot into penumbra—penumbra into full light. The idea conveyed is more that of a successive withdrawal of veils, the partial removal of definite films, than the melting away of a mist, or the mutual dilution of gaseous media." *

In adverting to the views of Dr. Wilson on the solar spots, it would be unjust to attach to them a degree of importance beyond what they really possessed in the author's own estimation. No philosopher could appreciate more fully than he has done, the essential distinction that exists between conclusions drawn from an examination of the real phenomena of nature, and all mere speculations in physical science, however ingenious or probable. In a paper on the same subject, which was subsequently communicated by him to the Royal Society, and which was written principally with the view of combating certain objections urged by Lalande against his explanation of the spots being excavations in the luminous matter of the sun, we find incidentally thrown out the following series of conjectures respecting the origin of the spots:—"Whether their first production and subsequent numberless changes depend upon the eructation of elastic vapours from below, *or upon eddies or whirlpools commencing at the surface*, or upon the dissolving of the luminous matter in the solar atmosphere, as clouds are melted and again given out by our air; or, if the reader pleases, upon the annihilation and reproduction of parts of this resplendent covering, is left for theory to guess at." † Some of these surmises, when attentively considered, will be found to be by no means destitute of probability.

Allusion has been made to an objection urged by Lalande against Wilson's explanation of the solar spots. The French astronomer, conceiving that the supposition of their being excavations, was incompatible with certain observations of Cassini and La Hire, came to the conclusion that they were rather protuberances existing on the solar surface ‡. In order to account for their formation, he supposed that the luminous fluid which covered the dark body of the sun occasionally subsided, laying bare the projecting eminences. The nuclei of the spots represented the latter, while the penumbræ indicated the surrounding shallows. Without alluding to the numerous defects of this theory, a fatal objection to it is, that the penumbra shades insensibly into the luminous region around it. This conclusion, as we have repeatedly had occasion to remark, is totally at variance with observation.

In 1776 Bode published a theory of the solar spots. He supposed the sun to be surrounded by two atmospheres. One of these was opaque, the other, which was the exterior atmosphere, was luminous. The appearance of a spot was occasioned by a partial opening formed in the luminous atmosphere, which disclosed to the eye of an observer the dark substratum beneath. The luminous matter was wholly withdrawn only at the centre of the aperture, occasioning the appearance indicated by the nucleus of the spot. Towards the sides it merely diminished in thickness, and in consequence exhibited a fainter lustre, whence originated the penumbra. He supposed the faculæ to be vast undulations in the luminous atmosphere, in proof of which he remarked that they were most conspicuous towards the limbs of the sun, and ceased to be visible when they approached the centre of the disk. This explanation of the solar spots does not differ materially from that which had been already advanced by Wilson; nor does it remove any of the objections to which the latter is liable.

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* Results of Astronomical Observations at the Cape of Good Hope.

† Phil. Trans., 1783, p. 162.

‡ Mem. Acad. des Sciences, 1776.

The next astronomer whose researches on the solar spots demand a brief notice is Sir William Herschel. That illustrious individual devoted considerable attention to this subject, and has presented the result of his labours in two papers, which appear in the volumes of the Royal Society*. In these papers the author has given a detailed statement of a multitude of interesting facts derived from his observations of the solar spots, and has exhibited a complete view of these phenomena which is in the highest degree instructive. The conclusions to which he was conducted by his researches on this subject are entitled in some respects to be ranked with the many other great achievements of his genius. He demonstrated, by means of his admirable observations, that the spots are real depressions in the surface of the sun, and that the faculæ are elevations above the ordinary level. He supposed, also, in common with several astronomers who preceded him, that the sun is an opaque mass surrounded by a luminous atmosphere. He maintained, however, that this atmosphere cannot be either gaseous or fluid; for on either of these suppositions, the luminous matter would flow with great velocity into the depressions, and would occasion an almost instantaneous disappearance of the spots, instead of a gradual diminution as observation indicated. He remarked, also, that the existence of faculæ is incompatible with the mobility of a fluid or gaseous substance; for these luminous ridges sometimes continue to maintain themselves for several days at a considerable elevation above the ordinary level of the solar surface. In order to account for the various appearances of the spots, he supposed the sun to be surrounded by a transparent atmosphere, in which are suspended two distinct strata of clouds at different elevations. The upper stratum is composed of self luminous clouds, which constitute the source of the solar light. The lower stratum is composed of opaque clouds, which shine only by the reflexion of the luminous regions above them. They are denominated by Herschel the planetary clouds, on account of their supposed resemblance to the clouds of the terrestrial atmosphere. The solid nucleus of the sun is effectually protected from the intense light of the luminous regions by the interposition of the planetary clouds. When corresponding apertures are formed in both strata of clouds, the dark body of the sun is perceptible through them, and gives rise to the appearance of a spot. If the apertures be of equal magnitude, the substratum will present an appearance of uniform blackness, and will therefore indicate a nucleus without a penumbra. If the aperture in the upper stratum be wider than that in the lower, a portion of the latter will be seen surrounding the dark body of the sun; but as it shines only by reflexion, it will exhibit a fainter lustre than the luminous regions, and therefore the appearance in this case will be that of a nucleus surrounded by a penumbra. If there be no aperture in the stratum of the planetary clouds, the appearance will manifestly be that of a penumbra without a nucleus. In order to account for the for-

* Phil. Trans. 1795, 1801.

mation of the spots, Herschel supposed a highly elastic gas to be generated in the solid body of the sun. This gaseous substance ascends in virtue of its inferior specific gravity relative to the solar atmosphere, forcing a passage through both strata of clouds, and exposing to the eye of an observer the dark body of the sun. When it reaches the luminous regions, it enters into combination with other gases, whence result decompositions which give rise to faculæ, and cause the solar surface generally to assume a mottled appearance.

It is scarcely necessary to state that any theory which involves the agency of a principle, of whose existence we have no positive proof, cannot be recognised as forming a part of the general body of science. The hypothesis of an elastic gas to whose energy the formation of the solar spots is mainly ascribed in the foregoing explanation, may or may not be true; but in the present state of our knowledge it can only be regarded as an arbitrary assumption, which possesses no stronger claims to our acceptance than many other similar hypotheses that might be adduced.

It is necessary, therefore, to draw a broad line of distinction between the purely speculative views of Sir William Herschel on this subject, and the important conclusions to which he has been conducted by reasoning from established facts. Indeed, it is quite clear that the illustrious astronomer himself did not attach any value to his hypothesis of the generation of the spots, beyond what it possessed as a convenient medium of connecting together the various results at which he had arrived in the course of his more rigorous researches *. The latter were pursued in the true spirit of inductive investigation, and the results constitute a valuable contribution to our knowledge respecting the interesting subject to which they refer. The facts upon which he has established the existence of two distinct strata of clouds in the solar atmosphere were exhibited by him in a very clear light; and the reasoning by which he fortifies that conclusion appears to be quite unexceptionable. This step had the effect of conducting him to an explanation of the penumbra of the solar spots—phenomena which all preceding astronomers had signally failed to render a satisfactory account of.

The geometrical relation of the nuclei of the solar spots with respect to the luminous surface, was first pointed out by Wilson. For a similar explanation of the penumbræ, astronomy is indebted to Herschel. With respect to the faculæ, the just surmises of Wilson and Bode were fully established by Herschel, on facts derived from accurate observation. A consistent view of the solar spots was thus obtained by a strict process of inductive reasoning. It still remained to render a legitimate account of the origin of these phenomena, by referring them to the operation of some really existing physical cause. Some interesting speculations designed to accomplish this object appear in Sir John Herschel's recent work, containing the results of his observations at the Cape of Good Hope. That distinguished philosopher justly remarks that, whatever be the cause of the spots, it is very evident that these phenomena

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* In confirmation of this remark, the following passages may be cited from the papers of 1795 and 1801. "They (the luminous clouds of the sun) plainly exist, because we see them; the manner of their being generated may remain an hypothesis, and mine, till a better can be proposed, may stand good; but whether it does or not, the consequences I am going to draw from what has been said, will not be affected by it."—*Phil. Trans.*, 1795, p. 61. "I am, however, well prepared to distinguish between facts observed, and the consequences that in reasoning upon them we may draw from them; and it will be easy to separate them, if that should be hereafter required."—*Phil. Trans.*, 1801, p. 303.

have an intimate connexion with the rotation of the sun about his axis. The remarkable fact of their absence from the polar regions, and their confinement to two zones, extending to about 35° on each side of the equator, with an intermediate equatorial belt in which they are more rarely perceived, appears to him to afford an indubitable indication that they derive their existence from circulating movements in the atmospheric fluid which encompasses the sun, depending on the rotation of that body about its axis, by a relation similar to that which connects the system of terrestrial trade and anti-trade winds with the earth's rotation. Now if any cause can be assigned which is capable of creating a circulation of the atmospheric fluid between the equator and the poles, the effect of the sun's rotation will be to modify the currents so produced—as the trade winds and monsoons on the surface of the earth are modified—and to dispose all the meteorological phenomena which accompany them as their visible manifestations into two zones parallel to the equator, with a calm equatorial zone interposed. The question then arises, where are we to discover the exciting cause of such circulations in the fluids of the solar atmosphere? With reference to this point, Sir J. Herschel remarks that the trade winds are occasioned by the unequal effects of the solar rays in heating the equatorial and polar regions of the terrestrial atmosphere; but, as no such external cause exists in the present case, we are necessarily led to look for the source of circulation in the economy of the sun itself. He then proceeds to state that if there can be assigned any physical difference in the constitutions of the equatorial and polar regions of the sun, which tends to repress the escape of heat in the one of these regions, and to favour it in the other, the effect will be the same as if these regions were unequally heated and all the phenomena of trade winds *mutatis mutandis* must arise. Before proceeding to a further exposition of his views, he takes this occasion to remark that the existence of a solar atmosphere cannot admit of any doubt. He adduces in support of his assertion the deficiency of light exhibited by the border of the solar disk, whether when viewed through coloured glasses, or by projecting its image on white paper. But a phenomenon which appears to him to establish still more conclusively the existence of such an atmosphere, is that which was witnessed during the total eclipse of the sun which occurred on the 8th of July, 1842*. On that occasion there appeared three rose-coloured protuberances of immense size projecting from the dark limb of the moon, which could not have been any other than clouds floating in, and sustained by, a transparent atmosphere enveloping the sun. To what distance from the surface of that body this atmosphere may extend, it is difficult to ascertain with precision; but, from the deficiency of light exhibited by the exterior part of the disk being gradual, and also perceptible at some distance within the margin, he is induced to suppose that it must be considerable, not merely in absolute measure, but as *an aliquot part of the sun's radius*. Admitting, then, the existence of such an atmosphere, the laws of equilibrium will cause it to assume the figure of an oblate spheroid, and the gaseous envelope so formed being thicker round the equator than at the poles, a different obstacle will be opposed to the escape of heat from the equatorial and the polar regions of the sun. The manifest result will be, that an habitual difference of temperature will prevail in the two regions.

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* For interesting accounts of this eclipse, drawn up from personal observation, see

"The spots, in this view of the subject," says the illustrious astronomer referred to, "would come to be assimilated to those regions on the earth's surface in which, for the moment, hurricanes and tornadoes prevail—the upper stratum being temporarily carried downwards, displacing by its impetus the two strata of luminous matter beneath (which may be conceived as forming an habitually tranquil limit between the opposite upper and under currents), the upper, of course, to a greater extent than the lower, and thus wholly or partially denuding the opaque surface of the sun below. Such processes cannot be unaccompanied with vorticose motions, which, left to themselves, die away by degrees and dissipate, with this peculiarity, that their lower portions come to rest more speedily than their upper, by reason of the greater resistance below, as well as the remoteness from the point of action, which lies in a higher region, so that their centre (as seen in our water-spouts, which are nothing but small tornadoes) appears to retreat upwards. Now, this agrees perfectly well with what is observed during the obliteration of the solar spots, which appear as if filled in by the collapse of their sides, the penumbra closing in upon the spot and disappearing after it."*

The views contained in the foregoing passage are eminently worthy of attention. It is remarkable that no preceding astronomer attempted to establish a connexion between the origin of the solar spots and the rotation of the sun on his axis, although the intimate relation which appeared to subsist between the situation of the spots and the solar equator, pointed out very unequivocally the mutual dependence of the two phenomena. Dr. Wilson, in a paper already cited, alludes to the invariable appearance of the spots in the regions near the solar equator, as a remarkable fact of which he was unable to offer any explanation †. It is curious, however, to find among a series of conjectures thrown out by him respecting the origin of these phenomena, an idea analogous to that propounded in the above passage ‡. His allusion to their possible generation by means of whirlpools in the solar atmosphere, commencing from the surface, in common with his other conjectures on the subject, shews that he possessed no inconsiderable share of the imaginative faculty which is so necessary in all physical speculations; but, whatever degree of truth may eventually turn out to be involved in this surmise, he cannot be entitled to any credit on its account, since he was unable to discover any physical connexion between it and the subject of investigation. The explanation proposed by Sir John Herschel is valuable, inasmuch as it exhibits the solar spots in the character of dynamical consequences flowing from the operation of established principles in physics. It is one of those conceptions of genius which, being formed by a comprehensive and luminous view of the mutual relations of facts accurately observed, rather than suddenly suggested by the sallies of a lively but wayward imagination, is found not unfrequently to constitute the germ of a theory of indisputable rigour. The distinguishing character of all those speculations in science which have *facts* for their basis consists in their tendency to promote further research. By a comparison of their results with those deduced from actual observation, the fundamental principles are modified, and a further advance is made towards a true explanation of the phenomena which form the groundwork of inquiry. In the present instance, the question whether the spots recur periodically in the same precise localities of the solar regions, acquires additional interest from the hypothesis of their origin advanced in the foregoing passage, and suggests the expediency of a continued prosecution of attentive observations of the spots. By these means it is to be hoped that some light will be thrown upon the real nature of these mysterious but singularly interesting phenomena, which, in its turn, will lead to a more accurate knowledge of the physical constitution of the sun.

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* Results of Astronomical Observations at the Cape of Good Hope, p. 434.

† Phil. Trans., 1774.

‡ See p. 222.

We have mentioned that Sir William Herschel, in his theory of the solar spots, supposes the solid nucleus of the sun to be effectually screened from the luminous clouds of the superior regions by the interposition of the planetary clouds which possess the property of reflecting the rays that fall upon them. That the clouds of the lower regions shine only by reflection, he infers from their constantly exhibiting a faint and uniform colour; and that they form a *continuous* envelope round the nucleus of the sun, appears to him equally manifest from his being enabled, by means of his powerful telescopes, to discover them through the minute fissures by which every part of the luminous surface of the sun is intersected *. He considers, therefore, that, notwithstanding the circumstance of the luminous atmosphere of the sun being the source from which light and heat are dispensed in all directions, the solid nucleus of that body may be constantly maintained at a moderate temperature. Proceeding upon this view of the physical constitution of the sun, he comes to the conclusion that the central body of the planetary system is eminently fitted to be the abode of animated beings, and that in fact it offers the example of a magnificent self-luminous planet. This opinion has been ably controverted by one of the most illustrious philosophers of the present age.—"It is indeed inconceivable," says Sir David Brewster, "that luminous clouds, yielding to every impulse, and in a state of perpetual change, could be the depository of that devouring flame, and that insupportable blaze of light, which are emitted by the sun; and it is still more inconceivable that the feeble barrier of planetary clouds could shield the subjacent mass from the destructive elements that raged above." According to the opinion of the eminent authority just quoted, the heat and light of the sun derive their origin from two distinct sources. The luminous rays are emitted from the phosphorescent mantle which constitutes the exterior envelope of the sun, while the calorific rays proceed wholly from the opaque nucleus. Our limits will not allow us to notice the ingenious arguments which the author of this explanation adduces in its support †. Unfortunately our knowledge of the nature of both heat and light is so obscure, that every attempt hitherto made to form an hypothesis respecting the physical constitution of the sun, appears to be beset with insuperable difficulties.

It is remarkable that, at so recent a period as that of the discovery of the solar spots, astronomers were unacquainted with any method by which the sun might be viewed without injury to the eyes. Appian, an astronomer of the fifteenth century, had indeed pointed out the utility of coloured glasses for this purpose, but the hint appears to have been entirely lost sight of by his successors. Fabricius observed the solar spots either by introducing the sun's rays through a small aperture into a dark room, and projecting his image on a piece of white paper, or by viewing him directly through thin clouds and vapours when he was near the horizon. He recommends all those who purpose making direct observations of the spots to admit to the eye at first only a small portion of the

* Phil. Trans. 1802, p. 291.

solar light, and to increase the quantity gradually, until the visual organ becomes so habituated to the effulgence of the light as to be capable of viewing with impunity the whole of the solar disk. Galileo was equally ignorant of any method of observing the sun except that which consisted in taking advantage of his horizontal position when rising or setting. In a postscript added to his second letter to Welser, he alludes to an immense spot which appeared on the centre of the sun's disk, and which he, as well as many others to whom he shewed it, was enabled to perceive with the naked eye *at sunset*, on the 19th, 20th, and 21st of August, 1612*. Scheiner appears to have been the first observer who reduced into practice the method suggested by Appian. In a dissertation on the solar spots, which he published in 1612, he remarks that the sun may be viewed at any altitude by placing before the telescope a piece of glass, of a green or azure colour. He adds that this is the method practised by the Dutch mariners, when they wish to determine the altitude of the sun. A decided improvement of this method was first indicated by Tarde, a Frenchman, who published a work on the solar spots in 1620. He states that when he observed the spots he placed a thick piece of blue or green glass *between his eye and the eye-glass of the telescope.* By placing the coloured glass in this position, the indistinctness of the image arising from impurities in the structure of the glass, or an imperfect parallelism of its surfaces, was in a great degree removed.

The theory of the moon's motion is a subject of so much importance, that the attention of astronomers is constantly directed to its improvement. We have already alluded to a method assigned by the theory of gravitation for determining the distance of the moon from the earth. By means of the formulæ of Laplace, Burchardt (assuming the moon's mass to be $\frac{1}{75}$ of the earth's) found that the constant part of the lunar parallax under the equator amounted to 57′ 0″. Damoiseau determined the moon's mass to be $\frac{1}{74}$, the mass of the earth being represented by unity, and hence derived 57′ 0″·9 for the constant of parallax. Plana makes the mass $\frac{1}{87}$, and hence computes the parallax under the equator to be 57′ 3″·1.

A more obvious method of ascertaining the horizontal parallax of the moon is founded upon a comparison of her apparent positions as determined simultaneously at different places on the surface of the earth. One of the principal objects of Lacaille's voyage to the Cape of Good Hope was to make observations of the moon for this purpose. By comparing his results with simultaneous observations made at different places in Europe, and assuming the ellipticity of the earth to be $\frac{1}{200}$, he obtained 57′ 13″·1 for the constant part of the moon's equatorial parallax. This is equivalent to a parallax of 57′ 4″·6, if the ellipticity be supposed equal to $\frac{1}{300}$. Lalande, by comparing Lacaille's observations at the Cape of Good Hope with simultaneous observations made by himself at Berlin, fixed it at 57′ 3″·7. Bürg determined it to be 57′ 1″ by a similar comparison of Lacaille's observations with corresponding observations made at Greenwich. Henderson, during his residence at the Cape of Good Hope in the years 1832 and 1833, determined a great number of declinations of the moon, with the view of arriving at a more accurate value of this element. By a comparison of his own observations with others, made simultaneously at Greenwich and Cambridge, he obtained 57′ 1″·8 for the constant of the equatorial parallax†. This gives $\frac{1}{78 \cdot 9}$ for the value of the moon's mass. Mr. Airy, by a discussion of the totality of the Greenwich observations, instituted subsequently to their recent reduction upon a uniform plan, has determined the constant part of the equatorial parallax to be 57′ 4″·94. This value, considering the broad basis upon which it rests, is more entitled to confidence than any other which astronomers have hitherto arrived at.

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* Istoria et Dimostrazione intorno alle Macchie Solari, p. 56.

† Mem. Ast. Soc., vol. x., p. 294.

In consequence of her proximity to the earth, the physical constitution of the moon offers to astronomers a more favourable subject of research than that of any of the other celestial bodies. Galileo's discovery, that her surface is diversified with mountains and valleys, like the surface of the earth, was one of the many signal triumphs which that illustrious philosopher achieved in favour of the Copernican system of the world. He concluded, from his observations, that the mountains of the moon are much higher than those of the earth, when the relative magnitudes of the two bodies are taken into account. Many of them, according to his estimation, attain an elevation of four or five miles above the surrounding plains. Hevelius, Riccioli, and more recently Schroeter, were conducted by their respective observations to similar conclusions. Sir William Herschel did not discover any grounds for ascribing such an enormous height to the lunar mountains. He inferred, from his observations, that with a few exceptions they did not exceed half a mile in height. This conclusion is undoubtedly more consistent with analogy than that which assigns a higher elevation. It must be admitted, however, that the recent researches of MM. Beer and Mädler, on this subject, go to support the remarkable results of Schroeter and the more ancient observers. These astronomers have determined the heights of 1093 lunar mountains, principally by means of micrometrical measures of the lengths of their shadows, and have found several of them to attain an elevation of four miles above the neighbouring plains *.

The numerous spots which appear on the surface of the moon when viewed with the telescope, besides being calculated to throw light on the physical constitution of that body, are of some utility in observations of eclipses, and on this account several astronomers have devoted considerable attention to a careful delineation of their relative positions. Charts of the telescopic appearance of the moon were first executed by Scheiner, but Langrenus, Cosmographer of the King of Spain, was the first person whose labours on this subject deserve notice. He constructed, and even actually engraved, thirty maps of different portions of the lunar surface, founded on observations made by himself with a large telescope, at Madrid and Brussels. It was he who introduced the practice of naming the different spots after mathematicians, philosophers, and other eminent men. Hevelius about the same time devoted four years to assiduous observations of the lunar spots and delineations of their various configurations. He rejected the nomenclature of Langrenus, and named the spots after continents, seas, islands, and other objects on the earth's surface to which he conceived they bore a resemblance. Riccioli was the next astronomer who constructed charts of the lunar surface. He restored the principle of nomenclature adopted by Langrenus, but, instead of distinguishing the

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* The following are the elevations of a few of the principal lunar mountains, as determined by MM. Beer and Mädler:—Dörfel, 23,174 feet; Newton, 22,141 feet; Cassatus, 21102 feet; Curtius, 20,632 feet; Callippus, 18,946 feet; Tycho, 18,748 feet. A mile contains 5280 feet. The four highest of these mountains, therefore, attain seve-

spots by the names of great men indiscriminately, he employed for this purpose only the names of eminent astronomers of ancient and modern times. In 1692 Cassini executed a chart of the full moon. The positions of the spots were determined by means of his own observations of eclipses. Delineations of the moon's appearance have also been executed by Schroeter, Lohrmann, and other observers. The most recent work of this kind is the elaborate representation of the lunar surface by MM. Beer and Mädler.

The question whether the moon be surrounded by an atmosphere, has been much discussed by astronomers. Various phenomena are capable of indicating such an atmosphere, but, generally speaking, they are found to be unfavourable to its existence, or at all events they lead to the conclusion that it must be very inconsiderable. From the uniform appearance presented by the lunar spots, when viewed from time to time with the telescope, it is reasonably supposed that they are not liable to be occasionally obscured by the interposition of dense clouds between them and the earth, like the different portions of the earth's surface ; and on this ground it has been maintained that the moon cannot be encompassed by a gaseous fluid, analogous in its properties to that which encompasses the earth. Hevelius indeed, who devoted many years to assiduous observations of the moon's surface, has remarked that the spots sometimes appeared to be less bright and less regularly defined than they usually were, even when the air was so clear as to allow stars of the sixth and seventh magnitudes to be distinctly visible *. His assertion, however, has not been borne out by the more searching observations of succeeding astronomers, and therefore it is not entitled to any weight in discussing so delicate a point as the existence of a lunar atmosphere.

The occultations of stars by the moon have hitherto afforded only very slight indications of an elastic fluid enveloping the lunar surface. If such a fluid really exists, it manifestly follows that, when the moon approaches within a very small distance of a star previous to occulting it, the star ought to exhibit a sensible change of colour and diminution of lustre, in consequence of the rays proceeding from it to the eye of the observer being partially absorbed in the course of their passage through the lunar atmosphere. Observation is, generally speaking, at variance with this conclusion, the star being found, in most cases, to retain its ordinary colour and brightness until it actually arrives at the moon's limb. There is not wanting high authority, however, in support of an occasional obscuration of the light of the star. On the 27th March, 1811, M. Arago witnessed at the Royal Observatory of Paris, the occultation of a small star in the constellation Taurus. The star, as long as it continued to be visible, retained its usual colour, but it exhibited a very sensible diminution of brightness during a short time previous to its immersion. The occultation took place at the dark limb of the moon, which was only four days old. The dimness of the star could, therefore, hardly be ascribed to the overpowering glare of the moon's light. On a subsequent occasion the same eminent astronomer perceived that the star 49 Libræ commenced to diminish in lustre three or four seconds previous to its immersion. Similar instances of obscuration have also been occasionally observed by other astronomers, but as the phenomenon is not general the existence of a lunar atmosphere cannot be considered to be established by it. There is another phenomenon attending the occultations of stars, analogous to that already mentioned, which has been found to afford similar indications of a lunar atmosphere without placing the question of its existence beyond all doubt. When the star comes up to the moon, it is observed frequently to hang on the margin of the disk for several seconds previous to its disappearance. An effect analogous to this would manifestly be produced by the interposition of the denser strata of the moon's atmosphere between the star and the observer. On the occasion of the first of the two observations of M. Arago, above cited, the star appeared to adhere to the moon's limb for the space of three or four seconds after it was perceived to be in contact with it. This circumstance could not be ascribed to the effect of irradiation, since the occultation took place at the dark limb of the moon, which was visible by means of the *lumière cendrée*, or reflected light of the earth. Numerous similar instances of the star's adherence to the limb of the moon have been recorded by astronomers, but, as in the case of a diminution of lustre, the phenomenon is not general. In the vast majority of occultations the star comes up to the limb of the moon, and then disappears instantaneously. Its occasional adherence to the limb cannot be admitted, therefore, to afford any conclusive proof in favour of a lunar atmosphere. There is a third circumstance connected with the phenomena of occultations which is calculated to throw light upon this interesting question. It is manifest that if the moon be surrounded by an atmosphere, the refractive power of the latter would cause a star to be visible during a short time subsequent to its immersion, and also during an equal lapse of time previous to its emergence. The interval, therefore, which elapses between the disappearance of the star at the moon's limb, and its subsequent re-appearance at the opposite limb, ought to be less than the time which the moon takes to describe an arc equal to her apparent diameter, by twice the horizontal refraction of the lunar atmosphere. When the interval, however, is calculated by means of the theory of the moon's motion and her apparent diameter, corresponding to the time of occultation, the result is found to exhibit an exact accordance with that derived from direct observation. It follows, therefore, that if the moon really be surrounded by an atmosphere, its refractive power must be very inconsiderable.

The occultation of a planet by the moon suggests a peculiar mode of testing the existence of a lunar atmosphere, in addition to those already alluded to, which are also applicable to the fixed stars. When the planet has so nearly approached the moon as to be almost in contact with her limb, the rays issuing from the margin of its disk will manifestly undergo different degrees of refraction in the course of their passage through the lunar atmosphere, and an apparent distortion of its figure will be the necessary consequence. Observation, however, does not afford any indications of such a phenomenon. In 1679 Cassini and La Hire witnessed an occultation of Jupiter by the moon, and they found that from the time of the planet's approach to the limb until its complete disappearance it did not exhibit the slightest indication of a change of figure*. Similar phenomena have been observed by modern astronomers, but on no occasion has any distortion of the round appearance of the planet been distinctly discernible.

The phenomena of solar eclipses have also been employed by astronomers for the purpose of establishing the existence of a lunar atmosphere. When the dark body of the moon is projected upon the luminous disk of the sun, the rays of light proceeding from those parts of the sun which are apparently contiguous to the lunar disk, will necessarily graze the moon's surface, and if a lunar atmosphere exists, they ought, by the inflexion which they suffer in passing through it, to distort the visible portion of the solar disk, and to cause the phase of the eclipse corresponding to any assigned instant to be different from what it would be according to the theories of the solar and lunar motions. From observations of a solar eclipse which took place in 1748, Euler concluded that the horizontal refraction of the lunar atmosphere amounted to 20″*. His researches, however, were incomplete, inasmuch as he took no account of the effect of irradiation, which, by causing an apparent enlargement of the solar disk, might be expected to exercise a perceptible influence on the phase of the eclipse. The latter is, in fact, modified by two distinct causes, which are so interwoven in their operation, that it is impossible to investigate the effects of the one without taking simultaneous cognizance of those of the other. In order to ascertain whether the phenomena of solar eclipses were capable of affording any indications of a lunar atmosphere, Du Sejour, a French astronomer of the last century, undertook a rigorous investigation of the subject, founded on observations of the solar eclipse of 1764. He introduced the effects of irradiation and inflexion into his researches in the shape of two unknown quantities, and then, by means of equations of condition, which it was necessary that they should satisfy, he determined the values of them which accorded best with the totality of the observations. The conclusion at which he finally arrived was, that the effect of irradiation amounted to 3″, and that the inflexion which the solar rays suffered in passing through the lunar atmosphere amounted to an equal quantity. This gives 1·″5 for the mean horizontal refraction of the lunar atmosphere. The mean refraction of the terrestrial atmosphere at the horizon amounts to 34′, a quantity which is 1400 times greater than 1″·5. It hence follows that the lunar atmosphere is 1400 times rarer than common atmospheric air, and consequently it exceeds in this respect the most perfect vacuum which has been hitherto formed by means of the air-pump. It appears, therefore, from the phenomena of solar eclipses, that, if a lunar atmosphere really exists, it is quite inconsiderable when compared with the terrestrial atmosphere.

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* [illegible]

Auzout first remarked that the moon could not be surrounded by an atmosphere without possessing also a twilight†. This effect ought to manifest itself in a faint light, extending to a short distance within the unenlightened part of the lunar disk, and thereby rendering a narrow strip of it visible, although not directly illuminated by the solar rays. Astronomers having failed to discover any indications of such a phenomenon, it was concluded, that the moon could not be encompassed by a fluid capable of dispersing and reflecting the sun's rays. The appearance, however, was at length recognised by Schroeter, who was enabled to deduce from it the existence of an atmosphere of a small refractive power‡. When the moon exhibited a very slender crescent, he discovered a faint crepuscular light, extending from each of the cusps along the circumference of the unenlightened part of the disk. Its length was 1′ 20″, and its greatest breadth, 2″. By means of these data he found that the height of the lunar atmosphere, considered in so far as it was capable of affecting the brightness of a star, or causing a sensible inflexion of the rays of light proceeding from a celestial body, did not exceed 5376 feet. An atmosphere of such a height would form a zone round the circumference of the lunar disk, the apparent breadth of which would amount only to 0·94″. The moon would describe this arc in less than two seconds of time. This circumstance has been adduced by Schroeter as affording a sufficient explanation of the difficulty of detecting any traces of a lunar atmosphere in the phenomena of occultations and eclipses.

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* Mém. Acad. Berlin, 1748.

† Mém. Acad. des Sciences, tome vii., p. 106.

In consequence of the intense lustre of Mercury, and its constant vicinity to the sun, astronomers have experienced great difficulty in arriving at any reliable results respecting its physical constitution. According to Schroeter the planet is surrounded by an atmosphere of considerable density. The same astronomer concluded, from his observations, that there exist very high mountains on its surface. It would appear, from the results at which he arrived, that the mountains of this planet are much loftier than those of the Earth, or the Moon, the relative magnitudes of the several bodies being taken into account. He estimated the height of Chimborazo in South America at $\frac{1}{1017}$ of the Earth's radius; the highest mountains of the Moon at $\frac{1}{214}$ of the lunar radius; and those of Mercury at $\frac{1}{126}$ of its radius. He also found, by careful observations of the phases of the planet, that it revolves about an axis inclined at a considerable angle to the ecliptic. He determined the period of rotation to be $24^h\ 5^m\ 30^s$.

Since Mercury appears to have a rotatory motion, it might be reasonably supposed that her figure would not be that of an exact sphere. Schroeter, however, could not discover any trace of ellipticity in the appearance of the planet. The observations of Sir William Herschel conducted him to a similar conclusion. That illustrious astronomer observed the transit of the planet which took place in November, 1803, but, although he devoted great attention to the appearance of the disk, he was unable to persuade himself that it exhibited even the slightest deviation from a circular form[*]. He therefore came to the conclusion that, unless the polar axis was turned towards the earth, the planet did not possess any sensible ellipticity. It appears, however, from recent micrometrical measures executed by the Rev. Mr. Dawes on the occasion of the transit of November, 1848, that the figure of the planet is really spheroidal to a small extent. The mean of several results, obtained by different methods of measurement, indicated an ellipticity equal to $\frac{1}{29}$[†].

One of the most interesting of the various phenomena which disclosed themselves to the admiration of Galileo, when he first directed the telescope to the heavens, was the appearance presented by the planet Venus. When observed near her inferior conjunction she exhibited a slender crescent of light, like the moon when she is a few days old. As she receded from the position intervening between the sun and the earth, the phase continued to increase in magnitude until, upon approaching her superior conjunction, she acquired a round appearance, like the full moon. The conclusion was irresistible, that the planet was an opaque body, which owed its luminous appearance solely to its power of reflecting the solar rays which fell upon its surface. An important point of resemblance was thus established between it and the earth, which formed the ground of an argument of overwhelming force in favour of the Copernican system of the world.

As in the case of Mercury, the intense lustre of this beautiful planet has proved a serious obstacle to astronomers, in their attempts to arrive at some knowledge respecting its physical constitution. In the years 1666–7 Cassini, while still residing under the clear sky of Italy, devoted his attention to a searching examination of the planet, with the view of detecting periodic changes in its appearance which might indicate a rotation on an axis. On the 21st April, 1667, he at length discovered a bright spot, which, when watched for some time, was found to return to the same position on the disk of the planet at successive intervals of about 23 hours. This would indicate a rotation of the planet agreeing very nearly with the diurnal motion of the earth. Cassini, however, was unable to follow the spot throughout an extent of its course sufficiently large to enable him to ascertain positively whether its change of position arose merely from a libratory motion of the planet, or from a complete rotation on an axis. Upon a subsequent occasion, during his residence in France, he resumed the examination of the planet, with the view of perfecting his previous researches, but although he strove, with great perseverance, to detect the spot on its disk which was visible in Italy, all his efforts were fruitless. In 1726 Bianchini, the domestic prelate of the Pope, commenced an examination of Venus at Rome, with a powerful telescope constructed by the celebrated Campani, and soon succeeded in detecting several dark spots on the disk of the planet, the motion of which he continued for some time to watch with great attention. The conclusion he arrived at was somewhat anomalous. He announced that the changes in the positions of the spots could not be satisfactorily accounted for without supposing a period of rotation amounting to $24^d\ 8^h$. The researches of astronomers on the rotation of the planet formed the subject of a critical discussion by J. Cassini. He remarked that the continuity of Bianchini's observations was interrupted by the interposition of the Barbarini Palace, which every night concealed the planet from him for some time. Adopting a hypothesis founded on this chasm in the observations, he shewed that if the period of rotation be supposed equal to $23^h\ 20^m$, the observations of his father as well as those of Bianchini may be equally well satisfied; but he remarked that if the period of $24^d\ 8^h$ be admitted, his father's observations must be rejected as totally worthless*.

The value assigned by Cassini to the period of the rotation of Venus has received a complete confirmation from the researches of modern astronomers. In 1789 Schroeter, having undertaken a careful examination of the planet with a seven-feet reflector, discovered a luminous point in the dark hemisphere, a little beyond the southern horn, indicating the existence of a high mountain. By continuing for some time to direct his attention to the periodic changes of this object, he was finally enabled to deduce from them the conclusion, that the planet performs a complete rotation on its axis in a period amounting to $23^h\ 21^m\ 19^s$. This result agrees with that recently arrived at by the late Sig. De Vico and his associates, of the Observatory of the Roman College. In 1839 they commenced a series of observations of Venus with a magnificent refractor by Cauchoix. In order more effectually to obviate the inconvenience arising from the intense lustre of the planet, the examination was conducted during the day time†. The ancient spots observed by Bianchini were speedily re-discovered, and were found to possess the precise forms assigned to them by that astronomer. The final conclusion was, that the planet performs a complete rotation on its axis, in a period agreeing almost exactly with that which Schroeter had deduced from his observations.

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* Élémens d'Astronomie, p. 526.

† A similar mode of observing the planet was employed by La Hire. See Mem. Acad. des Sciences, tome x., p. 20.

The various astronomers who have determined the rotation of Venus agree in estimating the inclination of the equator to the plane of the ecliptic at 75°. No trace of ellipticity has been discovered in the planet; nor will this appear surprising when the probable smallness of its amount is considered. If the equatorial axis exceeded the polar by only $\frac{1}{300}$th of its own length, as in the case of the earth, the difference of the two axes would subtend an angle of only a tenth of a second at the mean distance of the planet from the earth. It is obvious that the most delicate micrometrical measurements are incapable of establishing the existence of so small a quantity. That the ellipticity of the planet does not differ much from that of the earth, is probable from the circumstance that both bodies are nearly of the same size, and are endued with nearly equal velocities of rotation.

According to Schroeter there exist mountains of immense height on the surface of Venus. The most considerable of these elevations amounts to $\frac{1}{140}$ of the radius of the planet. Chimborazo, in South America, which was long supposed to be the highest mountain on the surface of the earth, attains an elevation equal only to $\frac{1}{1017}$ of the terrestrial radius. The same astronomer remarks that Venus, as well as Mercury and the Earth, have the highest mountains situate in their southern hemispheres. These results can only be admitted with a certain degree of reserve, when the extreme delicacy of the observations by means of which they are arrived at, is taken into account. Even the great Herschel was unable to deduce any trustworthy results of this nature from his observations of the planet. The remark of Schroeter, that the three planets nearest the sun have the highest mountains situate in their southern hemispheres, is at least no longer applicable to the earth, since the Himalayah range of mountains has been found to be the most elevated region on its surface.

Various circumstances concur to prove that Venus is surrounded by an atmosphere of considerable density. During the transits of the planet over the sun's disk in 1761 and 1769, the planet was observed, by several astronomers, to be surrounded by a faint ring of light similar to the appearance which would be occasioned by the passage of the solar rays through a circumambient fluid. This indication of the existence of an atmosphere is confirmed by the observations of Schroeter, who discovered what appeared to him to be a faint crepuscular light, extending beyond the cusps of the planet into the dark hemisphere[*]. From micrometrical measures of the space over which this light was diffused, he concluded that the horizontal refraction at the surface of the planet amounts to 30′ 34″, a quantity agreeing very nearly with the horizontal refraction of the terrestrial atmosphere[†].

When the planet Mars is observed with a telescope of ordinary power, its surface appears to be diversified with spots of an irregular character, which have conducted astronomers to some interesting conclusions respecting its physical constitution. These appearances were first noticed by Fontana, a Neapolitan astronomer. In 1636 he observed a spot on the disk of the planet, which subsequently re-appeared in 1638. From the changes which it seemed to undergo, he suspected that the planet was endued with a rotation on an axis. Hooke is one of the first astronomers who arrived at this conclusion by reasoning of a strictly legitimate character, founded upon his own observations of the planet. On the 28th of March, 1666, he communicated a paper to the Royal Society, containing an account of observations of certain spots on the disk of the planet. He mentions that he had seen the spots in the month of February, 1666; but the earliest observation contained in his paper is dated the 3rd of the following month. Having found that the spots returned at regular intervals to the same position on the disk, he inferred that the planet accomplishes a rotation on an axis in a period amounting to 12 or 24 hours [*]. The contemporaneous observations of Cassini in Italy conducted that astronomer to a result of greater precision. He concluded, from the interval which elapsed between the return of the spots to the same position on the disk, that the planet performs a complete revolution round its axis in 24^h 40^m. This result received a satisfactory confirmation from the researches of the elder Maraldi. In 1704 that astronomer, having watched the changes in the positions of the spots, inferred a period of rotation equal to 24^h 39^m. In the autumn of 1719, there occurred a peculiarly favourable opportunity for verifying this conclusion. When the planet arrived in opposition on the 27th August of that year it was only 2½° distant from the perihelion, and on account of its proximity to the earth it shone with such uncommon splendour that many persons supposed it to be a comet or a new star. On the 19th August, Maraldi, having examined the planet with a telescope 34 feet long, perceived two obscure bands on the disk, forming with each other an obtuse angle, which exhibited a very conspicuous point [†]. On the 25th September, he again observed the planet, and perceived the angular point in the same position on the disk. During the interval of 37 days, which had elapsed between the two observations, the planet had therefore completed 36 rotations on its axis. This gave 24^h 40^m for the period of rotation, a result agreeing exactly with that arrived at by Cassini. In the years 1777 and 1779, Sir William Herschel carefully observed the changes in the appearance of the planet, and concluded from them that the period of rotation amounted to 24^h 39^m $21^s{\cdot}67$ [‡]. He also found that the inclination of the planet's equator to the ecliptic amounts to 28° 42′, and that its ascending node is situate in 19° 28′ of Sagittarius [§]. The recent researches of MM. Beer and Mädler make the period of rotation to be 24^h 37^m 23^s.

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[*] Phil. Trans., 1792.

[†] The interesting fact of the existence of a twilight in Venus has been confirmed by

Mars being endued with a rotation on a fixed axis, it is reasonable to conclude that the figure of the planet will be flattened at the poles. It was reserved for Sir William Herschel to establish this interesting fact upon the basis of observation. By micrometrical measurement he determined the lengths of the equatorial and polar axes to be as 1355 to 1272, or as 16 to 15 nearly [‖]. This indicates an ellipticity considerably greater than what appears necessary to counterbalance the centrifugal force of the equatorial parts of the planet, if its density be supposed homogeneous. The discordance, however, in this as well as in all similar instances, may be explained by an adequate supposition respecting the internal structure of the planet.

The earlier observers of Mars had remarked the appearance of very bright spots at the poles of his equator. Maraldi, who devoted much attention to these phenomena, suspected that they were the indications of physical changes occurring on the surface of the planet. In 1777–79, Sir William Herschel examined the telescopic appearance of the planet with instruments far superior in power to any that had been hitherto used for a similar purpose, and with the scrupulous attention to accuracy which characterized all the observations of that illustrious astronomer. He supposed the bright appearances around the poles to be the light reflected from masses of ice and snow accumulated in the polar regions; and he ascribed their occasional diminution to the dissolving influence of the solar rays, when they became exposed to them by the revolution of the planet in its orbit. The changes actually exhibited by the spots afforded a striking confirmation of this view of their origin. Thus, in the year 1781, the spot about the south pole was extremely large, but this might naturally have been expected, since the pole had been recently involved in perpetual darkness during the space of twelve months. In 1783 it had become considerably smaller, and it continued to decrease from the 20th May of that year till about the middle of September. During the last-mentioned interval, the south pole had already been above eight months enjoying the benefit of summer, and still continued to receive the sunbeams, though, towards the close of the period, in so oblique a direction as to derive but little benefit from them. On the other hand, in the year 1781, the north polar spot which had been enjoying the sunshine for twelve months, and was but lately returning to darkness, appeared small, though increasing in size. It was indeed invisible in the year 1783, but this arose from the position of the axis of the planet, by which it was removed out of sight*. The explanation of Sir William Herschel has been generally adopted by astronomers, as the most probable that can be offered respecting the interesting phenomena to which allusion has just been made. It is hardly necessary to remark that such an explanation implies the existence of an atmosphere about the planet.

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[*] Phil. Trans., No. 14.

[†] Mém. Acad. des Sciences, 1720.

[‡] Phil. Trans. 1781, p. 134.

[§] Phil. Trans. 1784, p. 273.

[‖] Ibid.

If we consider the arrangement of the planetary system as it was known previous to the discovery of Uranus, it will readily occur, either from a comparison of the numerical values of the mean distances of the planets, or from an inspection of their orbits when traced concentrically by a graphic process on a sheet of paper, that while the intervals between the successive orbits increase continually outwards from the sun, a vacuity, disproportionately larger than any of the others, exists between the orbits of Mars and Jupiter. When Kepler, during the earlier period of his astronomical career, was endeavouring to discover a connexion between the mean distances of the planets from the sun, and their motions in their orbits, he encountered in this anomalous fact an insuperable obstacle to the success of his speculations. The most plausible hypothesis that his fertile imagination could suggest, appeared to require that the vacuity should be filled up by the interposition of another orbit. At length, having despaired of reconciling the actual state of the planetary system with any theory he could form respecting it, he hazarded the assertion that a planet really existed between the orbits of Mars and Jupiter, and that its smallness alone prevented it from being visible to astronomers. This bold surmise of the illustrious discoverer of the laws of the planetary movements, was generally regarded as not the least probable of those transient sallies of the imagination in which he delighted to indulge, and it was favourably alluded to by Lambert in his "Cosmological Letters," published in 1769. In 1772, Bode published a treatise on astronomy in which he first announced the singular relation between the mean distances of the planets from the sun, which has since been distinguished by his name. This relation, to which allusion has already been made in one of the foregoing chapters, exhibited in a very striking light the exaggerated leap from Mars to Jupiter, and suggested the strong probability of a planet revolving in the intermediate region. This conjecture was rendered still more plausible by the discovery of the planet Uranus, in 1781, the distance of which from the sun was found to conform exactly to the law of Bode. In Germany, especially, a strong impression had been produced that a planet really existed between Mars and Jupiter; and, through the active exertions of De Zach, an association of twenty-four astronomers was formed, having for its object to effect the discovery of the unknown body. For this purpose the zodiac was divided into twenty-four zones, one of which was to be explored by each astronomer; and the conduct of the whole operation was placed under the superintendence of Schroeter. Soon after the formation of this society the planet was discovered, but not by any of those astronomers who were engaged expressly in searching for it. Piazzi, the celebrated Italian astronomer, while engaged in constructing his great catalogue of stars, was induced carefully to examine, several nights in succession, a part of the constellation Taurus, in which Wollaston, by mistake, had assigned the position of a star which did not really exist. On the 1st January, 1801, Piazzi observed a small star, which, on the following evening, appeared to have changed its place. On the 3rd he repeated his observations, and he now felt assured that the star had a retrograde motion in the zodiac. The daily change of position in right ascension was 4′, and the change in declination towards the north pole was 3′ 30″. On the 24th of January, he transmitted an account of his discovery to Oriani and Bode, communicating the positions of the star on the 3rd and 23rd of that month, and adding that its motion, from being retrograde on the 11th, had become direct on the 13th of the same month. Piazzi continued to observe the star until the 11th of February, when he was seized with a dangerous illness which completely interrupted his labours. His letters to Oriani and Bode did not reach these astronomers until the latter end of March, but the planet had then approached too near the sun to admit of their obtaining a verification of his discovery by actual observation, and it was necessary for this purpose to wait until the month of September, when the planet would have effectually extricated itself from the solar rays. Its rediscovery, after the lapse of so considerable a period subsequent to the most recent observation, could not be expected to be accomplished without a pretty accurate knowledge of the orbit in which it was moving; but the data communicated by Piazzi were insufficient for this purpose. They merely served to indicate that the body revolved in a circular orbit between Mars and Jupiter, at a distance agreeing very nearly with that assigned by Bode's law, and so far offered a satisfactory confirmation of the views of the German astronomers. Meanwhile Piazzi, fearing lest he should be deprived in any degree of the glory attached to the discovery of the planet, communicated to astronomers all the observations of it made by him down to the 11th February. Gauss found that they might be all satisfied within a few seconds, by an elliptic orbit, of which he calculated the elements; and, with the view of aiding astronomers in searching for the planet, that illustrious geometer also computed an ephemeris of its motion for several months. After a careful examination of its geocentric path, the planet was finally discovered by De Zach on the 31st of December, and by Olbers on the following evening. A year had therefore elapsed between the original discovery of the planet by Piazzi and its subsequent re-discovery by the German astronomers. Piazzi conferred on it the name of Ceres, in allusion to the titular goddess of Sicily, the island in which it was discovered; and the sickle has been appropriately chosen for its symbol of designation.

The mean distance of Ceres, as determined by the calculations of Gauss, was 2·767. The distance assigned by Bode's law is 2·8. In this respect, therefore, the new member of the planetary system harmonized admirably with the other bodies of which it was composed. In other respects, however, it offered unexpected anomalies. Sir William Herschel found, by micrometrical measures of the planet's apparent magnitude, that its diameter at the mean distance of the earth from the sun would subtend an angle of only 0″·35. This result makes the linear diameter amount to only 161 miles. The newly-discovered body was, therefore, excessively minute, when compared with any of the older members of the planetary system. Its motion also exhibited peculiarities which in some degree assimilated it to bodies of a cometary nature. Its inclination to the ecliptic excededed 10°, and consequently it deviated from that plane to a much greater extent than any other planet.

The interesting discovery of Piazzi was soon followed by another of a similar nature. Olbers, while engaged in searching for Ceres, had studied with minute attention the various configurations of all the small stars which are situate near the geocentric path of that planet. On the 28th of March, 1802, while examining the north-western part of the constellation Virgo, his attention was directed to a small unknown star of the 7th magnitude. It was situated very near the place where he discovered Ceres, and it formed an equilateral triangle with two other small stars, whose positions were given in the catalogues of astronomers. From the intimate knowledge he had acquired of the region in which the star appeared, he felt assured that he had not perceived it on any former occasion; and his first impression was, that in all probability it was a variable star which had attained its maximum state of brightness. After the lapse of two hours, however, he found that it had a proper motion, the right ascension having sensibly diminished during that interval, while, on the other hand, the declination had increased towards the north. The observations of the following evening established beyond all doubt his suspicion that the body was a planet; its right ascension having diminished to the extent of 10′, while the increase of declination was 20′. On the 28th of April, when a month only had elapsed since the discovery of the planet, Gauss assigned the elements of the elliptic orbit in which it was found to revolve, and the results derived from them exhibited a most satisfactory accordance with observation. The mean distance, as determined by Gauss, was 2·670. The planet, therefore, offered in this respect a remarkable analogy to Ceres, the mean distances of both planets agreeing within a small fraction. Like Ceres also, it was remarkable for the minuteness of its volume. From Herschel's estimate of its apparent magnitude, it followed that its linear diameter did not amount to more than 110⅓ miles *. In the form and position of its orbit, it exhibited deviations from the older planets, much more considerable than those by which Ceres was distinguished. The eccentricity was found by Gauss to be 0·24764, and consequently was greater than that of Mercury, the most remarkable of all the older planets in this respect. But the most striking feature of the planet's motion, if

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* Phil. Trans., 1802, p. 218.

we except the coincidence of its mean distance with that of Ceres, was the inclination of its orbit to the ecliptic, which amounted to the very considerable angle of 34° 39′. In consequence of the various peculiarities of the two new bodies, Sir William Herschel proposed to denominate them asteroids, instead of planets, and this appellation is frequently applied to them as well as to the other bodies which have since been discovered revolving in the same region.

An examination of the relative magnitudes of the planetary orbits had suggested the existence of an unknown planet, revolving between the orbits of Mars and Jupiter. Instead of one planet, however, two were found, conforming in this respect to the requirements of analogy. Considering this singular fact, in connexion with the extreme minuteness of the two bodies, and the other circumstances by which they were distinguished from the more ancient planets, Olbers was led to suspect that they might possibly be the fragments of a larger planet, which had once been revolving in the same region, and at an anterior epoch had been shattered to pieces by the energy of some unknown cause. Pursuing this ingenious conception, he was led to conclude that there might be many more similar fragments which had not yet been discovered. He also inferred that the eccentricities and inclinations of their orbits might be very different, but that the mean distances would be nearly equal, and, as they all had a common origin, that their orbits would have two common points of intersection, situate in opposite regions of the heavens, through which every fragment would necessarily pass in the course of each revolution. He proposed, therefore, to search carefully every month the north-western part of the constellation Virgo, and the western part of the constellation of the Whale, being the two opposite regions in which the orbits of the two bodies already discovered were found to intersect each other. Meanwhile the discovery of a third planet tended in a strong degree to confirm the truth of his hypothesis, and to encourage him in his arduous undertaking. In consequence of the extreme smallness of Ceres and Pallas, they are liable to be confounded with the telescopic stars, which lie near their paths, and consequently when any time is allowed to elapse without their being observed, it becomes a difficult matter to recognise them again. With the view of facilitating this object, M. Harding, one of the astronomers attached to the Observatory of Lilienthal, had undertaken to construct a series of charts, whereon were laid down the positions of all the small stars which appear near the geocentric paths of the two planets. On the 2nd September, 1804, while engaged in exploring the heavens for this purpose, he perceived a small star in the constellation Pisces, very near to that part of the constellation of the Whale, through which Olbers had asserted that the fragments of the shattered planet would be sure to pass. On the 4th of September he repeated his observation, and found that the star had changed its place. The observations of the succeeding evenings established, beyond all doubt, that the star was in reality a new planet. The elements of its orbit were calculated by Gauss, who found, as in the case of the other two planets recently discovered, that it revolved between the orbits of Mars and Jupiter. The mean distance was 2·670, consequently it almost coincided with the mean distances of Ceres and Pallas. The eccentricity amounted to 0·2543, and therefore surpassed that of any other member of the planetary system. Harding conferred on the planet the appellation of Juno, with a sceptre surmounted by a star for the symbol of designation. Like Ceres and Pallas, it is remarkable for its smallness. Herschel was unable to pronounce with certainty that its diameter exhibited any sensible magnitude.

Stimulated by the discovery of Juno, which so remarkably supported his theory, Olbers continued, with unremitting assiduity, to explore the two opposite regions of the heavens, which appeared to him to offer the strongest probability of detecting any additional fragments of the shattered planet. At length, after he had been engaged nearly three years in this laborious pursuit, his admirable perseverance was crowned with success. On the evening of the 28th of March, 1807, as he was proceeding to re-examine the northern wing of the constellation Virgo, his attention was drawn all at once to a brilliant star of the sixth magnitude, which appeared a little to the west of the star marked 223 in Bode's catalogue. From his intimate acquaintance with this part of the heavens, he felt persuaded that the star had not appeared in it on any former occasion, and he concluded, without hesitation, that it was a planet. On the same evening he established this fact beyond all doubt, having ascertained, by means of two observations, that the motion of the body was retrograde. The elements of the orbit were determined by Gauss, who executed the calculations required for this purpose before more than ten hours had elapsed after he obtained possession of the observations. The planet was found to revolve in the same region with Ceres, Pallas, and Juno, its mean distance being somewhat less than that of any of those bodies. Olbers delegated to Gauss the privilege of naming the planet. The illustrious geometer chose the appellation of Vesta, the symbol of designation being the altar on which burned the sacred fire in honour of the goddess. This planet is even smaller than any of the three others previously discovered in the same region. It is remarkable for the brilliancy of its light. Schroeter states that he once saw it with the naked eye.

The hypothesis of Olbers respecting the origin of the asteroids was in many respects so plausible that geometers were induced to direct their attention to the subject. In the "*Connaissance des Temps*" for 1814, Lagrange has investigated the explosive force which would be necessary to detach a fragment of matter from a planet revolving at a given distance from the sun. Applying his results to the earth, he found that if the velocity of the detached fragment exceeded that of a cannon ball in the proportion of 121 to 1, the fragment would become a comet with a direct motion; but if the velocity rose in the proportion of 156 to 1, the motion of the comet would be retrograde. If the velocity was less than in either of these cases, the fragment would revolve as a planet in an elliptic orbit. For any other planet besides the earth, the velocity of explosion corresponding to the different cases would vary in the inverse ratio of the square root of the mean distance. It would, therefore, manifestly be less, as the planet was more distant from the sun. In the case of each of the four smaller planets, the velocity of explosion, indicated by their observed motion, would be less than 20 times the velocity of a cannon ball.

Although a strong probability existed that many more bodies revolved between the orbits of Mars and Jupiter similar to those already discovered, a considerable period elapsed before the planetary system was enriched by any further accession from this source. This circumstance arose, doubtless, from the immense labour attending the operation of searching for the bodies, and the want of good charts containing a representation of the relative positions of all the small stars situate in those regions where it might be expected that the bodies would be discovered. In 1825, a fresh impetus was given to researches of this nature, by the resolution of the Berlin Academy of Sciences to procure the construction of a series of charts representing the relative positions of all the stars, down to the tenth order of magnitude, in a zone of the celestial regions extending 15° on each side of the equator. All the charts connected with this great undertaking have not yet been executed; but we have already had occasion to illustrate their great utility, in our account of the discovery of the planet Neptune. It was probably by reflecting on the advantage of such delineations of the celestial regions that M. Hencke, an amateur astronomer of Driessen, in Germany, was induced, about the year 1830, to commence a careful survey of those regions wherein it was most likely that any additional planets would be found. During fifteen years he continued with unwearied assiduity to prosecute this object, tracing the relative positions of the small stars, and making himself acquainted with their various configurations. At length his unflinching perseverance met with its due reward. On the 8th of December, 1845, while engaged in examining a portion of the fourth hour of right ascension, his attention was directed to a small unknown star which appeared, between two others of about the same size, with it. As he had acquired an intimate acquaintance with this part of the heavens, he felt confident that the star had not been previously visible in it. He therefore suspected that he had discovered a planet, and he wrote to M. Encke, of the Observatory of Berlin, announcing the existence of the unknown star, and communicating to him its position on the day before mentioned. On the evening of the 14th of December, the Berlin astronomers directed the magnificent refractor of the observatory to the part of the heavens in which M. Hencke had asserted he had seen the suspected planet, and presently they discovered a star of the ninth magnitude, which was not marked in the corresponding chart of the academy, executed by Professor Knorre. The subsequent observations of the same evening shewed it to be a planet. It was then retrograding with a daily motion in right ascension, amounting to 14′ 21″.2 of arc. On this occasion the elements of the orbit were rapidly determined, not by Gauss individually, as on previous occasions of a similar kind, but by a host of young astronomers throughout Europe, who had become familiar with the methods of that illustrious master. The results of their calculations shewed the body to be one of the family of asteroids. The honour of naming it was delegated by the discoverer to M. Encke, who conferred on it the appellation of Astræa.

The industry of M. Hencke was soon again rewarded by a similar discovery. On the 1st of July, 1847, while engaged in examining the seventeenth hour of right ascension, he perceived a small star of about the ninth order of magnitude, which was not marked in the corresponding map of the academy, constructed by Dr. Bremiker. On the 3rd he repeated his observation, and found that during the intermediate period its right ascension had sensibly diminished, while its southern declination had increased. It proved to be another of the smaller planets. Gauss, who had been invited to choose its name, bestowed on it the appellation of Hebe.

The year 1847 was not destined to come to a close before it was signalized by two other discoveries of a similar nature. Mr. Hind, the Astronomer attached to the Observatory of Mr. Bishop, Regent's Park, London, had been for some time engaged in revising the Berlin maps, of which the utility had now become so apparent. He had also under-ascension, representing the relative positions of all the telescopic stars down to the tenth magnitude. On the 13th of August, 1847, while surveying the heavens in the prosecution of these labours, his attention was drawn to a star of the 8.9th magnitude, which was not marked in Hora xix. of the Berlin Maps constructed by Wolfer. He was especially surprised to find that, although the same region had been examined on the 22nd of June, and also on the 31st of July, no note had been made in reference to the unmarked star. Even this circumstance, however, was not considered by him to afford conclusive proof in favour of a proper motion of the star, since it appeared to him not improbable that the previous invisibility of the star might arise from its light being variable, having met with a great many new stars of this nature in the course of his observations. The application of the micrometer, however, dispelled all doubts on this point. Before half an hour elapsed he was enabled to satisfy himself, by its change of position, that the star had a proper motion. Subsequent observations shewed that it was one of the ultra-zodiacal planets. At the suggestion of Mr. Bishop, the planet was named Iris. On the 18th of October of the same year, Mr. Hind discovered a star of the tenth magnitude, which was not marked in Hora v. of the Berlin Maps. Micrometrical measures of its position, made after the lapse of about three hours and a half from the time when he first observed it, established the existence of a proper motion. It proved to be another planet of the same class with Iris. Sir John Herschel, having been invited to name the planet, conferred on it the appellation of Flora. The discovery of the two planets Iris and Flora reflects equal credit on the assiduity of Mr. Hind, and on the enlightened zeal of the gentleman under whose auspices he has been prosecuting his observations. It is pleasing to reflect, that in recent times the British Isles have furnished a greater number of examples of the devotion of private resources to the cause of astronomical science than any other country.

The planet Flora formed the eighth fragment of the hypothetic planet of Olbers which had been discovered since the commencement of the present century. Two more have subsequently been added to this number. One of them was discovered on the 25th of April, 1848, by Mr. Graham, of Mr. Cooper's Observatory, Markree, Ireland. It has received the name of Metis. This planet is remarkable for the near coincidence of its mean motion with that of Iris, the difference of their periodic times having been found by the most recent calculations to amount only to five days. The other planet was discovered at Naples, on the 16th of April, 1849, by M. De Gasparis. At the suggestion of Sig. Capocci it has been called Hygeia. This makes ten fragments of the shattered planet which have been already recovered. When it is considered that, since the discovery of six of these fragments, an equal number of years has not yet come to a close, it may reasonably be presumed that a long period is not destined to elapse before many more similar discoveries will be made.

When Jupiter is observed with a telescope of considerable power, there appear several obscure belts encompassing his body. These belts have been found to vary in number, magnitude, and mutual distance; but for the most part three only are observed, extending in parallel directions across the centre of the disk. These phenomena escaped the observations of Galileo, his telescopes not being sufficiently powerful to exhibit them. According to Riccioli, they were first observed by Zucchi, an Italian Jesuit *. On the 17th of May, 1630, that individual perceived two belts on the disk of the planet. In 1633, Fontana perceived three belts. In 1648, Grimaldi was unable to perceive more than two. He was the first person who remarked that the direction of the belts is parallel to the plane of Jupiter's orbit. In addition to the belts which surround the body of the planet, the rest of the surface appears diversified with dusky spots of an irregular character. These maculæ are generally of transient duration, but some which appear in the vicinity of the belts are of a more permanent nature than the others; and astronomers, by watching from time to time their position on the disk, have been enabled to establish the interesting fact that the planet revolves round a fixed axis. It is worthy of remark, that the illustrious Kepler, previous to the invention of the telescope, had surmised that Jupiter revolves on an axis, and from a consideration of the rapid motion of the satellites of the planet round their primary, compared with the slower motion of the moon round the earth, he inferred that the period of rotation is less than twenty-four hours. This sagacious conjecture has received a remarkable confirmation from the researches of subsequent astronomers. Hooke appears to have been the first person who was led by observation to suspect that Jupiter revolves on an axis. In the month of May, 1664, he discovered a small spot on the largest of the three belts. In the course of two hours, he found that it had moved from east to west about half the length of the diameter of the planet †. With that fickleness of purpose which characterized his conduct throughout life, he appears to have abandoned all further observations of the planet, leaving to others in this as in almost all similar instances, the glory of substantiating the interesting fact which, by his superior acuteness, he was enabled first to foreshadow. It was reserved for Cassini to prove, beyond all doubt, that Jupiter revolves on an axis, and to assign with considerable precision the period of rotation. In the month of July, 1665, he observed the planet with a telescope constructed by Campani, the object-glass of which had a focal length of thirty-five French feet. He discovered a great number of spots on the disk, some of which were brighter than the rest of the surface, resembling in this respect the solar faculæ. They all appeared to have a motion on the disk from east to west, but in every instance they exhibited such rapid variations of form and magnitude, that no certain result could be deduced from them. At length he discovered a permanent spot situated close to the northern border of the most southern of the three belts, which he continued to observe for several months. It appeared largest on the centre of the disk, from which it gradually diminished until, before reaching the margin, it became invisible. Its motion also was swifter on the centre of the disk than towards the circumference. From these facts Cassini concluded that the spot adhered to the surface of the planet, and that the latter consequently revolves on an axis. With respect to the time in which it effected a complete

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* Almag. Nov. tome i., p. 486. Dr. Moll, of Utrecht, in his interesting account of the invention of the telescope, published in the first volume of the Journal of the Royal Institution, ascribes the discovery of Jupiter's belts to Toricelli, the pupil of Galileo.

† Phil. Trans., No. 1. This communication, consisting of eight lines, is the second which appears in the famous collection of the Transactions of the Royal Society.

rotation, he found that the belts generally presented a very altered appearance after the lapse of five hours. It followed, therefore, that the time of rotation was somewhere about ten hours. In order to arrive at a result of greater precision, he watched the motion of the spot which appeared near the southern belt, and he found that it returned to the same position on the disk exactly in $12^d\ 0^h\ 4^m$. During this lapse of time it had effected twenty-nine complete rotations, consequently the period of a single rotation was $9^h\ 56^m$. Cassini attempted on subsequent occasions to determine the time of rotation, but the results obtained by him did not exhibit so exact an accordance with each other as was desirable. It appeared, from this circumstance, impossible to withhold the conclusion that the spots, besides participating in the rotatory motion of the planet, had also a proper motion on his surface. The accurate observations of Sir William Herschel lead to a similar conclusion. That illustrious astronomer carefully watched the spots of the planet in the years 1778–79, but the various results at which he arrived relative to the time of rotation fluctuated between $9^h\ 55^m\ 40^s$ and $9^h\ 50^m\ 48^s$; the discordance, therefore, amounted, in some instances, to $4^m\ 52^s$, a quantity too great by far to be ascribable wholly to errors of observation[*]. Schroeter experienced similar anomalies, in the course of his attempt to determine the rotation of the planet. He concluded, from his observations, that the most probable value of the period of rotation is $9^h\ 55^m\ 33^s.6$. In the years 1834–5, Mr. Airy made observations on the planet, at Cambridge, for the purpose of determining the time of rotation. He perceived a dark spot below the apparent lower belt, which seemed to him to be well adapted for establishing this point. His observations comprehended 225 rotations, commencing December 16, 1834, and terminating March 19, 1835. The period of rotation was found by means of them to be $9^h\ 55^m\ 21^s.3$[†]. Mr. Airy considers that the probable error of this result does not exceed four seconds.

Since Jupiter revolves round an axis, it might be expected that his figure would be flattened at the poles; and further, it might be inferred, from his immense magnitude and the rapidity of his rotation, that the flattening would exceed that of the terrestrial spheroid. The results of observation have been found to accord with both these conclusions. In 1691 Cassini remarked that Jupiter appeared to him to have an oval shape. The greater diameter extended east and west, and it seemed to him to exceed the diameter at right angles to it by about a fifteenth part[‡]. The remark of that illustrious astronomer was soon confirmed by the observations of La Hire, Roemer, and Picard. The first determination of the ellipticity of the planet by careful measurement is due to Pound, whose results assign to it a mean value equal to $\frac{1}{13.25}$[§]. In the present day M. Struve has found, by means of micrometrical measurements made with Fraunhofer's great refractor, that the apparent magnitude of the equatorial diameter, as seen from the sun, at the mean distance 5.20279, is equal to $38''.327$, and that the apparent magnitude of the polar diameter, at the same distance, is equal to $35''.538$[‖]. These results give $\frac{1}{13.71}$ for the ellipticity of the planet. We have seen that Laplace found from theory, that if the equatorial axis of the planet be represented by unity, the polar axis will be equal to 0.9286. In the present case, by adopting the same unit of length, the polar axis will be found to be equal to 0.9272. A more striking instance of agreement between theory and observation could not be adduced from the whole body of physical science.

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[*] Phil. Trans., 1781.

[†] Mem. Ast. Soc., vol. ix., p. 5.

[‡] Anc. Mém. Acad. des Sciences, tome ii., p. 130.

[§] Principia, lib. iii., prop. 19.

[‖] Mem. Ast. Soc., vol. iii., p. 301

The great distance of Jupiter prevents the discovery of any phenomena analogous to those which have rendered the existence of an atmosphere about Venus so probable; nor do the polar regions of the planet exhibit any changes of appearance resembling those which have conducted astronomers to a similar conclusion with respect to Mars. In the absence of these favourable circumstances, the rapid rotation of the planet comes to the aid of the astronomer; for it can hardly admit of doubt, that the intense centrifugal force arising from this cause is intimately connected with the formation of those remarkable bands which encompass the equatorial regions of the planet, in directions parallel to each other and to the plane of rotation. Although, generally, there appear only three obscure belts on the disk of the planet, sometimes a greater variety is perceptible. In 1664, Campani perceived four dark belts and two white ones. In 1691, Cassini saw eight obscure belts on the disk of the planet, situate very close to each other *. Sometimes only one belt is visible, and this is always the principal belt, which is situate on the northern side of the planet's equator. On the other hand, the whole surface of the planet has occasionally been seen covered with belts. On the 18th of January, 1790, Sir William Herschel, having observed the planet with his 40-feet reflector, perceived two very dark belts, divided by an equatorial zone of a yellowish colour, and on each side of them were dark and bright belts alternating, and continued almost to the poles †. A similar appearance was also once observed by Messier ‡. These phenomena sometimes undergo very rapid transformations, affording thereby a strong proof that they owe their origin to the fluctuating movements of an elastic fluid enveloping the body of the planet. On the 13th of December, 1690, Cassini perceived five belts on the planet, two in the northern hemisphere, and three in the southern. An hour afterwards there appeared only the two belts nearest the centre, and a feeble trace of the remaining northern belt §. The same astronomer frequently witnessed the formation of new belts on the planet in the course of one or two hours ||.

The dark spots on the disk of the planet also afford unequivocal indications of the existence of an atmosphere, for it is impossible to reconcile their variable velocities with the supposition of their being permanent spots adhering to the surface of the planet. Cassini found, from his observations, that the spots near the equator of the planet revolved with a greater velocity than those more distant from it ¶. This fact has been confirmed by the more recent observations of Schroeter. Sir William Herschel found that the velocity of a spot sometimes underwent a very sensible change in the course of a few days. He supposes the spots to be large congeries of clouds suspended in the atmosphere of the planet, and he ascribes their movements to the prevalence of winds on its surface, which blow periodically in the same direction **. From the circumstance that

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* Mém. Acad. des Sciences, tome x., p. 6. Élémens d'Astronomie, p. 406.

† Phil. Trans., 1794, p. 30.

‡ Delambre, Astronomie Théorique et Pratique, tome iii., p. 77.

§ Élémens d'Astronomie, p. 405.

|| Anc. Mém. Acad. des Sciences, tome ii., p. 105. ¶ Ibid., p. 130.

** Phil. Trans., 1781, p. 118.

the spots generally assign to the planet a period of rotation less than that which appears to be its most probable value, it may be inferred that the wind constantly blows in the same direction as that in which the planet revolves upon its axis. Herschel found that a spot, after its first appearance, continued to complete its successive rotations in less and less intervals of time, affording thereby a clear indication that its proper motion was constantly increasing. This circumstance, he remarks, may naturally be explained by supposing that a considerable time necessarily elapses before the spot can acquire the full velocity of the wind by which it is impelled. It is evident, from the amount of discordance exhibited by a comparison of the different periods of rotation derived from the movements of the spots, that the currents in the atmosphere of the planet must possess very considerable velocities. Sir William Herschel found, from his observations, that the spots, on one occasion, must have been transported through 60° in twenty-two rotations of the planet. This would indicate that the wind on the surface of the planet was travelling at the rate of somewhat more than 200 miles in an hour, a velocity which, however, does not exceed that of occasional winds on the surface of the earth. The nearest approach to the velocity of the wind will of course be indicated by the smallest period of rotation. In 1692, Cassini observed a spot near the equator of the planet, which performed a complete rotation in $9^h\ 50^m$ [*]. Assuming (which is certain to be true) that the period of the planet's real rotation lies between $9^h\ 55^m$ and $9^h\ 56^m$ [†], this would indicate that the current in the atmosphere of the planet was travelling at the rate of about 250 miles in an hour. In general the spots complete their rotations in periods varying between $9^h\ 50^m$ and $9^h\ 56^m$, whence it may be concluded that the currents in the atmosphere do not possess a velocity exceeding the hundredth part of the rotatory motion of the planet.

Although the spots on Jupiter, by their transient existence and variable motion, generally indicate that they are of atmospheric origin, there are some of a more permanent character, whose frequent re-appearance in the same position can only be explained by supposing them to adhere to the surface of the planet. The most remarkable of these is the spot situate close to the northern margin of the southern belt, by watching the apparent motion of which Cassini originally succeeded in determining the period of the planet's rotation. From its first discovery in 1665 down to 1713, it vanished and re-appeared no less than nine times. When the southern belt disappeared, the spot disappeared also, but the belt was frequently perceived without the spot. On no occasion did the spot continue visible so long as three years*. Its disappearance is manifestly dependent on the same cause which determines the invisibility of the belt. Cassini supposed the belts to indicate currents in the seas of the planets, analogous to those which occur in different regions of the earth†; but this hypothesis affords no explanation of the dingy colour which the belts usually exhibit. A more probable hypothesis of their real nature has been suggested by Sir William Herschel, who considers the light of the bright regions of the planet to arise from dense accumulations of vapours suspended in its atmosphere, while the dark belts, on the other hand, indicate the opaque surface of the planet, which possesses an inferior power of reflecting the solar light, and admits of being seen through those regions of the atmosphere that are comparatively free of clouds‡. This hypothesis affords a natural explanation of the simultaneous disappearance of the southern belt and the spot adjacent to it, for the vapours which close in upon each side of the belt and cover the surface of the planet must necessarily submerge the spot at the same time, and thereby render it invisible also.

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[*] Mém. Acad. des Sciences, tome x., p. 8. Élémens d'Astronomie, p. 406.

[†] It appears, from the researches of Cassini, Schroeter, and Airy, that the period of rotation is included between $9^h\ 56^m$ and $9^h\ 55^m$. Independently of the results arrived at by the two last-mentioned astronomers, it is evident, from observations of the permanent spot originally discovered by Cassini, on the occasions of its successive re-appearances between 1665 and 1713, that the period of rotation is somewhat less than $9^h\ 56^m$. In 1672, Cassini, by means of it, made the period of rotation $9^h\ 55^m\ 50^s$. In 1708, Maraldi deduced from it a period equal to $9^h\ 56^m\ 48^s$, and from observations of it in 1713 he made the period equal to $9^h\ 56^m$. On the last-mentioned occasion he compared Cassini's original observations of the spot in 1665 with his own observations in 1713. Assuming the period of rotation to be $9^h\ 56^m$, he found that in 1713 the actual position of the spot was about 36° in advance of the computed position. This would afford a valuable means of determining the period of rotation, if we could be assured of the precise number of complete rotations embraced between the observations of 1665 and 1713; but this is an uncertain datum, on account of the magnitude of the probable error of the element to be determined.

In a former part of this work, we have given a detailed account of the researches of astronomers on the motions of Jupiter's satellites. It is remarkable that, down to the middle of the seventeenth century, there were persons who refused to believe that these bodies actually circulated round the planet. In 1665 Cassini announced his discovery of two phenomena which furnished incontestable proofs that the satellites conformed in this respect to the analogy offered by the motion of the terrestrial satellite. By means of the powerful glasses of Campani, he succeeded in perceiving the satellites when, in the course of their orbitual motion, they were interposed between the planet and the earth. On such occasions, they exhibited the appearance of small spots on the disk of the planet; but he shewed that, from their motion near the margin being the same as on the centre, and from their apparent magnitudes being constant during the time they were visible, they could not be attached to the surface of the planet. He also discovered the shadows which the satellites projected upon the body of the planet while in the act of passing between it and the sun. In order to convince the most sceptical of the real nature of these appearances, he announced, beforehand, the days and the hours in the months of August and September of the year above-mentioned, when the bodies of the satellites and also their dark shadows would be seen upon the disk of the planet§. These predictions having been verified by observation, no doubt could henceforth exist in any mind that the satellites revolve round the planet in the same manner as the moon revolves round the earth.

In 1707, Maraldi I. discovered that the fourth satellite, on the occasion of passing between the planet and the earth, sometimes appeared brighter than the planet, while at other times it resembled a black spot upon the disk. He further discovered that in the latter case the magnitude of the satellite was frequently less than that of its shadow, which was at the same time visible on the disk, although, according to the laws of optics, it ought to have been greater. A similar appearance was witnessed by him in 1713. The conclusion he drew from it was, that there are dark tracts of great extent on the surface of the satellite, which are incapable of reflecting the light of the sun. It may be remarked that the question with respect to the real nature of these spots admitted of two distinct explanations. If the satellite was supposed to perform a complete rotation on an axis, in the precise time which it took to revolve round the planet, it would invariably turn the same hemisphere to the observer on the occasion of each transit; and if the spots adhered to the surface, it ought always to exhibit the same appearance on the disk of the planet. The variation of brightness, on this hypothesis, indicated that the spots were not of a permanent nature, but were merely transient phenomena, arising probably from physical changes occurring on the surface of the satellite. On the other hand, if the motions of rotation and revolution were not equal, the satellite might turn a different hemisphere to the observer on the occasion of each transit, and the variations in its appearance might be accounted for by supposing that there existed permanent tracts of great extent on its surface which reflected only a small portion of the solar light which fell upon them. Sir William Herschel has shewn, from his observations, that the periods of rotation and revolution of the satellite are equal, whence it follows that the spots cannot be permanently attached to its surface. They must, therefore, arise from changes occasionally occurring either on the surface of the satellite or in its atmosphere. The other satellites have been found to exhibit appearances similar to those of the fourth *, and as, in the case of each of them also, the periods of rotation and revolution are equal, a similar conclusion is equally applicable to them. The interesting fact, that each of the four satellites performs a complete rotation on its axis in a period equal to that of its revolution round the planet, was deduced by Sir William Herschel, from a comparison of the relative quantities of light emitted by the satellites in different parts of their orbits. He discovered that all the satellites were subject to great variations of brightness in the course of their revolutions round the planet, but, having carefully watched the appearance of each satellite throughout a great number of revolutions, he invariably found that when it arrived in the same position of its orbit it exhibited the same degree of brightness †. The obvious conclusion was, that the rotatory motion of each satellite was equal to its orbitual motion round the planet. This curious fact had been already established in the case of the moon and the fifth satellite of Saturn, and there is good reason to suppose that it forms an essential characteristic of the movements of all the secondary bodies of the planetary system.

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* For a curious account of the circumstances attending the successive re-appearances of this spot see—*Anc. Mém. Acad. des Sciences*, tome ii., p. 105, tome x., pp. 1, 513; *Mém. Acad. des Sciences*, 1708, 1713; also Cassini's *Elémens d'Astronomie*, p. 405, et seq.

† Anc. Mém. Acad. des Sciences, tome ii., p. 106.

‡ Phil. Trans., 1793, p. 218.

§ Journal des Savans, February 21, 1666; see also Phil. Trans., No. 10, p. 171.

The determination of the magnitudes of the satellites has formed an interesting object of research to astronomers, but it is attended with great difficulty, in consequence of the extreme minuteness of the bodies,

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* Maraldi found that the third satellite, on the occasions of its transit across the disk of the planet, exhibited variations similar to those which he remarked in the fourth satellite (*Mém. Acad. des Sciences*, 1707, p. 193). According to that astronomer, Cassini had previously noticed similar variations in all the satellites. The observations of Cassini and Maraldi were confirmed soon afterwards by those of Pound (*Phil. Trans.*, 1719, p. 902). Bianchini was the first person who discovered that the satellites are subject to great variations of brightness in other parts of their orbits, as well as in conjunction. He remarked that the fourth satellite sometimes became so small as to be almost invisible (*Mém. Acad des Sciences*, 1714, p. 32).

† Phil. Trans., 1797, Part II., p. 332, et seq.

and their immense distances from the earth. It is only when viewed with the exquisite telescopes of the present day that they have finally exhibited real disks of appreciable magnitude, and consequently it was impossible on any previous occasion to ascertain their apparent diameters by direct measurement. In 1734, Maraldi II. attempted to determine their magnitudes by noting the times which they occupied in entering upon the disk of the planet. In this manner, he found that the linear diameter of the third satellite amounted to $\frac{1}{18}$ of that of Jupiter, and the diameters of the other three satellites to about $\frac{1}{20}$ of the same unit*. Now Jupiter, at his mean distance from the sun, appears under an angle equal to 36″.7. Hence, according to Maraldi, the apparent diameters of the satellites, when viewed at the same distance, would be 2″ for the third satellite, and 1″.8 for the three others. These results considerably exceed the true magnitudes of the angles as determined by micrometrical measurement. In 1738, Whiston gave a statement of the magnitudes of the satellites, but he does not mention upon what grounds it is founded. He estimated the third satellite to be equal in size to the Earth or Venus; the first to be equal to Mars; the second to be somewhat larger than Mercury; and the fourth only a little larger than the Moon†. Bailly attempted to determine the magnitudes of the first three satellites by noting the times which they occupied in entering into the shadow of the planet, or in emerging from it‡. This method is not susceptible of great precision, and consequently the results obtained by that astronomer were not regarded as worthy of any confidence. Lalande subsequently determined the magnitude of the fourth satellite by the same method§. Sir William Herschel determined the magnitude of the second satellite by noting the time which it took to enter upon the disk of the planet. Having found, from observations of the transit which took place on the 28th of July, 1794, that an interval of four minutes elapsed between the instant of the satellite's exterior contact with the margin of the planet, and that of its complete projection on the disk, he hence determined by calculation that at the mean distance of Jupiter from the sun it would subtend an angle equal to 0″.87‖. He did not attempt to determine the magnitudes of the other satellites by the same method of strict calculation, but, from a nice estimation of the relative magnitudes of the four bodies, he concluded that the third satellite is considerably larger than any of the others; that the first is nearly of the size of the fourth; and that the second is a little smaller than either of the two latter, or the smallest of all. Schroeter and Harding determined the magnitudes of the four satellites by means of observations of their transits over the disk of the planet. According to the results at which they arrived, the apparent diameters of the four satellites, when viewed from the earth, at the mean distance of the planet, are severally 1″.063, 0″.870, 1″.543, and 1″.074, commencing with the first satellite, and proceeding in the order of their distances from the planet¶. The result for the second satellite agrees very nearly with that deduced by Sir William Herschel.

In 1826, M. Struve determined the magnitudes of the satellites by micrometrical measurements of their disks with the great Dorpat refractor. The following are the apparent diameters which they would severally exhibit, supposing them to be viewed at the mean distance of the planet from the earth *:—

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* Mém. Acad. des Sciences, 1734.

† "The Longitude discovered by means of Jupiter's Satellites," London, 1738, p. 7.

‡ Mém. Acad. des Sciences, 1771. § Ibid., 1774.

‖ Phil. Trans., 1797. Part II., p. 350. ¶ Mem. Ast. Soc., vol. iii., p. 301.

Apparent diameter of	1st	satellite	1″.015
,, ,,	2nd	,,	0 .911
,, ,,	3rd	,,	1 .488
,, ,,	4th	,,	1 .273

These results present a very satisfactory accordance with those which Schroeter and Harding obtained by a less direct method. They also harmonize admirably with the estimations which Sir William Herschel formed of the relative magnitudes of the four bodies. The coincidence in the last-mentioned instance is especially valuable, inasmuch as it shews the degree of confidence that may be placed in the *apperçus* of Herschel, with respect to many delicate points in the science of astronomy, which rest mainly on the sagacity of that great observer. From the above measures of M. Struve it is easy to infer that the linear diameters of the four satellites, commencing with the first and proceeding outwards, are severally 2429 miles, 2180 miles, 3561 miles, and 3046 miles. It appears, therefore, that the third satellite exceeds Mercury in magnitude by a small quantity, and that the second is about the size of the Moon. The first and fourth satellites of course occupy, in respect of magnitude, an intermediate position between these two bodies.

Saturn, when viewed with the telescope, exhibits obscure bands, disposed in parallel directions across the centre of his disk, analagous to those which encompass the body of Jupiter, though not so well defined. These bands were first observed by Dominique Cassini, but they appeared so faint and uniform that he was unable to draw any conclusions from them. In the year 1715, when the ring had totally disappeared, J. Cassini examined the planet with a telescope of 114 feet focal length, and perceived two broad bands exactly parallel to the plane of the ring, and extending over a considerable portion of the disk. He searched in vain for a spot which, by observations of its position at different intervals, might indicate a rotatory motion; but, from the circumstance of all the satellites, except the most distant, revolving in the plane of the ring, he shrewdly suspected that the planet revolved about an axis perpendicular to that plane†. It was reserved for Sir William Herschel to establish this fact by observation, and also to assign the period of rotation. He commenced his examination of the planet in the year 1775. By means of his powerful telescopes, he discovered that the belts were in a state of constant change, and from their arrangement, which was generally parallel to the ring‡, he inferred the probability of the planet revolving round an axis perpendicular to the plane of the ring. His suspicion of a rotatory motion was confirmed upon finding that a spot which he discovered on the 19th of June, 1780, occupied different positions of the disk on each of the two following evenings§. On the 11th of November, 1793, having examined the planet with his seven-feet reflector, he perceived a bright and uniform belt situated a little to the south of the shadow of the ring. Close to it was a darker belt, in which were two narrow divisions, so that the planet appeared to be encompassed by a quintuple belt, composed of three dark and two bright belts. By watching the variations in the appearance of this belt he discovered that the planet revolves round an axis perpendicular to the plane of the ring in the short period of $10^h\ 16^m\ 0^s.44$[*]. The observations by means of which he arrived at this interesting conclusion extended from December 4, 1793, to January 16, 1794, and included 100 rotations of the planet. It is worthy of remark, that Huyghens inferred, from the shortness of the period of the satellite discovered by him, that the planet had a rapid rotation on an axis. We have already mentioned that Kepler had previously hazarded a similar conjecture with respect to Jupiter.

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* Mem. Ast. Soc., vol. iii., p. 301.

† Mém. Acad. des Sciences, 1715.

‡ In the course of his earlier observations of the planet, Sir William Herschel occasionally discovered belts which deviated sensibly in direction from the plane of the ring. Cassini had in some instances observed belts on Jupiter's disk disposed in a similar manner. (*Anc. Mém. Acad. des Sciences*, tom. x., p. 683.) In the latter case they were generally the precursors of extensive changes in the arrangement and appearance of the equatorial belts. It is very reasonable to suppose that these anomalous belts are the indications of tempestuous movements in the atmospheres of both planets.

§ Phil. Trans., 1790, Part I., p. 16.

The discovery that Saturn is compressed at the poles, is also due to Sir William Herschel. In 1782, he determined the lengths of the equatorial and polar axes of the planet, by micrometrical measurement with his 20-feet reflector. He found the apparent equatorial diameter to be 22″.81, and the apparent polar diameter to be 20″.61, whence it followed that the two diameters were to each other nearly as 11 to 10[†]. The precise ellipticity indicated by these measures was equal to 0.09645. So far the results of observation presented a satisfactory accordance with those derived from mechanical principles, and Saturn appeared to conform in this respect to the analogy of the other planets. In 1805, however, the same astronomer announced the existence of an unexpected irregularity in the figure of the planet. The equatorial diameter appeared to exceed the polar, agreeably to his previous observations, but he now found that the longest diameter occupied an intermediate position between them, so that the planet, in consequence, exhibited the form of a quadrangle with the four corners rounded off. He determined by actual measurement that the three diameters were as 36, 35, 32, and that the longest diameter was inclined to the plane of the equator at an angle of 43° 20[‡]. The observations of the following year convinced him that he had made the equatorial diameter too short, and he finally adopted 36, 35.41, and 32, as the numbers expressing the relative lengths of the three diameters. By this revisal the anomaly in the figure of the planet was to some extent diminished. It is worthy of remark, also, that the ellipticity assigned by the new values of the equatorial and polar diameters coincides with that deducible from the measures of 1789. This circumstance is the more satisfactory, as the original determination of the ellipticity by the illustrious astronomer has been found to accord exactly with that derived from the most recent micrometrical measures of the axes of the planet[§].

The irregularity in the figure of Saturn, to which allusion has just been made, was regarded by astronomers as an inexplicable anomaly in the planetary system. Herschel ascribed it to the attraction of the ring. It was found, however, upon investigation, that the ring was not capable of producing an effect similar to that indicated by the observations of Herschel. The subject of the figure of the planet, therefore, excited a lively interest, more especially on account of the high authority upon which the alleged irregularity rested. In 1832, Bessel resolved to ascertain the figure of the planet by direct measurement. He had been led to suspect that the observations of Herschel were illusive—in the first place, because they were at variance with the results of theory, and, in the second place, because they were made under unfavourable circumstances, the contour of the disk having been broken by the ring, which, in 1805, was very open. By careful micrometrical measurement, he found that the apparent magnitude of the equatorial diameter of the planet at the mean distance from the earth, was 17″.053, and that the apparent magnitude of the polar diameter was 15″.381 *. These results gave 0.098047 for the ellipticity, upon the supposition that the figure of the planet was an oblate spheroid. We have seen that Herschel made it 0.09645. The two results do not therefore differ by so much as $\frac{1}{500}$ of either. In order to ascertain the real figure of the planet, Bessel in the first instance assumed that it was an oblate spheroid whose ellipticity was equal to that indicated by the ratio of the two axes, and having calculated, upon this supposition, the lengths of several diameters inclined at different angles to the plane of the equator, he compared them with the corresponding results obtained by direct measurement. A complete accordance was found to subsist between the calculated and measured diameters of the planet, consequently no doubt could henceforth exist that the figure of the latter is in reality that of an oblate spheroid. The following are the results at which Bessel arrived :—

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[*] Phil. Trans., 1794, p. 62. [†] Ibid., 1790, Part I., p. 17. [‡] Ibid., 1805, p. 280.

[§] In every work which the author has seen containing any allusion to the observations of Herschel on the figure of Saturn, it is stated that the diametral values 36, 35, and 32 were the *final* results at which he arrived. The material revisal mentioned in the text appears to have been totally lost sight of, although nothing can be more distinct than the language of the illustrious astronomer on this point. The following are his express words on the occasion:—"The equatorial diameter of my last year's figure is, however, a very little too short; it should have been to the polar diameter as 35.41 to 32, which is the proportion that was ascertained in 1789, from which I have hitherto found no reason to

Distance from the Equator.		Measured Diameter.	Calculated Diameter.
0°	0′	17″.139	17″.053
22	30	16 .679	16 .777
45	0	16 .242	16 .160
67	30	15 .605	15 .607
90	0	15 .332	15 .394

It is impossible to contemplate these numbers without a feeling of admiration of the theory which is capable of responding so faithfully to the requirements of nature, and of the exquisite skill displayed by the illustrious astronomer who executed measures so singularly delicate as those above given with a success apparently so complete.

Sir William Herschel was induced to suspect from his observations that Saturn is surrounded by an atmosphere of considerable extent. Having assiduously observed the belts of the planet during the period included between the years 1775 and 1780, he found them to be subject to incessant variations, analogous to those which characterize the fluctuating movements of an atmospheric fluid. The phenomena observed by him during the occultations of the satellites were also favourable to the same hypothesis. He found in each instance, that the satellite, after coming up to the planet, continued to hang on its limb for some time before

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* See an account of Bessel's researches on this subject, translated from the original

it actually vanished. The seventh satellite was observed by him on one occasion to linger in contact with the disk for the space of 20 minutes. This would give 2″ for the amount of the horizontal refraction at the surface of the planet*. It would be hardly safe to draw any positive conclusions from so minute a result. Herschel did not fail to remark that the irradiation of the planet was not taken into account. He, however, discovered other phenomena which afforded more palpable indications of the existence of a Saturnian atmosphere. He perceived an occasional brilliancy in the surface of the planet around both the poles, analogous to that observed in the polar regions of Mars, and he found, as in the case of that planet, that the region around each pole diminished or increased in brightness, according as it was more or less exposed to the influence of the solar rays. In 1793, when the south pole had been long turned towards the sun, he perceived that the whole surface around it was of a pale whitish colour, far inferior in brightness to the ring, and not even equal in this respect to the white belts encompassing the equator of the planet. On the other hand, the surface around the north pole, which had been less exposed to the solar rays, was of a bright white colour. In 1806, the north pole having been for some time illumined by the sun, the regions around it had lost much of their lustre, but those around the south pole appeared to him to regain their former bright colour. The latter were now decidedly brighter than the equatorial regions of the planet, as was evident from the appearances they severally exhibited when viewed with the telescope. With a magnifying power of 527, the south polar regions continued to appear very white, while, on the contrary, the regions near the equator assumed a yellowish tinge†. Herschel suggests to future astronomers the prosecution of attentive observations of these interesting phenomena, with the view of establishing a definitive connexion between them and the vicissitudes of the Saturnian year. It is manifest, as in the case of Mars, that such appearances necessarily imply the existence of an atmosphere, whether they be supposed to arise from variations in the temperature of the planet, or from the fluctuating movements of vapours suspended above its surface.

Among the many objects which excited the admiration of Galileo when he first turned the telescope towards the heavens, there was none which filled his mind with such astonishment as the appearance presented by Saturn. Writing on the 13th of November, 1610, to Julian de Medicis, the Tuscan Ambassador at the Imperial Court, he announced to him having discovered, to his great admiration, that Saturn was not a single planet, but was composed of three bodies, which almost touched each other, and constantly maintained the same relative position. He remarked that the three bodies were arranged in the same straight line; that the middle body was the largest, and that the two others were situated respectively on the east and west sides of it. He added that, with telescopes of inferior power, the planet did not appear triple, but exhibited an oblong form, somewhat like the shape of an olive‡. The illustrious philosopher did not long continue his observations of the planet before he discovered that the lateral bodies did not constantly retain the same apparent magnitudes. In a letter to Castelli, dated the 30th of December, 1610, he mentioned that, since the month of July, the two bodies had been gradually diminishing, although they appeared to be immoveable, both with respect to each other and to the central body*. They continued to grow less and less during the course of the two following years, at the close of which they finally vanished altogether, leaving the planet quite round, like Jupiter. Galileo announced this mysterious disappearance of the phenomenon in his third letter to Welser, dated December 4, 1612. The astonishment which he felt on the occasion was mingled with a feeling of alarm lest the Aristotelians, taking advantage of his inability to adduce an adequate explanation of the cause of disappearance, should attempt to make his observations a subject of derision, by alleging them to have been destitute of any foundation in nature. "What," he remarks, "is to be said concerning so strange a metamorphosis? Are the two lesser stars consumed after the manner of the solar spots? Have they vanished and suddenly fled? Has Saturn perhaps devoured his own children? Or were the appearances indeed illusion or fraud, with which the glasses have so long deceived me, as well as many others, to whom I have shewn them? Now, perhaps, is the time come to revive the well nigh withered hopes of those who, guided by more profound contemplations, have discovered the fallacy of the new observations, and demonstrated the utter impossibility of their existence. I do not know what to say in a case so surprising, so unlooked for, and so novel. The shortness of the time, the unexpected nature of the event, the weakness of my understanding, and the fear of being mistaken, have greatly confounded me."†

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* Phil. Trans., 1790, Part I., p. 15. † Ibid., 1806, Part II., p. 464, et seq.

‡ Opere di Galileo, Edit. Pad., tom. ii., p. 41.

During the course of nearly half a century which elapsed after the invention of the telescope, the singular vicissitudes which the appendage of Saturn underwent, continued to form an inexplicable enigma to astronomers. The only real progress made towards its explanation consisted in arriving at more accurate views of the appearances exhibited by it in the telescope and in ascertaining the time which it occupied in passing through the cycle of its phases. Instruments superior to those of Galileo shewed that the objects composing the appendage were not round like the other celestial bodies, but rather resembled two luminous crescents, attached by their cusps to the planet, and forming, as it were, two handles to it. The conformation of these handles, or *ansæ*, was found to be subject to a slow alteration, which caused the appearance of the planet to be constantly changing. When the ansæ first became visible, after the planet had passed through its round form, they appeared like two arms extending on each side of the planet, but they continued gradually to open out during the following seven or eight years, at the close of which they again began to contract, and after an equal lapse of time, vanished entirely, when the planet again assumed its round form. The appendage therefore completed the cycle of its various appearances in about fifteen years, or half the period of the planet's revolution round the sun. It was reserved for Huyghens to discover the true cause of these singular changes. He first communicated to the world his explanation of them, under the form of an enigma, in a small tract, entitled, "De Saturni Luna Observatio Nova," published in 1656; but he subsequently gave an explicit announcement of the real nature of the appendage of the planet, with a detailed exposition of its theory, in a book written expressly on the subject which appeared in the year 1659‡. In this work, before proceeding to explain his own theory, he considers the various hypotheses which had been advanced by astronomers with a view to account for the appearances of the planet, and he points out the insuperable difficulties by which each of them is accompanied. Roberval, it must be acknowledged, approached more nearly to the true explanation of the phenomenon than any of his contemporaries. He supposed that vapours capable of reflecting the rays of the sun escaped from the torrid regions of Saturn, and, after attaining a certain elevation, remained in a state of suspension around his equator. When they filled the whole space included between the surface of the planet and the region of their highest elevation, they caused the planet to assume an elliptic form. When they ascended in less abundance, a wide space was left between them and the planet, and they only reflected the solar light in the parts that were densest, which, relatively to an observer at the earth, were the parts most remote from the centre of the planet. In this case the appendage would resemble two ansæ with obscure spaces between them. When no vapours ascended, the planet would appear quite round *. Huyghens objected to this hypothesis that it made the exhalation of vapours to depend upon the position of the planet in its orbit (since the round phase occurred when the planet was in two opposite points of the ecliptic, and the opening of the ansæ was greatest when it was about 90° distant from either), while at the same time it assigned no cause why the vapours should cease in some parts of the orbit, and in other parts should ascend in great abundance. With respect to the alleged elliptic form of the planet, he had already shewn that it did not exist, being in fact an illusive appearance which the planet assumed when it was observed with telescopes incapable of exhibiting the dark spaces between the ansæ. It was therefore a superfluous task to assign its cause.

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* Opere di Galileo, Edit. Pad., tom. ii., p. 46. † Ibid., p. 152.

‡ Systema Saturnium, 4to. Hagæ, 1659.

Huyghens introduces his own explanation of the phenomenon by remarking that all the primary bodies of the planetary system, beyond doubt, revolve about fixed axes. He further considers it to be established by observation, that the rotatory motion of each primary is more rapid than the orbital motion of any of the smaller bodies circulating round it, and he hence concludes that Saturn and his appendage revolve with a rapid velocity round an axis. In 1655, while he was engaged in pondering upon this subject, the planet exhibited an appearance which strongly

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* Delambre appears to have committed a slight inadvertence in his account of Roberval's theory as explained by Huyghens. The following are the terms in which he notices it:—"Roberval avait imaginé que Saturne etait rond comme les autres planètes, mais que de son équateur il s'échappait des vapeurs, qui demeuraient suspendues à certain distance et formaient autour de la planète un cercle qui, vu par nous obliquement, devait se montrer comme une ellipse."—*Hist. Ast. Mod.*, tome ii., p. 564. It will be seen, however, by reference to the original, that Huyghens makes no allusion whatever to the principle of perspective, that a circle exhibits the form of an ellipse when viewed obliquely with respect to the visual ray. The figure which Roberval ascribes to the planet is a real not an apparent ellipse, being occasioned by the absence of vapours in the polar regions of the planet, in the same manner as the flattening at the poles of Jupiter causes the disk of that planet to assume the form of an ellipse. But although the French astronomer does not appear to have recognized clearly that the opening out and closing of the ansæ might be explained by the variable obliquity of the vaporous zone with respect to the visual ray, he very sagaciously conjectured, by the aid of considerations identical with those which conducted Huyghens to a similar conclusion, that the appendage must encompass the planet equally in every direction. This is candidly acknowledged by Huyghens himself (See *Systema Saturnium*, p. 42, *Opera Varia*, vol. ii., p. 562). There was a good deal of truth in Roberval's views, but it is clear that he wanted the vigour of mind necessary to correct his theory by instituting a close comparison between its results and the recorded observations of astronomers.

attracted his attention. The appendage resembled two arms extending in the same right line on opposite sides of the planet, as if an axis had passed through its centre. As the appearance of this axis was constantly the same, when observed from day to day, he concluded that the appendage must encompass the planet similarly in every direction so as to assume the form of a ring; for it was manifest that the rapid rotation of the planet and its appendage was incompatible with any other supposition. On the other hand, if this be once admitted, the appendage obviously would exhibit the same appearance whatever might be the velocity of rotation. When he proceeded to compare the various appearances of which such a ring is susceptible with the recorded observations of the phases of the planet, he soon found that they could not be reconciled without supposing the plane of the ring to be inclined to the ecliptic at a constant angle, similarly to the plane of the terrestrial equator. The existence of the ansæ indicated also to him that the ring did not adhere to the surface of the planet, but was everywhere separated from it by an interval of equal breadth. Assuming these particulars with respect to the ring, he found that all the appearances of the planet might be satisfactorily accounted for by means of such an appendage. The logogriphe containing his explanation of the phenomenon was published by him in the following form :—

aaaaaaa ccccc d eeeee g h iiiiiii llll mm nnnnnnnnn oooo pp q rr s ttttt uuuuu

He now restored the letters to their original places, when they stood thus :—

annulo cingitur, tenui plano, nusquam cohærente, ad eclipticam inclinato;

or, in other words, "the planet is surrounded by a slender flat ring, everywhere distinct from its surface, and inclined to the ecliptic." Nothing can be more convincing or beautiful than the explanation which this theory affords of the various phenomena presented by the planet. When the position of the planet in its orbit is such that the plane of the ring passes through the sun, the edge only of the ring is exposed to the solar rays, and the extent of the illuminated surface being very small, it is incapable of producing a sensible impression on the visual organ. In this position, then, the ring is invisible, and the planet presents a round appearance, like the sun or full moon. The ring also disappears when its plane passes through the earth, for, although one of its sides may then be illuminated by the sun, it is only the edge which is turned towards the observer. Besides these two causes of disappearance, which are of a transient nature, and render the ring invisible only for a few days at most, there is a third cause which generally continues in operation during a longer period of time, and produces a more lasting effect. When the planet is so situated that the plane of the ring passes between the earth and the sun, the unenlightened side of the ring is then turned towards the earth, and consequently during the whole time that the planet is in this position (which frequently extends to several months) the ring will be invisible. The same theory affords an equally satisfactory account of the different phases assumed by the appendage of the planet during the period of its visibility. It is manifest that when the plane of the ring passes through the sun, and when consequently the ring ceases to be visible, the planet, if viewed from the sun, would appear in the node of the ring. When the planet revolves from this position ring, and the latter in consequence becomes visible in the form of a very elongated ellipse, gradually opening out in breadth. The ellipse continues to approach towards a circular form, until the planet has reached a distance of 90° from the node of the ring, when the elevation of the sun above the plane of the ring has attained its maximum. The ring henceforth begins to contract, and the same succession of appearances, in a reverse order, will obviously ensue, as the planet revolves towards the opposite node, where the ring again will cease to be visible. The ring will, therefore, complete the cycle of its phases in a period equal to half a revolution of the planet round the sun, or in about fifteen years. The appearances will not be materially different whether the ring be viewed from the earth or the sun, except during the time that the planet is in the vicinity of either of the nodes of the ring. At such a juncture the combined motions of the earth and the planet may cause the plane of the ring to pass more than once through the earth, and the ring may, in consequence, disappear and re-appear twice, before its plane has entirely swept over the terrestrial orbit.

Huyghens predicted that the planet would appear round in the month of July or August, 1671. Cassini, in fact, found that the ring totally disappeared towards the end of May, in that year. The coincidence was sufficiently satisfactory, considering that the position of the node of the ring, upon which the times of the round phase of the planet depends, could not possibly have been determined with a great degree of accuracy.

The position of the ring is usually determined by the inclination of its plane to the ecliptic and the longitude of its ascending node. If the plane of the ring was perpendicular to the ecliptic, the ring would appear like a luminous line at the node, and would thence gradually open out, until it finally assumed the form of a circle at a distance of 90° from the node. Now it appears, from observation, that the minor axis of the ellipse, which the form of the ring usually exhibits, does not become equal to the major axis, even when the opening of the ring is greatest. The obvious conclusion, therefore, is that the plane of the ring is not perpendicular to the ecliptic, but is inclined to it at a sensible angle. It is easy to shew, from the principles of perspective, that when the planet is at a distance of 90° from the node of the ring, the major axis of the ellipse is to the minor axis as radius to the sine of the angle contained between the plane of the ring and the visual ray. Hence by taking into account the elevation of Saturn above the plane of the ecliptic, upon which, to a certain extent, the direction of the visual ray depends, it is easy to determine the inclination of the plane of the ring to the ecliptic. Huyghens originally supposed that the plane of the ring was parallel to the equator, and hence inferred that its inclination to the plane of the ecliptic was 23° 30′. In 1668, however, Picard and himself found, by measurements made with a telescope of 21 feet focal length, that the inclination was 31° *. This was a nearer approximation to the true value than any other which astronomers had arrived at previous to the commencement of the present century. The longitude of the node of the ring is determined by the position of the planet relative to the sun or the earth, during the disappearance or reappearance of the ring; for it is manifest that when the sun or the earth passes through the plane of the ring, the heliocentric longitude of the planet in the one case, and the geocentric longitude in the other, are equal to the longitude of the node of the ring.

Huyghens first fixed the ascending node of the ring in 153° of longitude, and afterwards in 170° 30′. The elder Maraldi was the first astronomer who gave a complete theory of the method for determining the elements of the ring. From observations of the passage of 1715, he found that the inclination of the plane of the ring was 31° 20′, and that the longitude of the ascending node was 166° 17′ *. Lalande adopted Maraldi's value of the inclination, and by means of observations of the passage of 1774, he determined the longitude of the ascending node to be 167° 5′ †. This result he found to differ from Maraldi's only by 1′, taking into account the effect of precession which he estimated at 49′ for the interval of 59 years which had elapsed between 1715 and 1774. He therefore inferred that the position of the node of the ring is invariable. The researches of subsequent astronomers have shewn, however, that this was an erroneous conclusion.

In 1811, when the ring was very open, Bessel measured the axes of the ellipse, formed by the ansæ, and hence determined the inclination of the ring to be 28° 34′.1. This evaluation differed considerably from Maraldi's, which had been hitherto adopted by astronomers. It received a strong confirmation, however, from the subsequent measures of M. Struve. In 1826, when the planet was at a distance of 90° from the node of the ring, and when, in consequence, the opening of the ring was again at its maximum, that eminent astronomer executed a series of micrometrical measures of the axes of the ring with Fraunhofer's great refractor, and by means of them he determined the inclination of the ring to be 28° 5′.9. He also estimated that the probable error of this result did not exceed 6′.4 ‡.

The value assigned by Bessel to the inclination of the ring was deduced from measures executed with a micrometer attached to a 19-inch telescope, by Dollond. At a subsequent period, having obtained possession of a splendid heliometer, he resolved to employ it in determining the elements of the ring with greater precision. In pursuance of this design he executed a great number of micrometrical measures of the apparent position of the ring during the period embraced between the years 1830 and 1834. He also introduced into his investigation all the recorded disappearances and re-appearances of the ring, from the passage of the planet through the node of the ring in 1701, down to the passage of 1832. By a comprehensive and masterly treatment of the subject, he finally arrived at the conclusion that the longitude of the ascending node of the ring in the year 1800 was 166° 53′ 8″.9, and that the inclination of its plane to the ecliptic was 28° 10′ 44″.7. He also found that the node of the ring retreats upon the plane of the ecliptic at the rate of 46″.462 annually §. The result which he obtained for the inclination of the ring agrees with that deduced by M. Struve within the limits of error assigned by the latter astronomer. It may, therefore, be regarded as the most accurate determination of that element which has yet been arrived at.

In 1675 Cassini, having observed Saturn after his emergence from the rays of the sun, discovered that the ring was divided into two parts by a black band, so that it appeared to be composed of two concentric rings ||. This fact was confirmed by subsequent observations of the planet in different parts of its orbit, from which it appeared that the band was constantly

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* Mém. Acad. des Sciences, 1715.

† Ibid., 1774.

‡ Mem. Ast. Soc., vol. ii., p. 583.

§ Connaissance des Temps, 1838.

visible, and that its position on the northern side of the ring corresponded exactly to its position on the southern side[*]. Sir Willliam Herschel, without being aware of the reasons which induced Cassini and Maraldi to suppose an actual division of the ring into two smaller concentric rings, undertook a most searching examination of the appearance of the dark band, with the view of deciding this delicate point. Various facts were detected by this great observer which afforded unequivocal indications of the duplicity of the ring. He found that the band was of the same colour as the space between the ring and the planet, and was equally well defined on both its borders. It also constantly exhibited the same breadth, colour, and sharpness of outline throughout the whole period of ten years, during which he observed the northern side of the ring. The passage of the planet through the descending node of the ring in 1789 having rendered the southern side of the ring henceforth visible, he found that the band existed on it, as well as on the northern side, and exhibited similar characteristics. He also perceived, agreeably to the observations of preceding astronomers, that the divisions on both sides of the ring corresponded exactly to each other. It was impossible, therefore, to avoid the conclusion, that the dark band indicated a material division of the ring, and that in fact, the planet was surrounded by two concentric rings, separated from each other by a space, through which the open heavens were visible[†]. The observations of succeeding astronomers have afforded a complete confirmation of the justness of this conclusion. Some persons have even asserted that they have perceived a great number of concentric dark lines on the ring. This indication of the ring being divided into numerous parts was observed by Short and some of his contemporaries. In more recent times a similar phenomenon has been witnessed by De Vico, Encke, Lassel, and other observers. On the other hand Sir William Herschel and M. Struve, notwithstanding the high optical qualities of their telescopes have not discovered any traces of the existence of such a subdivision of the ring.

The determination of the dimensions of the ring has formed an interesting object of research to astronomers. Huyghens concluded, from his own observations, that the diameter of the ring was to that of the planet as 9 to 4[‡]. In 1719 Pound found, by means of a telescope of 123 feet focal length, that the two diameters were as 7 to 3[§]. In more recent times the micrometrical measurements of Sir William Herschel and M. Struve have led to an accurate knowledge of the magnitude of this stupendous zone. The following are the dimensions of the exterior and interior rings, as assigned by M. Struve:—

1.	Exterior diameter of the exterior ring	40″.095
2.	Inner diameter of the exterior ring	35″.289
3.	Exterior diameter of the interior ring	34″.475
4.	Interior diameter of the interior ring	26″.668
5.	Equatorial diameter of Saturn	17″.991
6.	Breadth of the exterior ring	2″.403
7.	Breadth of the division between the rings	0″.408
8.	Breadth of the interior ring	3″.903
9.	Distance of the interior ring from the ball	4″.339
10.	Equatorial radius of Saturn	8″.995

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[*] Mém. Acad. des Sciences, 1715, p. 13.

[†] Phil. Trans., 1792.

[‡] Systema Saturnium, p. 78.

[§] Phil. Trans., 1719.

These measures are adapted to the mean distance of the planet from the earth. We may form an estimate of the linear dimensions of the ring from the fact that an object situated at the same distance, in order to subtend an angle of only 1″, would require to have an absolute diameter of 4387 miles. It is easy hence to infer that the exterior ring will have an absolute diameter of 175,928 miles! The dimensions of the other parts will of course bear a similar relation to their apparent magnitudes.

Various circumstances concur to prove that the thickness of the ring must be very inconsiderable. When its plane passes through the earth or the sun (on either of which occasions its edge only is capable of reflecting the solar rays to the observer) it has been generally found to disappear, even when the most powerful telescopes have been directed towards it. When it is about to vanish, or when it begins to re-appear, after being for some time invisible, it resembles an excessively-narrow luminous line, before and behind which the satellites are observed to pass in the course of their revolution round the primary. Being sometimes apparently situated on the ring, these bodies were employed by Sir William Herschel as standards of comparison whereby to estimate its thickness. On such occasions the satellite was invariably found to project on the opposite sides of the ring, whence it followed that the ring could not be so thick as the diameter of the satellite. On the 29th of August, 1789, (when only three days had elapsed since the plane of the ring passed through the earth,) having perceived the third satellite upon the ring, he concluded that the thickness of the ring was not equal to one-third of the diameter of the satellite[*]. He estimated the diameter of the latter to be less than 1″; consequently the thickness of the ring did not subtend an angle so great as 0″.3. If we suppose the planet to have been at its mean distance from the sun, this would indicate a thickness of 1462 miles. Even the seventh satellite, notwithstanding its extreme minuteness, was observed by Herschel to project upon opposite sides of the ring. When the edge of the ring was almost completely turned towards him, the satellite, to use his own beautiful comparison, appeared like a bead moving upon a thread. He was of opinion that the diameter of the satellite did not exceed a thousand miles. It was probable, therefore, that the ring was not more than a few hundred miles in thickness. The illustrious astronomer did not fail on this occasion to remark, that if the ring was surrounded by an atmosphere, the refraction which it would exercise upon the rays of light proceeding from the satellite to the observer, would cause an apparent projection of the satellite similar to that indicated by observation, even although its diameter considerably fell short of the thickness of the ring. It appears, however, from other considerations, that the projection of the satellite is in reality due to the extreme thinness of the ring. Schroeter, from observations of the breadth of the shadow which the ring threw upon the planet, when it was about to disappear, concluded that its apparent thickness at the mean distance of the planet amounted only to 0″.125[†]. This indicates a real thicknes of rather more than 500 miles. Sir John Herschel estimates the thickness not to exceed the half of this quantity. He is of opinion that if it even subtended an angle of 0″.05 the ring would have been visible when the planet was observed by him, with a 20-feet reflector, on the 29th of April, 1833[‡].

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[*] Phil. Trans., 1790, Pt. I., p. 6.

[†] Mem. Ast. Soc., vol. ii., p. 517.

[‡] Outlines of Astronomy, p. 315. Only three days had then elapsed since the plane of the ring passed through the earth

While engaged in observing the planet when the edge of the ring was turned towards him, Sir William Herschel perceived several lucid protuberances upon the ring, the positions of which could not be reconciled with the actual motion of any of the satellites. His suspicion that they were phenomena of a distinct character was strengthened by the fact that they never came off the ring and showed themselves as satellites. The question occurred to him whether they might not be the indications of an eighth satellite revolving between the seventh satellite and the planet. By comparing together the different positions of the brightest of the spots, he found that they might be accounted for, by supposing the spot to revolve round the planet in a period equal to $10^h\ 32^m\ 15^s.4$. Having then computed, by means of Kepler's third law, the distance at which a satellite would require to be placed, in order that it might accomplish a complete revolution round the planet in the same time, he found the result to be 17″.227. This distance would bring the satellite precisely upon the plane of the ring. It followed, therefore, either that the particles of the ring possessed sufficient mobility to allow the satellite to revolve through them, or that the variable positions of the lucid protuberances arose from a revolution of the ring itself around the planet. That the ring was not fluid, but, on the contrary, was composed of a substance as solid as the materials of the planet itself, appeared to him evident, from the sharp definition of its borders, the brilliancy of the light reflected by it, and the darkness of the shadow which it threw upon the body of the planet. It was impossible, therefore, to withhold the conclusion that the ring is endued with a rotatory motion round the planet, which it completes in the short period of $10^h\ 32^m\ 15^s.4$ [*]. This result presents a complete accordance with that which Laplace deduced about the same time, from an investigation of the mechanical conditions which are necessary to assure the stability of the ring. It must be acknowledged, however, that the observations of Schroeter and Harding, on the occasion of the disappearances of the ring in 1803, are in direct contradiction to a rotatory motion of the ring about the planet. These astronomers observed several lucid points upon the ring which continued immovable during a period of eight hours. When watched also from night to night, the points were always found to retain the same position [†]. A similar objection to the rotation of the ring has been indicated by the observations of Prof. Bond, of Cambridge, U.S., on the occasion of the disappearance of the ring in 1848 [‡]. Another fact has been established by the observations of astronomers which, although essentially explicable by the principles of mechanical science, is accompanied by a phenomenon totally at variance with the supposition of a rotatory motion of the ring. In 1826 M. Struve found that the ring is not concentrically situate with respect to the planet. It appeared from micrometrical measurements, that the distance of the outer edge of the ring from the body of the planet was equal to 11″.288 on the east side of the planet, and only to 11″.073 on the west side. The ring was, therefore, nearer to the west side of the planet than it was to the east side by 0″.215 [§]. That the ring should revolve eccentrically with respect to the planet, is a condition which accords admirably with the principles upon which its conservation depends; but the fact of its invariably deviating towards the *same side* of the planet cannot be reconciled with the supposition of a revolution of any kind whatever. Lastly, there is a circumstance attending the disappearances and re-appearances of the ansæ which is unfavourable to the existence of a rotatory motion of the ring. It has been frequently found on such occasions that one ansa alone was visible, and in by far the greater number of cases this was the western ansa. It is difficult to reconcile this fact with the rapid rotation assigned to the ring by Herschel, since, upon the supposition of such a movement, a complete alternation ought constantly to take place in the appearance of the ansæ at the close of every five hours. There are, indeed, some observations of this kind, such as those of Cassini and Maraldi to be noticed presently, which positively favour the hypothesis of a rotatory motion of the ring. The interesting observations of the Rev. W. R. Dawes, on the occasion of the passage of the plane of the ring across the terrestrial orbit, in 1848-9, also lead unequivocally to the same conclusion. The rotation of the ring can hardly, therefore, admit of any doubt, although there are some difficulties attending the subject which remain to be explained.

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[*] Phil. Trans., 1790, Pt. II., p. 480.

[†] Connaissance des Temps, 1808.

[‡] Month. Proc. Ast. Soc., December, 1849.

[§] Mem. Ast. Soc., vol. iii., p. 301.

Sir William Herschel first remarked that the light of the ring is brighter than that of the planet. With a high magnifying power the light of the planet assumed a yellowish tinge, while that of the ring still continued white *. By means of the superior brilliancy of the ring he was enabled to trace it in those parts where it crossed the luminous disk of the planet †. It has been already mentioned that Cassini found the interior ring to be brighter than the exterior ring. He compared the difference between the two rings in brightness to that which subsists between polished and unpolished silver ‡. The observations of succeeding astronomers lead to a similar conclusion. Sir William Herschel discovered that the interior ring gradually diminishes in brightness towards the inner edge, the light of which, according to him, does not exceed in intensity that of the dark equatorial belts of the planet §. A similar remark has been made by M. Struve, who considers that the inner edge of the interior ring is less sharply defined and less regular in its construction than the outer edge ||. In all calculations relative to the disappearance and re-appearance of the ring, it has been assumed by astronomers that the appendage of the planet is regular in its construction, and that it is bounded by parallel planes. Observations of the ansæ, however, when they are about to disappear, or when they first begin to re-appear after a period of invisibility, afford grounds for suspecting that the actual conformation of the ring is irreconcilable with such an hypothesis. In 1671, when the ring was about to disappear, the ansæ were observed by Cassini to contract considerably. This circumstance was ascribed by him to the inferior brightness of the exterior ring which caused it to disappear, while the interior ring was yet visible ¶. He remarked also that, on the same occasion, one of the ansæ was partially visible when no trace of the other could be discerned, but he found that the visible remnant *was not always on the same side of the planet* **. A similar phenomenon was witnessed by Maraldi. On the 9th of October, 1714, (six days previous to the passage of the plane of the ring through the earth,) that astronomer perceived that the ansæ were reduced to half their ordinary dimensions. The eastern ansa appeared also to be somewhat broader than the western.

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* Phil. Trans., 1790. Part I., p. 5.

† Ibid., 1805. Part II., p. 273.

‡ Mém. Acad. des Sciences, tome x., p. 583.

§ Phil. Trans., 1794, p. 53.

|| Mem. Ast. Soc., vol. ii., p. 517.

¶ Mém. Acad. des Sciences, tome x., p. 583.

On the 12th of the same month, the planet was attended by the western ansa only. Maraldi remarked that these anomalous appearances might be accounted for by supposing that the ring is not wholly situated in one plane, or that its surface is very irregular, and that the western ansa, which from either of these causes appeared fainter than the eastern on the 9th of October, was subsequently transported by the rotation of the ring, so as to occupy the eastern side of the planet on the 12th of the same month *. The observations of succeeding astronomers generally agree in indicating a similar inequality in the construction of the ring. On the 29th of November, 1743, Heinsius perceived that the eastern ansa was shorter than the western †. On the 6th of October, 1773, the planet was observed at Cadiz with the western ansa only ‡. On the 11th of January, 1774, Messier perceived that the eastern ansa was longer than the western §. On the 4th of January, 1803, Harding witnessed the re-appearance of the western ansa, while the eastern was yet totally invisible ||. On the 16th of June, 1803, Schroeter, with great difficulty, observed the western ansa; but the eastern had some time previously disappeared ¶. It would seem, from these observations, that the western ansa is in general more easily discernible than the eastern. This fact, although at variance with the alleged rotation of the ring, does not affect the question under consideration, namely, the irregularity of the surface of the ring, which is unequivocally indicated by all the observations. There is another circumstance which abundantly serves to prove that the bounding surfaces of the ring are not composed of parallel planes. If such were the case, it is manifest that the instants of the disappearance and re-appearance of the ring would be constantly regulated by the motion of the planet, and a determinate position of the plane of the ring. Bessel, however, found, by a strict analytical investigation, that the recorded observations of these phenomena were incompatible with the results derived from the supposition of such a plane **. It is impossible, therefore, to admit that the bounding sides of the ring are composed of parallel planes.

Sir William Herschel was induced to suspect, from his observations, that the ring is encompassed by an atmosphere. In 1807 he perceived that the two poles of the planet did not exhibit the same shape. There appeared a protuberance about the regions of the south pole which he had not witnessed on any previous occasion, while, on the other hand, the north polar regions retained their usual shape. By careful observation he satisfied himself that this appearance did not arise from any irregularity in the surface of the planet. It was, therefore, evidently an optical phenomenon, due to some cause which did not similarly affect the regions about both poles. In reference to this point, he remarked that the ring was differently situ-situated with respect to the opposite sides of the equator of Saturn, being before the planet in the southern hemisphere, and behind it in the northern. The rays of light proceeding from the south polar regions to the earth passed pretty close to the ring, while those issuing from the regions in the vicinity of the north pole traversed an open space remote from any external influence. Hence, if the ring be supposed to be surrounded by an atmosphere, it would refract the rays proceeding from the regions about the south pole, and would thereby

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* Mém. Acad. des Sciences, 1715, p. 12.

† Lalande, Astronomie, Art. 3373.

‡ Ibid.

§ Ibid.

|| Connaissance des Temps, 1808, p. 429.

occasion an apparent distortion of the figure of the planet similar to the observed protuberance *. Herschel was more especially inclined to adopt this explanation of the phenomenon, as he had previously been led to suspect the existence of an atmosphere about the ring, from the fact of the satellites projecting on each side of it during the period when it resembled a luminous line. The recent observations of astronomers go to support the same conclusion. It has been already mentioned that the ring is found to disappear when its plane passes either through the earth or the sun. To the greater number of observers it has also continued invisible, as might naturally be expected, during the whole time that the unenlightened side is turned towards the earth. It is a remarkable fact, however, that Sir William Herschel succeeded in perceiving the ring when its plane was interposed between the earth and the sun, and when consequently the bright side was turned away from him †. It was also repeatedly seen under similar circumstances, by the Rev. W. R. Dawes in this country, and by Mr. Bond in America, during the passage of its plane across the terrestrial orbit in 1848-9 ‡. Now, since it would seem that the edge of the ring is invisible when it is turned directly to the observer, it ought, *à fortiori*, to be invisible in every other position. Hence it follows, that the visibility of the ring, during the time that its plane was interposed between the earth and the sun, did not arise from the light reflected by its edge. The inference is therefore unavoidable, that the ring was visible by means of the surface which was turned away from the sun, for the opposite surface upon which the rays of that luminary directly fell, was then turned away from the earth. The observations of Mr. Dawes are especially conclusive with respect to this point. When the plane of the ring was interposed between the earth and the sun (on which occasion the bright side of the ring was turned away from the earth), the ring was still faintly visible; but as the earth continued to approach the plane, the ring was seen with greater difficulty, and when it was actually situated in the plane, the ring was invisible, even when the planet was viewed with Mr. Lassel's 20-feet reflector §. Now, if the visibility of the ring in the above instance was due to the light reflected by its edge, a succession of appearances the very reverse of that indicated by observation ought to have taken place; for as the earth approached the plane of the ring, the edge of the ring was turned more directly towards the observer, and therefore it ought to have been seen with increased facility. That the visibility of the ring, when its plane was interposed between the earth and the sun, was occasioned by light reflected from its obscure surface, and not from its edge, was still further proved by the observations of Mr. Dawes during the interval when the earth was receding from the plane of the ring; for, in this case, the ring became more and more distinctly visible, according as the elevation of the earth above the plane continued to increase.

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* Phil. Trans., 1808, p. 162.

† Phil. Trans., 1790, Part I., p. 8.

‡ When Saturn is proceeding from the ascending to the descending node of the ring, the north side of the ring is that which is enlightened by the sun; and on the contrary, when he is proceeding from the descending to the ascending node, it is the south side which is enlightened by that luminary. The planet having passed through the descending node of the ring on September 3, 1848, it follows that the north side of the ring, from being hitherto the illuminated side, became, henceforward, the dark side. Now, on the 22nd of April, 1848, the earth met the plane of the ring advancing in the opposite direction, and passed from the north to the south side of the ring. On the 12th of September, 1848, it overtook the same plane, and passed from the south to the north side of the ring. On the 18th of January, 1849, having again reversed its motion, it met the plane of the ring a second time, and passed from the north to the south side of the ring. It is easy to infer from these particulars, that throughout the whole period embraced between April 22, 1848, and January 18, 1849, the unenlightened side of the ring was turned towards the earth, with the exception of the few days that elapsed between the 3rd and 12th of September, 1849.

With respect to the physical explanation of the interesting appearance above alluded to, Sir William Herschel suggested that it might arise from the light reflected by the planet upon the dark surface of the ring. Mr. Dawes, however, considers that the quantity of light derived from this source would not suffice to render the ring so distinctly visible as the observations indicated, especially in those parts that were less exposed to the illuminated hemisphere of the planet. He proposes, therefore, to account for the phenomenon, by supposing the ring to be surrounded by an atmosphere which occasions a twilight sufficiently strong to render the ring visible, even after the sun has descended below the surface turned towards the earth. He remarks, in support of this explanation, that during the whole period embraced by his observations, the depression of the sun below the obscure surface of the ring, did not exceed 1°. It is manifest, therefore, that a twilight of considerable brightness might arise, even without the necessity of assigning a high degree of density to the atmosphere of the ring. This explanation was still further strengthened by the colour of the obscure surface of the ring, which appeared to Mr. Dawes to have a ruddy tinge, somewhat resembling the appearance of the western sky after sunset[*]. The observations of Mr. Dawes, and the interesting conclusion which he so reasonably draws from them, go to support the assertion of Sir William Herschel relative to the protuberance about the south pole of the planet, observed by him in the year 1807. The establishment of the elliptical figure of the planet by Bessel, and still more recently by the Rev. Mr. Main, of the Royal Observatory of Greenwich, cannot of course invalidate the existence of that apparent anomaly; since the observations of both these astronomers were made at a time when the atmosphere of the ring could exercise no optical influence on the figure of the planet[†].

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[*] Granting that the visibility of the ring, when its plane is interposed between the earth and the sun, is due to the twilight occasioned by a circumambient atmosphere, it is manifest that the effect so produced will be greatest when the sun is just depressed below the obscure surface of the ring, and the earth at the same time has attained its maximum elevation above that surface. From the position assigned to the ascending node of the ring by Bessel, it is easy to infer that this condition will be satisfied, if the passage of the planet through either node of the ring should take place on the 7th of June, or the 9th of December. A juncture of this kind occurred in the year 1832, the planet having passed through the ascending node of the ring about the beginning of December. On the other hand, the twilight will be viewed to least advantage if the planet should pass through the ascending node of the ring on the 7th of March, or through the descending node on the 9th of September. It appears, therefore, that the recent passage of the planet which took place on the 3rd of September, 1848, was unfavourable for witnessing this interesting phenomenon. In fact, at the very time when the sun was about to ascend to the level of the obscure surface of the ring, the earth had almost overtaken the plane of the ring, so that the twilight when strongest was viewed when the edge only of the ring was turned towards the observer. If the planet should pass through the ascending node of the ring on the 9th of September, or through the descending node on the 7th of March, there will be only one brief disappearance of the ring; but as the planet on either of such occasions will be in conjunction with the sun, it will be invisible, as well as its appendage, so that, in point of fact, there will be no disappearance at all, in the usual sense of the term.

[†] It is interesting to know that the result at which Bessel arrived, relative to the elliptical figure of Saturn, has been completely verified by means of similar observations

The singular appendage with which Saturn is furnished has naturally given rise to speculations respecting its physical origin, and various hypotheses, characterized by more or less ingenuity, have been formed in connexion with this subject. Maupertius supposed the materials of the ring to be composed of the tail of a comet, which happening to revolve in the vicinity of the planet was arrested by its attractive force, and compelled to circulate as a satellite round it. According to De Mairain, Saturn was formerly a body of much greater dimensions than it now is, the ring being the residue of the equator of the ancient planet. Buffon supposed, that while the planet was yet in a liquid state the equatorial parts were driven to a considerable distance from its centre by the centrifugal force arising from its rotation, and that having subsequently become solid by cooling, they encompassed the planet in the form of a ring. Du Sejour adopted this view of the origin of the ring, but he further maintained that a continuance of the rotatory movement was necessary to assure its conservation. He remarked that while the ring was yet in a liquid state, its constituent particles being exposed to the incessant action of the planet had a constant tendency to precipitate themselves upon its surface. It was necessary, therefore, to suppose that the ring was endued with a rotatory movement, sufficiently great to generate a centrifugal force capable of counterbalancing the gravity of the planet. He asserted further that, as the attraction of the planet on the more distant parts of the ring was less, while the centrifugal force, *cæteris paribus*, was greater, it was indispensable towards maintaining the equilibrium of the two forces, that the parts of the ring, at different distances from the centre, should revolve with different velocities. He, therefore, supposed that the ring was formed of several concentric solid zones, endued with different rotatory movements round the planet, the velocity of rotation being less according as the zone was more remote from the centre of the planet. This very plausible hypothesis has been borne out to a certain extent by the researches of subsequent astronomers, as we have already had occasion to mention.

On the 25th of March, 1655, when Huyghens was engaged in examining Saturn with a telescope of 12 feet focal length *, which he had constructed with his own hands, his attention was drawn to a small star which appeared to the west of the planet, at a distance from it of about 3′. He remarked that the star was disposed in the same right line with the ring, which then resembled a luminous line extending on opposite sides of the planet. He also perceived a small star to the east of the planet, which was nearly at the same distance from it with the other, but declined sensibly from the plane of the ring. His impression was that the latter was merely a fixed star; but from its peculiar position with respect to the ring, he suspected that the star on the west side of the planet, was a satellite. On the following evening, having again directed his telescope to the planet, he found that the star on the east side was removed to twice its previous distance, while the one on the west side still retained the same position, relative to the planet which it occupied, when he first saw it. It was now obvious that the former was a fixed star which the planet had left farther behind in the course of its retrograde motion, while the latter was a satellite which accompanied its primary round the sun.

which Mr. Main made on the occasion of the last disappearance of the ring. See *Month. Proc. Ast. Soc.*, vol. ix.

He found by a rough estimation, that the satellite completed its synodic revolution in 16 days, and that its greatest apparent distance from the planet was rather less than 3′. A few years afterwards he fixed the period of revolution more accurately at $15^d\ 23^h\ 13^m$[*]. A curious remark was made by Huyghens in connexion with this discovery, which strikingly illustrates the tenacity with which the ancient notions respecting the perfectibility of the heavens, and the harmony of numbers, continued to cling to minds even of the very first order. He asserted that as the planets and satellites were now equal in number, and as the aggregate of both amounted to twelve, which was universally admitted to be a perfect number, it was reasonable to suppose that the planetary system was complete, and upon this ground he ventured to predict that no more satellites would in future be discovered[†]. Twelve years only elapsed when the discovery of two additional satellites of Saturn served to expose the fallacy of a prediction so unworthy of the genius of its author. Since the epoch of that discovery, the satellites have constantly kept in advance of the planets, in respect of number, until the present day, when the ultra-zodiacal planets have at length come to the rescue of the more ancient members of the system, and a relation of equality again subsists between the planets and satellites, with this material difference, however, that the number of each, instead of being only six, now amounts to eighteen.

The passage of Saturn through the node of his ring in 1671 was an event of peculiar interest to astronomers, inasmuch as it formed the criterion which was to establish the truth or fallacy of Huyghens' theory of the appendage being composed of a ring; and the appearance of the planet on that occasion was, in consequence, an object of more than ordinary attention. On the 25th of October, 1671, while Cassini was engaged in observing Saturn with a telescope of 17 feet focal length, he perceived a small star to the west of it, in a position which very nearly coincided with the plane of the ring. Having repeated his observations on the following evenings, he soon discovered a sensible change in the position of the star with respect to the other stars around it. He continued to observe it on twelve successive nights, at the close of which he established various properties of its motion which convinced him that it was a satellite. He found that its greatest elongation from the planet exceeded three times that of the satellite discovered by Huyghens. He estimated the time of its synodic revolution to be equal in round numbers to 80 days[‡]. Having on a subsequent occasion instituted a comparison between observations which embraced 150 revolutions of the satellite, he determined the period to be $79^d\ 22^h\ 4^m$[§]. The satellite thus discovered by Cassini arrived at its greatest elongation about the end of October, 1671. He continued to observe it till the month of December, when his labours were interrupted by unfavourable weather, and when he afterwards resumed them the satellite could not be found. Having procured a new telescope of 34 feet focal length, he succeeded in rediscovering the satellite on the 13th of December, 1672; but a few days afterwards it again disappeared, and it continued invisible for some time, notwithstanding that he made the most persevering efforts to obtain a sight of it. We shall presently give a more detailed account of the curious property of the satellite which causes it to be periodically invisible to the greater number of observers.

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[*] Opera Varia, tom. ii., p. 551.

[†] Systema Saturnium, Ded.; Opera Varia, tom. ii., p. 530.

On the 23rd of December, 1672, Cassini, while engaged in searching for his lost satellite, discovered a small star near the place where he expected to find it, but still in a position which did not accord sufficiently well with the theory of the satellite's motion. This turned out to be a third satellite, revolving nearer the planet than either of the two others. It completed its revolution in about four days and a half. Subsequently he determined the period with greater precision to be $4^{d}\ 12^{h}\ 27^{m}$. He also found that its greatest distance from the planet did not amount to more than a diameter and two-thirds of the ring*.

In the month of March, 1684, Cassini discovered two more satellites circulating round Saturn. They were both nearer the planet than any of the others previously discovered. The interior of the two satellites, at its greatest elongation, receded from the planet to a distance measuring only two-thirds of the diameter of the ring, and revolved completely round it in $1^{d}\ 21^{h}\ 19^{m}$. The exterior satellite attained an elongation equal to three-fourths of the diameter of the ring, and completed its revolution in $2^{d}\ 17^{h}\ 43^{m}$†. It is obvious that these discoveries could not have been made without telescopes of very considerable optical power. Before, however, any means were devised of obviating the effects of chromatic aberration, it was found to be impossible to construct refracting telescopes of a high power without assigning such an enormous focal length to the object glass as to render the instrument totally unmanageable. Cassini eluded this difficulty by setting aside the tube of the telescope altogether, and placing the object glass in a suitable position for viewing the object through it. A similar mode of observing was also practised about the same time by Huyghens‡. Cassini discovered the two interior satellites of Saturn with object glasses of 136 and 100 feet focal length; but he afterwards succeeded in observing them with glasses of 90 and 70 feet focal length. These glasses were constructed by Campani, at Rome. Cassini first placed them in an aperture, which he had caused to be left for that purpose in one of the towers of the Royal Observatory of Paris at the time of its erection. As this mode of observing could not be conveniently practised at all altitudes, he afterwards adopted the expedient of placing the object glasses sometimes on the top of a pole, and at other times on a wooden tower of great height§.

In order to distinguish the different satellites from each other, Cassini proposed to denominate them according to their distances from the planet, the innermost being the first satellite, the one next to it the second, and so on to the outermost satellite, which was the fifth. According to this nomenclature, the satellite discovered by Huyghens, although the first in the order of discovery, was denominated the fourth satellite.

The reader will not fail to remark that the five satellites of Saturn were discovered at the times of the disappearance of the ring, or at least on the occasions during which it assumed the form of a luminous line. This circumstance is doubtless to be ascribed partly to the superior attention with which the planet was observed at those junctures, and partly to the greater facility of making such discoveries, in consequence of the planet being then disencumbered of its appendage. Six passages of the planet through the node of its ring had occurred without leading to any similar results, when the seventh was at length illustrated by Sir William Herschel's discovery of two additional satellites. On the 28th of August, 1789, having directed to the planet his 40-feet reflector, which he had just completed, that astronomer perceived six small stars, which, from their bright appearance, and their arrangement in the plane of the ring, he at once suspected to be *all* satellites [*]. The planet was then retrograding with great rapidity, and the opportunity was therefore favourable for deciding this point. A very short time served to convince him that his suspicion was well founded. After the lapse of about two hours and a half he discovered, to his great delight, that the planet had carried away all the six stars from their original positions. They proved to be the five old satellites of the planet, and a sixth, which for the first time, had revealed itself to mortal eyes. This satellite was nearer the planet than the innermost or first satellite of Cassini. By a comparison of his observations, Herschel found that it completed a sidereal revolution round the planet in $1^d\ 8^h\ 53^m\ 9^s$. The addition of this satellite to the Saturnian system was succeeded by that of another, which Herschel was enabled, by means of the same powerful telescope, to detect on the 17th of the following month. He found this satellite to revolve still nearer the planet than any of the others. He determined the time of its revolution to be $22^h\ 40^m\ 46^s$. According to the principle of nomenclature adopted by Cassini, the last-mentioned satellite of Herschel should be denominated the first satellite of the planet, the other satellite discovered by that astronomer should be denominated the second, and so on, proceeding outwards from the planet. As a rigorous adherence to this principle would have the effect of altering the designations of the five old satellites, Herschel proposed to call the two satellites discovered by him the sixth and seventh satellites, counting inwards with respect to the planet. Hence the seventh satellite is the nearest to the planet, while the fifth is the most remote from it [†]. The two satellites discovered by Herschel are visible only in telescopes of extraordinary power. The seventh satellite was estimated by that astronomer to be beyond all comparison smaller than the sixth. Even in the 40-feet reflector it appeared only like a very small lucid point [‡]. On account of its vicinity to the planet, it is hidden by the ring throughout the greater part of each revolution. Schroeter, who never could obtain a sight of it, was induced to doubt its existence. It has, however, been repeatedly seen in several of the powerful telescopes of the present day.

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* Mém. Acad. des Sciences, tome x., p. 586.

† Ibid., p. 694, et seq.

‡ The credit of first practising this mode of observation has been generally ascribed to Huyghens, but Cassini distinctly asserts that his observations of Saturn's satellites were the first that had been made without employing the tube of the telescope. He states that he had already discovered the two interior satellites in that manner when Huyghens published his '*Atroscopium*,' in which he explains a method analogous to his, though much more troublesome in detail.—(*Mém. Acad. des Sciences*, 1705, p. 23.)

§ Mém. Acad. des Sciences, tome x., p. 702; 1705, p. 23.

It will be readily seen, by a comparison either of the distances or the periodic times of the satellites of Saturn, that a disproportionately-wide interval exists between the orbits of the fourth and fifth satellites. The vacuity hence arising has been to a certain extent filled up by the discovery of an eighth satellite of the planet, on the occasion of the recent passage of the plane of the ring across the earth's orbit. This interesting result is due to the independent labours of two astronomers, who although residing in different hemispheres, recognized the satellite on the very same day, viz., the 19th of September, 1848. On the 16th of the month just mentioned, Prof. Bond, of Cambridge, U. S., while engaged in observing Saturn, perceived a small star of the seventeenth magnitude, situated nearly in the plane of the ring. On the 19th he discovered that the star was retrograding with the planet, whence its real nature at once suggested itself to him. Mr. Lassel, of Starfield, Liverpool, arrived at the discovery of the body in the same manner. He first observed it as a star on the 18th of September, and on the following evening he detected such indications of its motion as enabled him to establish, beyond all doubt, that it was a satellite. Thus, although Mr. Bond first saw the satellite as a star, he discovered its real nature only on the same night with Mr. Lassel. But, indeed, although either of these astronomers had discovered the satellite several days previous to the other, it would be absurd in such a case to draw any distinction between their respective merits. Of course the discovery of Mr. Lassel was generally announced throughout Europe, before the surprising intelligence of Mr. Bond's simultaneous discovery of the satellite was wafted across the Atlantic*. It is a curious fact that Huyghens, as if to atone for his unfortunate prediction relative to the secondary planets on a former occasion, suggested the probability of a satellite revolving in the interval included between the orbits of the fourth and fifth satellites of Saturn†. In conformity with the nomenclature proposed by Sir John Herschel, the new satellite has received the name of Hyperion. Its period has been estimated to be $22^d\ 12^h$, but this can only be considered a provisional evaluation.

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[*] Phil. Trans., 1790, Part I., p. 10.

[†] As an improvement in the nomenclature of the Saturnian system was very desirable, Sir John Herschel has recently proposed to denominate the satellites after the Titanian divinities. The names of the seven satellites, commencing with the one most remote from the planet, and proceeding regularly inwards, are contained in the following line:

Japetus, Titan, Rhea, Dione, Tethys, Enceladus, Mimas.

This mode of distinguishing the satellites seems in a fair way of being generally adopted. The discovery of an eighth satellite has shown the absolute necessity of some such nomenclature.

[‡] Herschel, however, succeeded in seeing it with his 20-feet reflector.

The only fact which has been established relative to the physical constitution of the satellites of Saturn is, the remarkable variation of the light of the fifth satellite. It has been already mentioned that this satellite disappeared soon after its discovery in 1671, and that after Cassini recovered a sight of it in the following year, it again speedily eluded his observations. Having watched it throughout a great number of revolutions, that astronomer found that it was invariably invisible in the eastern part of its orbit. It regularly disappeared two or three days after passing its superior conjunction, and did not re-appear until two or three days before its arrival in inferior conjunction. As the period of the satellite is nearly equal to 80 days, it continued consequently invisible for about a month during each revolution. Two interesting conclusions were deducible from this fact. In the first place, it was obvious that there existed extensive tracts on the surface of the satellite that were incapable of reflecting a sufficient quantity of the solar light to render them visible. In the second place, since these dark tracts were constantly turned towards the sun when the satellite was in the same parts of its orbit, it followed that the satellite presented the same hemisphere towards the planet, during each synodic revolution, and consequently its motion upon its axis was equal to its motion round its primary, as in the case of the earth's satellite.

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* Mr. Lassel discovered the satellite with a Newtonian reflector of 20-feet focal length, and 24-inches aperture. Prof. Bond effected its discovery with a magnificent refracting telescope, the object glass of which has a diameter of 15 inches.

† Cum enim inter extremas duas, spatium amplius pateat quàm pro distantiis cæterarum; posset hoc insidere sextus satelles.—*Cosmotheoros*, p. 99, *Opera Varia*, tom. ii.,

The curious discovery of Cassini, above referred to, has been verified by the observations of subsequent astronomers. Sir William Herschel, with a view to establish beyond all doubt the variation of the light of the satellite, observed it with the most scrupulous attention throughout a great number of revolutions. By means of his powerful telescopes, he was enabled to perceive it throughout the entire course of its revolution round the planet, but he found that it constantly experienced a great diminution of lustre when it was passing through the eastern half of its orbit. He also discovered, by a nice comparison of its light with that of each of the other satellites, that it varied much in brightness throughout each revolution, but that it always exhibited the same degree of brightness when it appeared in the same part of its orbit. This interesting fact accords admirably with the conclusion previously suggested by the observations of Cassini, namely, that the satellite rotates completely on an axis in the same time which it takes to accomplish its revolution round the planet. Herschel concluded from his observations, that the light of the satellite is in full splendour when it is traversing the part of its orbit which is between 68° and 129° past the inferior conjunction. He estimated that, in passing through this arc, it does not fall above one magnitude short of the brightness of the fourth satellite. On the other hand, from about seven degrees past the opposition till towards the inferior conjunction, it is not only less bright than the third satellite, but it hardly rivals the second or even the first. Upon the whole, the alteration of brightness appeared to him to be equivalent to a change from the fifth to the second magnitude *.

On the evening of the 13th of March, 1781, when Sir William Herschel was engaged in examining the small stars in the neighbourhood of H Geminorum, his attention was attracted towards a star which appeared sensibly larger than any of those around it. Being struck with its unusual size, he instituted a comparison between it and two other small stars, and finding it to be much larger than either, be began to entertain a suspicion that it was a comet. In order to obtain a stronger assurance on this point, he had recourse to a delicate criterion, by means of which astronomers are usually enabled to distinguish a fixed star from a planet or comet. The apparent diameters of both the fixed stars and planets are generally found to increase when a higher magnifying power is applied to the telescope with which they are observed; but there is this essential distinction between the two classes of objects,—that while the apparent diameters of the planets are enlarged in the exact proportion of the magnifying power, those of the fixed stars do not increase at so rapid a rate. At the same time, however, the light of the planet becomes fainter, and its outline appears ill defined; while, on the other hand, the fixed stars, under similar circumstances, retain their usual lustre and distinctness. When Herschel first saw the star, he had been using a magnifying power of 227. He now applied to his telescope (which was a seven feet reflector) magnifying powers of 460 and 932, and he found, agreeably to his conjecture, that the star acquired successively a duller and more confused appearance, and was in each case enlarged in the exact proportion of the magnifying power, while the stars with which he compared it, retained their usual aspect, and exhibited a less rapid varia-

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* Phil. Trans., 1792, p. 14.

tion of magnitude. Having now felt a strong persuasion that the object was a comet, he determined, by careful estimation, its position with respect to a telescopic star near to it, intending to make observations of it on the following evenings, for the purpose of ascertaining whether it had a proper motion. A very brief lapse of time served to dispel all doubts upon this point, the star having been found by him to be revolving with a slow motion, according to the order of the signs, in an orbit which deviated very little from the plane of the ecliptic. He continued to observe the star until the 19th of April, determining its position on each occasion by measuring, with a micrometer, its distance from a telescopic star near to it, and also its angle of position with respect to the same star, or in other words, the angle contained between an imaginary line joining the two stars and the parallel of declination passing through the telescopic star. He also executed several micrometrical measures of its apparent diameter. Having drawn up an account of his observations, he communicated it to the Royal Society in a paper, which was read before that body on the 26th of April, 1781. In this paper he does not appear to entertain a suspicion that the object of his discovery was any other than a comet*.

Previous to transmitting the above-mentioned communication to the Royal Society, Herschel had taken an opportunity of announcing his discovery to Dr. Maskelyne, the Astronomer Royal, who in his turn gave due notice of it to the astronomers of France. Messier commenced his observations of the supposed comet on the 16th of April, 1781, and his example was speedily followed by Lalande, Lemonnier, Mechain, and D'Agelet, as well as by Reggio, De Cesaris, Bode, Wargentin, and various other astronomers on the Continent. As soon as a few observations of it were obtained at Paris, an attempt was made by means of them to determine the elements of the parabolic orbit in which it was presumed to revolve. A serious difficulty, however, soon presented itself to those engaged in this enquiry. It was found that although a parabola might be assigned, which would represent with tolerable fidelity a limited number of observations of the comet, yet in a few days afterwards, the positions of the body, when calculated upon the same hypothesis, appeared to be totally irreconcilable with the actual motion. Various attempts to discover an orbit which would *permanently* represent the motion of the body were made by Mechain, the President de Saron, Laplace, Boscovich, and others, but in all instances they proved to be equally unavailing for this purpose. Nor is their failure at all to be surprised at, for, since the body was universally supposed to be a comet, it was concluded, by reasoning from analogy, that the perihelion, at the utmost, would not extend beyond the orbit of Jupiter, and that in all probability it was situated far within the terrestrial orbit†. The attention of each calculator was therefore constantly directed towards constraining the body to move in an orbit the perihelion distance of which, even upon the most extravagant supposition, was imagined not to amount to four times the radius of the terrestrial orbit. It was never suspected all the while, that the nearest distance of the body from the sun exceeded the same standard of measurement at least eighteen times.

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* Phil. Trans., 1781, p. 492, et seq.

† In Delambre's catalogue of 116 comets, comprehending all the bodies of this nature whose elements have been determined down to the year 1813, there is only one comet, viz., that of 1729, whose perihelion distance (4.0698) exceeds four times the terrestrial orbit. There are only six comets in the catalogue which, at their perihelia, passed beyond the orbit of Mars, and the whole number which passed beyond the earth's orbit

The President de Saron appears to have been the person who threw the first glimmering of light on this perplexing subject. On the 8th of May, 1781, he announced that the comet was in reality much more remote from the sun than astronomers had hitherto supposed it to be. He estimated its perihelion distance to be equal to at least twelve times the radius of the terrestrial orbit *. This was an important suggestion, for it had the effect of directing the attention of enquirers to the region of the heavens in which the body was actually revolving. By adopting it, the observations were represented with greater precision than they had been on any previous hypothesis, and hopes began to be entertained of arriving at a determination of the real orbit of the body.

The next step in the enquiry was made by Lexell, who happened to be in England at the time of Herschel's discovery. In an account of his researches which he communicated to the Academy of St. Petersburg, he mentions that Dr. Maskelyne, and the other English astronomers who observed the body, agreed with him in supposing that in all probability it was a planet †. Various circumstances, he remarks, concurred in suggesting this view of its nature. In the first place, observation shewed it to be a well-defined object, whereas comets generally have a nebulous appearance. Again, although very small, it was not difficult to discern a difference in its light from that of the fixed stars. Lastly, its slow motion in latitude (indicating that its inclination to the ecliptic was very inconsiderable), and its motion in the zodiac according to the order of the signs, were two independent facts which both strongly supported the hypothesis of its being a planet. Taking two extreme observations of the body, one by Herschel, dated March 17, 1781, and the other by Maskelyne, dated May 11 of the same year, Lexell found that they might be both satisfied by a circular orbit, whose radius was equal to 18.93, the mean distance of the earth from the sun being supposed equal to unity. In the month of June or July, while still residing in England, he wrote a letter to one of his friends in Paris, in which he stated, that the motion of the body which formed the subject of so much anxious investigation might be represented by a circular orbit, whose radius was equal to eighteen times the mean distance of the sun from the earth. "From that time," says Lalande, "it appeared to me that the body ought to be called the new planet." ‡ Lexell soon afterwards found that on account of the slow motion of the body, and the consequent smallness of the arc described by it within a limited interval of time, the observations included between March 17 and May 28 might be satisfied by an infinite number of parabolas, whose perihelion distances varied from 6 to 22 times the radius of the terrestrial orbit. From this circumstance it appeared evident to astronomers, that until the planet had described a larger arc, it would be impossible to arrive at an accurate knowledge of the elements of its orbit.

After the lapse of a few months, when the motion of the planet began to be developed more clearly, its distance from the sun, upon the suppo-

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* Mém. Acad. des Sciences, 1779, p. 529.

† Nov. Act. Acad. Petrop., tom. i., p. 69, et seq.

‡ "Dès lors il me parut qu'on devait lui donner le nom de *nouvelle planète*.—Mém. Acad. des Sciences, 1779, p. 530.

sition of a circular orbit, was determined with considerable precision; but from the discordances which still existed between the computed and the observed positions, it was plainly apparent that the real orbit was an ellipse of small eccentricity. Elliptic elements of the planet were first calculated by Laplace, and were communicated by him to the Academy of Sciences, in the month of January, 1783.

When it was ascertained beyond all doubt that the body discovered by Herschel was a planet, it became desirable to distinguish it by some special name. As the privilege of choosing a name in all such cases is the incontestable right of the discoverer, Herschel, urged by a feeling of gratitude towards his royal patron, George III., proposed to confer on the planet the appellation of the *Georgium Sidus*. Lalande, influenced by an equally honourable motive, suggested the name of *Herschel*. Both these names sounded incongruously with the prevailing nomenclature of the planetary system, and neither of them consequently met with much favour on the part of astronomers. The names of various heathen divinities were proposed as more appropriate for this purpose. After some time had been spent in discussing the rival claims of different deities, the name of Uranus, suggested by Bode, was finally adopted by astronomers, and has always since been employed to distinguish the planet.

A point of great interest to be determined was, the magnitude of the body, by the discovery of which the planetary system had just been enriched. For this purpose two data were indispensable, namely, the distance of the planet from the earth at any assigned instant, and the angle subtended by its diameter when viewed at that distance. The former of these could be determined with facility, and with a considerable degree of precision; the case was very different with respect to the latter. Herschel's first measures of the apparent diameter of the planet, exhibited a remarkable discordance with each other. On the 17th of March he fixed it at 2″.53‴, on the 2nd of April he made it 4″.25‴, and on the 18th of the same month he determined it to be 5″.2‴ [*]. A similar discordance existed between the measures of other astronomers. Maskelyne was induced to fix the magnitude of the apparent diameter at 3″. The astronomers of Milan fixed it between 6″ and 7″. Mayer of Manheim estimated it to be as high as 10″. Lexell, despairing of the possibility of determining the apparent diameter of the planet by micrometrical measurement, attempted to ascertain its value by comparing the planet with another body whose apparent diameter was known. For this purpose he compared it with Mars, at a time when that planet was near the position of apogee, and when his apparent diameter in consequence did not exceed 5″. Finding that Uranus appeared to be less than the planet with which he compared it, he hence concluded that its apparent diameter fell certainly below 5″, and in all probability did not exceed 3″ [†]. The mean of these two extremes gives 4″ for the apparent diameter of the planet, a result which forms a closer approximation to the true value than any other that had been hitherto assigned by astronomers. In order to remove all doubts upon this subject, Herschel in 1782 undertook a series of measures of the planet with two micrometers, one of which, called the lamp micrometer, was an instrument of his own invention. He thus obtained a number of results, from which it appeared that the mean value of the angle subtended by the diameter of the planet was somewhere about 4″[*]. On a subsequent occasion he instituted a strict comparison between these results, and hence concluded that the apparent diameter of the planet, when viewed at its mean distance from the earth, was equal to 3″.91. By combining this result with the distance of the planet as recently ascertained (viz., 19.08, the mean distance of the earth being supposed equal to unity), he was enabled to determine its linear dimensions and volume. In this manner he found that the diameter of the planet measured 34217 miles. It therefore exceeded the diameter of the earth in the proportion of 4.3 to 1. It was easy also to infer that in volume it exceeded the same body in the proportion of 80 to 1[†]. From these results it appeared that the newly-discovered body was, after Jupiter and Saturn, by far the most considerable of those bodies hitherto recognised as revolving round the sun.

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[*] Phil. Trans., 1781, p. 494.

[†] Nov. Act. Acad. Petrop., tom. i., p. 78.

The enrichment of the planetary system consequent on the accession of Uranus to it, marks the commencement of the long series of brilliant discoveries and sublime speculations which adorned the astronomical career of Sir William Herschel[‡]. It has been frequently asserted that this noble achievement was the effect of chance, and the inference has been hastily drawn, that the merit associated with it is of a very inferior order compared with that due to the same astronomer on account of the many other efforts of his genius. It is true that the discovery was accidental, inasmuch as it did not result from an examination of the heavens, instituted in pursuance of any theoretical views respecting the existence of the body; but if it is thereby meant that the planet might with equal probability have presented itself *as such* to any observer, there cannot we conceive be a more vulgar error. A few remarks upon the subject will amply illustrate the justness of this conclusion. In the first place, it may be asserted that the discovery was no other than the legitimate reward which might be expected eventually to crown the exertions of an astronomer, who continued from night to night with unwearied enthusiasm to explore the heavens, with optical appliances which owed their exquisite character solely to the resources of his own genius. Upon this ground alone, therefore, the author of the discovery, even if he had not borne the immortal name of Herschel, would have been entitled to a high place among those who have successfully explored the celestial regions. Nor would a less generous award be in unison with the natural promptings of the human heart. The motto of one of England's most illustrious sons, "*palmam qui meruit ferat*," expresses, in appropriate language, the spontaneous response of the mass of mankind in all ages, to every result achieved, whether in arts or arms, by a well-directed course of skilful energy and unflinching perseverance. But the planet, in fact, was involved in an extensive field of observation, which the astronomer had conceived the design of submitting to a systematic scrutiny; and it only required the application of his mental powers to the realisation of this design to conduct him inevitably to the wandering body[§]. The *occasion* was, therefore, favourable for detecting the planet, but this would have been a useless advantage without the *man*. Similar junctures must frequently offer themselves to every person who devotes his attention to physical phenomena, but how few are sagacious enough to discern their presence, and extract from them their legitimate consequences! The circumstances attending the discovery in the present instance, abundantly serve to prove that its author was no ordinary observer. The singling out of the planet from among the multitude of similar objects by which it was surrounded, was an operation of the utmost delicacy, which demanded extraordinary powers of discernment. Lalande has expressed his astonishment that Herschel should have been led to direct his attention especially to the planet, considering that with an instrument of his own, which magnified 120 times, the appearance of the body did not differ from that of a star of the seventh magnitude [*]. The language of Messier is equally decisive upon this point. "Nothing," says that astronomer, writing to Herschel, "was more difficult than to recognise the body; and I cannot conceive how you have been induced to return repeatedly to that star or comet, for it has been absolutely necessary for me to observe it several days in succession, in order to obtain an assurance that it had a proper motion." [†]

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[*] Phil. Trans., 1783, p. 13.

[†] Ibid., 1788, p. 378.

[‡] Born at Hanover 1738; died at Slough, in England, 1822.

[§] Herschel was engaged in a series of observations with a view to the investigation of the annual parallax of the stars, when his attention was first drawn to the planet.

The different sets of elements which Laplace and his contemporaries had calculated for Uranus, soon after its discovery, could only be considered as provisional, the motion of the planet not having been yet sufficiently developed to justify the hope of determining with precision the form and position of the orbit in which it revolved. In 1790, the Academy of Sciences, with the view of eliciting a definitive determination of the orbit of the planet, proposed its theory as the subject of a prize. It has been already mentioned in one of the foregoing chapters, that the prize was awarded to Delambre. An account has also been given, in the same chapter, of the subsequent researches of astronomers on the theory of the planet, and of the memorable consequence which ensued from the study of the irregularities of its motion.

The immense distance of Uranus precludes all hopes of discovering any phenomena indicative of its physical constitution, analogous to those interesting appearances, which an examination of the other principal planets with the telescope has revealed to astronomers. Sir William Herschel was induced to suspect that the figure of the planet is sensibly spheroidal. Observations with his 7-feet, 10-feet, and 20-feet reflectors, all concurred in suggesting the same conclusion relative to this point. The longer axis of the planet also appeared to him, agreeably to the analogy of Jupiter and Saturn, to be situated in the plane of the orbits of the satellites. Assuming the ellipticity of the planet to be an established fact, he hence concluded that the planet revolves with considerable velocity upon an axis [‡]. M. Arago has naturally expressed his surprise that an observer so scrupulous as Herschel should have contented himself with a simple estimation of the figure of the planet, when he might have obtained a definitive assurance upon this point, by direct measurement of the equatorial and polar axes with a micrometer [§]. There are two distinct circumstances, however, which concur in rendering the determination of the ellipticity of Uranus an operation of extreme difficulty. In the first place, the smallness of the apparent magnitude of the planet has a tendency to cause a small error in the measurement of either of the axes to exercise a very material influence on the ellipticity. In the case of Jupiter, the difference between the equatorial and polar axes, at the mean distance of the planet from the earth, is equal to 2″.789; and as the ellipticity is measured by the ratio of this quantity to the equatorial axis, it is obvious that an error of a tenth of 1″, committed in its determination, will entail on the ellipticity an error less than $\frac{1}{25}$th of its real value. Now, if we assume the ellipticity of Uranus to be equal to that of Jupiter, the difference between the equatorial and polar axes (supposing the former to be equal to 4″) will amount only to 0″.2916. It follows, therefore, that an error equal to a tenth of 1″ committed in the measurement of either of the axes would occasion an error in the ellipticity greater than one-third of its true value. M. Mädler, indeed, has obtained for the planet an ellipticity equal to $\frac{1}{9.22}$, indicating a difference between the equatorial and polar axes amounting to 0″.435. But even upon such a supposition, an error of measurement equal to that assumed in the two previous cases would produce an alteration to the extent of one-fourth in the value of the ellipticity. There is another circumstance, however, which operates unfavourably in attempting to determine the ellipticity of Uranus. The remark of Herschel, that the longer axis of the planet extends in the plane of the orbits of the satellites, leads to the curious conclusion that the equator of the planet is nearly perpendicular to the plane of the ecliptic, since the satellites have been found to revolve in orbits which are nearly perpendicular to that plane. The consequence of this anomalous condition will be, that the planet will exhibit the full quantity of its ellipticity only in two opposite points of its orbit, namely, those wherein the plane of its equator passes through the earth, for the planet in such positions alone is projected upon a plane passing almost through its poles. On the other hand, there are two opposite points, 90° distant from these, in which the planet will assume nearly a circular appearance, since the plane of projection almost coincides with the plane of the equator. In any intermediate position the apparent ellipticity will be less than the true, and it will continually diminish as the planet recedes to a greater distance from either node of its equator. It is obvious from this circumstance, that even if the ellipticity of the planet should be very considerable, it is not liable to be detected except when the planet is passing through either of the nodes of its equator, an event which can only occur at successive intervals of half a revolution of the planet, or about forty-two years.

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[*] Mém. Acad. des Sciences, 1779, p. 528.

[†] "Rien n'etait plus difficile que de la reconnaître; et je ne puis pas concevoir comment vous avez pû revenir plusieurs fois sur cette étoile ou comète, car absolument il a fallu l'observer plusieurs jours de suite pour s'appercevoir qu'elle avait un mouvement." —Phil. Trans., 1781, p. 500.

[‡] Phil. Trans., 1798, p. 71.

[§] Annuaire, 1842, p. 579.

It follows from the foregoing remarks that, even if the ellipticity of Uranus should be considerable, the difference between the lengths of the apparent equatorial and polar axes, which is at all times very small, on account of the minute dimensions of the disk, is in general rendered still smaller by the peculiar position of the equator of the planet. Perhaps this circumstance may explain the reluctance of Herschel to determine the ellipticity of the planet by actual measurement, the small difference between the *apparent* lengths of the equatorial and polar axes not offering any chance of his obtaining a trustworthy result by this means. It appears, indeed, as already stated, that M. Mädler has obtained for the planet an ellipticity equal to $\frac{1}{9.22}$. M. Otto Struve, however, who observed the planet still more recently with the magnificent refractor of Pulkowa, was unable to discern the slightest trace of ellipticity.

Sir William Herschel at one time was inclined to suspect that Uranus is surrounded by one or two rings. On the 4th of March, 1787, having examined the planet with his 20-feet reflector, he perceived two projecting points, at opposite extremities, of a diameter extending east and west, and also two similar points, though somewhat smaller, at a distance of 90° from the others. The same appearance having been witnessed by him on several successive evenings, he suspected that the planet might be encompassed by two rings at right angles to each other *. Subsequent observations, however, shewed him that the appearance was illusory. The suspicion of a ring returned to his mind on a future occasion; but in this instance also it proved to be unfounded. Indeed, he remarked that during the interval of ten years, extending from 1782 to 1792, which his observations of the planet embraced, whatever might have been the position of the node of the ring, the planet must have removed to a sufficient distance from it at some time during that period, to cause such an opening of the ring as would render it pretty visible, if it had existed at all.

Soon after his discovery of Uranus, Herschel endeavoured to ascertain whether it was attended by satellites; but although he repeatedly examined the planet for this purpose, with his most powerful telescopes, he was unable to discover any trace of the existence of such bodies. He ascribed his failure to the want of a sufficient quantity of light to render visible such faint objects as he presumed the satellites of so remote a planet would be. As soon, however, as he experienced the advantage of employing the front view in his telescopes, from the additional quantity of light which he gained by this contrivance, he again resolved to prosecute this interesting enquiry. Accordingly, on the 11th of January, 1787, he directed one of his telescopes † to the sweep, including the planet, and when it arrived on the meridian he perceived several faint stars near to it, the positions of which he noted down with great care. On the following evening, when the planet returned to the meridian, he looked out with eager scrutiny for his stars, and he found that two of them were missing. He repeated his observations on the 14th, 17th, 18th, and 24th of January, and also on the 4th and 5th of February, carefully delineating on each occasion the configurations of the small stars in the vicinity of the planet. Although he had no longer any doubt of the existence of one satellite at least, he deferred making any communication respecting it, until he had seen it actually in motion. Accordingly he directed his telescope to the planet on the 7th of February, and having fixed his attention on the satellite at six o'clock in the evening, he steadily kept it in view until 3 o'clock in the following morning. During the course of nine hours that he remained at the telescope, he had the gratification of perceiving that the satellite continued faithfully to attend the planet, while at the same time it described a considerable arc of its proper orbit. He did not omit following another star which, from his previous observations, he suspected to be a satellite, but from his attention having been so strongly directed to the object already mentioned, he could not be so well assured of its motion. The observations of the 9th of February removed all doubts from his mind, the star, during the interval which elapsed since his previous observation, having advanced in the same direction with the other star already recognised as a satellite, but with a

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* Phil. Trans., 1798, p. 67.

† This appears, from the context, to have been his 20-feet reflector, although he does not expressly mention that it was that instrument.

quicker motion. From this circumstance he inferred that it revolved between the latter and the planet. He, therefore, called it the first satellite while the one more remote, although the first that had been discovered, received from him the name of the second satellite.

Herschel communicated an account of his discovery of the satellites of Uranus to the Royal Society, in a paper which was read before that body on the 15th of February, 1787[*]. A sufficient interval of time had not yet elapsed to enable him to determine the elements of their motion; but by a rough estimation, he found that the first satellite completed its synodic revolution in about eight days and three quarters, and the second in thirteen days and a half. He also remarked that the orbits were inclined at considerable angles to the ecliptic. As soon as he obtained an adequate number of observations of the two satellites, he undertook a rigorous determination of their elements. The results of his researches are contained in a paper which was read before the Royal Society on the 22nd of May, 1788[†]. He found the time of a synodic revolution of the first satellite to be $8^d\ 17^h\ 1^m\ 19^s.3$, and that of the second to be $13^d\ 11^h\ 5^m\ 1^s.5$. He determined the apparent distance of the second satellite at the mean distance of the planet from the earth to be $44''.23$. It was a matter of such extreme difficulty to obtain even a sight of the first satellite, that he did not attempt to establish its distance from the planet by observation; but knowing the periodic time, and knowing also the periodic time and distance of the second satellite, he was enabled to deduce it from these data by means of Kepler's third law. In this manner he found the apparent distance of the satellite from the planet to be $33''.09$. He determined the inclination and the longitude of the node of the second satellite, but the results exhibited an ambiguous character, in consequence of an uncertainty in the observations, which could not be removed until the planet had revolved round the sun through an arc sufficiently large to occasion a sensible change in the position of the satellite's orbit relative to the earth. The ambiguity involved an extraordinary alternative, to which we shall presently have occasion to allude more particularly. Herschel found from his observations of the first satellite, that the position of its orbit did not deviate sensibly from that of the second. On account of the great inclinations of their orbits, the satellites could only be eclipsed when the planet was passing through either of their nodes. Herschel announced that they would undergo eclipses in either of the years 1799 or 1818, adding that they would appear, on such an occasion, to ascend out of the shadow of the planet, in a direction almost perpendicular to the plane of the ecliptic. This interesting phenomenon was predicted with a remarkable degree of accuracy, considering the extreme difficulty of obtaining reliable observations of the satellites, and the short interval of time which elapsed since their discovery; but from a circumstance connected with the visibility of these minute bodies, which the illustrious astronomer was the first to point out, it has not been hitherto allotted to mortal eyes to witness it.

We have mentioned that the results at which Sir William Herschel arrived, relative to the position of the orbits of the satellites, were to a certain extent ambiguous. It was manifest that the orbits were almost perpendicular to the plane of the ecliptic, and that the planet was advancing towards their ascending node; but there were two positions of the orbits, either of which would satisfy the observations. This will be easily understood when it is borne in mind that a circle viewed obliquely assumes the form of an ellipse, whose eccentricity depends upon the degree of obliquity, and that it is possible to assign two different positions to a circle, in each of which it will be equally oblique with respect to the visual ray. As an illustration of this remark let the circle A B C D represent the orbit of the satellite, and let the plane of the paper be perpendicular to

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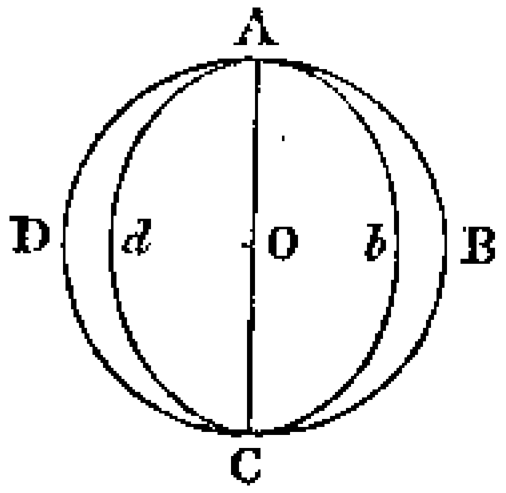
[*] Phil. Trans., 1787, p. 125, et seq.

[†] Phil. Trans., 1788, p. 364, et seq.



the visual ray. Let us now suppose the orbit to turn through a certain angle round the axis A C, so that the semicircle A B C shall, by this means, be elevated above the plane of the paper, while the opposite semicircle, A D C, is equally depressed below it. When the orbit is viewed in this new position, it will be projected upon the plane of the paper, and will appear to coincide with the ellipse A *b* C *d*, the eccentricity of which will increase as the orbit revolves through a greater angle, or, in other words, according as its plane becomes more oblique with respect to the visual ray. If, however, we had supposed the orbit to revolve in the opposite direction through an equal angle, so that the semicircle A B C was depressed below the plane of the paper, while the opposite semicircle, A D C, was equally elevated above it, the projected orbit, in this case also, would manifestly coincide with the ellipse A *b* C *d*. It will, therefore, be impossible for an observer, by simply viewing the motion of the satellite from a remote point situate above the plane of projection, to ascertain whether the semicircle A B C is turned towards him, or whether it is turned away from him. It is manifest, however, that the position of the orbit of the satellite, with respect to a fixed plane, will be very different in the two cases. The true position can only be decided by the motion of O, the common centre of the two circles, which causes one of the assumed orbits of the satellite to become more, and the other to become less oblique with respect to the visual ray; whence, by confronting the two corresponding ellipses with the results of observation, it is possible to ascertain which of the circles represents the real orbit of the satellite. For example, let us suppose O (representing the planet) to revolve from right to left round the eye of the observer as the centre of motion, the plane of the circle A B C D, constantly moving parallel to itself, it is then manifest that if the semicircle A B C be turned *towards* the observer it will become more and more oblique with respect to the visual ray, and, therefore, the ellipse A *b* C *d* will gradually contract in width. On the other hand, if the semicircle A B C be turned *away from* the observer, the motion of O will cause the plane of the orbit A B C D to become less and less oblique with respect to the visual ray, and the ellipse will gradually open out. Herschel accordingly remarked in 1788, that although the position of the orbit of the second satellite was then doubtful, the true position would be apparent ellipse of the satellite. If the ellipse should contract, the longitude of the ascending node of the satellite would be 168° 0′ 3″.9, and the planet would pass through it in 1799; but on the other hand, if the ellipse should open out, the longitude of the ascending node would be 246° 2′ 2″.3, and the passage of the planet through it would not take place before the year 1818*. The most interesting circumstance connected with this ambiguity referred to the motion of the satellite, which would be direct or retrograde, according as the ellipse was observed to open or close. A similar remark was applicable to the first satellite, the position of whose orbit was found to coincide with that of the second. Herschel found from observations of both satellites, subsequent to 1788, that their apparent ellipses continued for some time to contract, and he hence drew the legitimate conclusion that their motion is retrogade. He formally announced this result for the first time in a paper which he communicated to the Royal Society towards the close of the year 1797†. This singular anomaly in the planetary system has been confirmed by the observations of subsequent astronomers. Herschel was now enabled definitively to announce that the ascending node of the satellites was situated in 168° 0′ 3″.9 of longitude, and that the planet would pass through it in the year 1799. The common inclination of their orbits to the orbit of the planet had been found by him in 1788 to be 80° 20′ 11″.1‡. On a future occasion he arrived at a more accurate determination of the position of the orbits.

In the paper above referred to, Herschel announced his discovery of four additional satellites of Uranus. The number of the satellites of the planet therefore now amounted to six. One of the new satellites was the nearest of all to the planet; another revolved in the space included between the orbits of the two satellites already discovered; the remaining two were exterior to both the old satellites. Conformably to the practice of naming the satellites according to the order of their distances from the planet, Herschel, in this paper, distinguished the two satellites discovered in 1787, by the names of the second and fourth satellites§. With respect to the four new satellites, although confidently asserting their existence, he did not venture to give an exact determination of their distances from the planet or their periodic times, on account of the small number of undoubted observations of them which he possessed, but he assigned, as provisional data, certain approximate values of these elements, remarking that future observations might require that they should be considerably modified. These results, including the elements of the second and fourth satellites, as determined by him in 1788, are contained in the following table.

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* Phil. Trans., 1788, p. 375.

† Phil. Trans., 1798, p. 47, et seq.

‡ Phil. Trans., 1788, p. 375. Herschel, indeed, assigned in this paper two values of the inclination, corresponding to the two values of the longitude of the node, and it was by means of these results that he expressed the ambiguity in the position of the orbits. As, however, the other value of the inclination, viz. 99° 39′ 48″.9, is the supplement of that mentioned in the text, it is manifest that the conclusion at which Herschel arrived may be equally well expressed by adopting the smaller inclination in both cases, and supposing the motion to be retrograde in the case wherein the supplement was assigned. For the sake of simplicity this mode of interpretation has been employed.

§ The illustrious astronomer did not, however, invariably adhere to this nomenclature, for in all discussions relating exclusively to the two old satellites, he applied to them their original appellations. In order not to introduce confusion into the text, we shall con-

Order of Distance.	Date of Discovery.	Distance from the Planet.	Periodic Time.
1st satellite	January 18, 1790	25″.5	$5^d\ 21^h\ 25^m$
2nd do.	January 11, 1787	33 .09	8 17 1
3rd do.	March 26, 1794	38 .57	10 23 4
4th do.	January 11, 1787	44 .23	13 11 5
5th do.	February 9, 1790	88 .46	38 1 49
6th do.	February 28, 1794	177 .92	107 16 40

The distance of the first satellite was the result of a micrometrical measure executed by Herschel at a time when he supposed the satellite to be at its greatest elongation from the planet. The distance of the third satellite was determined by assuming its orbit to bisect the linear interval included between the orbits of the two older satellites; the fifth satellite was supposed to be twice as distant from the planet as the fourth, and the sixth satellite to be four times as distant as the same satellite was.

The periodic times were not deduced from observation, but were calculated by means of Kepler's third law *.

In 1815 Herschel communicated to the Royal Society his fourth and last paper on the satellites of Uranus †. The passage of the planet through the ascending node of the satellites had supplied him with a favourable opportunity of obtaining a more accurate determination of the elements of the two old satellites, their apparent orbits on that occasion having assumed the form of straight lines. He found from observation, that the planet passed through the ascending node of the satellites on the 12th of March, 1798. The longitude of the node was, therefore, 165° 30′. He fixed the inclination of the orbits of the satellites to the ecliptic at 78° 58′. A new investigation of the synodic revolutions of the two satellites gave him $8^d\ 16^h\ 56^m\ 5^s.2$ for the period of the second satellite, and $13^d\ 11^h\ 8^m\ 59^s$ for the period of the fourth. With respect to the question of the distances of the satellites from the planet, he did not undertake to correct the results he had already arrived at, but he assigned certain measures of both satellites, which he considered might be useful in future enquiries relative to this point.

The passage of the planet through the node of the satellites in 1798, enabled Herschel to obtain a few more observations of the satellites whose existence he first announced in 1797. He still continued to assert his firm belief in the existence of other satellites besides those of 1787, but it was a matter of such extreme difficulty to obtain even occasional glimpses of these remote atoms, and still more so to distinguish them from one another, that he did not venture to give a rigorous determination of their elements, or even to assign the precise number of satellites. He therefore confined himself to the communication of all the observations indi-

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* Herschel's observations of the supplementary satellites appear to have been made with his 20-feet reflector. In a subsequent paper (*Phil. Trans.*, 1815) he explains the grounds upon which he was induced not to make more frequent use of his gigantic telescope of 40 feet focal length, which conducted him, immediately after its completion in 1789, to the discovery of the two interior satellites of Saturn.

† Phil. Trans., 1815, p. 293, et seq.

cative of their existence, leaving to future astronomers the task of deducing positive results respecting them, when a more complete system of data for this purpose should be obtained by means of additional observations.

Sir William Herschel found that the interior of the two old satellites was brighter than the exterior satellite, whence he concluded that it was the more considerable of the two in point of magnitude *. It appeared from his observations, that both satellites were subject to great variations of brightness. It happened, in consequence, that the exterior satellite was sometimes more distinctly visible than the interior. Herschel concluded, from this variation in the brightness of the two satellites, either that there existed dark tracts on their surfaces which were turned in succession towards the earth by the rotation of the bodies on their axes, or that each satellite was enveloped in an atmosphere which, by its fluctuating movements, occasionally laid bare the dark surface of the satellite, as in the case of the Sun, Jupiter, and Saturn †.

Herschel found that both satellites invariably disappeared when they arrived within a certain distance of the planet. The limit of visibility, however, was different for each satellite. The interior satellite generally vanished at a distance of 18″ from the planet. The exterior satellite ceased to be visible at the distance of 20″ ‡. A dense atmosphere enveloping the planet would explain the disappearance of the satellites, did it not happen that they were lost sight of, when traversing the nearest half of their orbits as well as the opposite half. The only satisfactory explanation which could be given of the phenomenon was, that the feeble light of the satellites was overpowered by the superior lustre of the planet. Herschel remarked, that, owing to a similar cause, the Earth, Venus, and Mercury, would for ever remain invisible to the inhabitants of Uranus, being constantly lost in the effulgence of the Sun's light. It is interesting to trace the progress of optical science (or rather its application to practical purposes) in connexion with the question of the visibility of the smaller bodies of the planetary system. Huyghens generally lost sight of the satellite discovered by him, two days before its arrival in conjunction with the planet, and did not again perceive it until two days after conjunction §. Cassini was unable to perceive the satellite immediately interior to that of Huyghens, except at its greatest elongation from the planet, even when its orbit was so open that at the time of conjunction it passed quite clear of both the planet and the ring ‖. Sir William Herschel, however, succeeded in keeping sight of every one of the seven satellites of Saturn until they actually arrived in contact with his disk ¶. Perhaps some future observer may succeed in accomplishing for the satellites of Uranus what that illustrious astronomer achieved in respect to those of Saturn **.

The satellites of Uranus require telescopes of such a high degree of perfection to render them barely discernible that, after Sir William Herschel ceased to direct his attention to these remote bodies, any further observations of them were for a long time neglected. This remark applies

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[*] Phil. Trans., 1798, p. 78.

[†] Ibid., 1815, p. 356.

[‡] Ibid., 1798, p. 76; ibid., 1815, p. 355.

[§] Opera Varia, tom. ii., p. 524.

[‖] Anc. Mém. Acad. des Sciences, tome x., p. 586.

[¶] Phil. Trans., 1790, Part I., p. 7; also 1798, p. 74.

[**] It results from the invisibility of the satellites of Uranus near conjunction, that the eclipses of these bodies (which, on account of the great inclination of their orbits can only take place on the occasion of the passage of the planet through either of the nodes) will necessarily be invisible also.

not merely to the four satellites alluded to by Herschel at a later period of his researches, the existence of which might be regarded as problematical, but also to the two satellites originally discovered by him in 1787, the theory of whose motions he had brought to a state of considerable perfection. It does not appear that any astronomer obtained even a sight of these faint objects until the year 1828, when Sir John Herschel detected them with a 20-feet reflector. In order to verify the general character of the views of his illustrious father, relative to the orbits of the two satellites, and to obtain a fresh correction of their periodic times, the same astronomer executed a series of observations of their angles of position with the meridian, throughout the years 1830, 1831, and 1832. By a skilful discussion of his father's observations of the second satellite, and a similar treatment of his own, he obtained two epochs of the passage of the satellite through the ascending node of its orbit, which embraced an interval of 15122^d. 4^h 17^m, and included 1737 revolutions of the satellite. Having calculated the duration of 1737 revolutions of the satellite, from the periodic time, as determined by his father, he found it to be 15121^d 15^h 42^m 32^s.4. This was less than the time deduced from the observations, by 12^h 34^m 28^s. It followed, therefore, that the assumed period was a little too short; and from the number of revolutions comprehended between the two epochs, it was easy to conclude, that the time of each revolution required to be lengthened by 26^s.06. In this manner Sir John Herschel obtained 8^d 16^h 56^m 31^s.3 for the corrected period of the satellite. By pursuing a similar process with respect to the fourth satellite, he determined its periodic time to be 13^d 11^h 7^m 12^s.6. In this case, therefore, his father's period required to be shortened by 1^m 46^s.4. The positions of the satellites, when calculated from their corrected periods, and the inclination and node of their orbits, as determined by Sir William Herschel, exhibited an accordance with the corresponding observations sufficiently close to establish the general accuracy of the theory of both bodies. Sir John Herschel found that the observations of the second satellite afforded pretty strong indications of ellipticity. He also considered it very probable that the data of the fourth satellite might prove to be equally indicative of elliptic motion, although it would be premature to attempt the investigation of this part of the subject*.

The next astronomer who directed his attention to the satellites of Uranus was M. Lamont, of Munich, who, in the autumn of 1837, made several accurate observations of the second and fourth satellites with a refractor of 15 feet focal length and 10½ inches aperture. The main object of his observations was to determine the greatest elongations of the satellites, with the view of arriving at an accurate knowledge of the mass of the planet. He did not, however, omit to derive a new determination of the periodic times of both satellites, by a comparison of his own observations with those of Sir William Herschel and his son. By means of a series of micrometrical measures he determined the greatest elongation of the second satellite to be 31″.35, and that of the fourth to be 40″.07. He also obtained 8^d 16^h 56^m 28^s.5, and 13^d 11^h 7^m 5^s.9 for the times of the synodic revolutions of the two satellites†.

M. Lamont states, in his memoir, that he saw the most distant of the six satellites of Uranus on the 1st of October, 1837. This was the only verification that had been hitherto obtained of Sir William Herschel's observations relative to the existence of other satellites of the planet besides those discovered by him in 1787. Additional light has been thrown upon this interesting subject by the observations of astronomers during the last few years. The *first* satellite, or the nearest of the six to the planet, has been repeatedly observed by Mr. Lassel in this country, and also by M. O. Struve in Russia. Mr. Lassel first saw the satellite on the 14th of September, 1847. The observations of M. Otto Struve date from the 8th of October of the same year. The Rev. W. R. Dawes having instituted a comparison between the results of both these astronomers, came to the conclusion that they are incompatible with each other upon any supposition whatever relative to the elements of the satellite, and upon this ground he has suggested the probability of there being two satellites instead of one, revolving within the interior of the two old satellites. By a discussion of the observations of Mr. Lassel, he found that the apparent distance of the satellite is only 11″, and the time of its revolution $2^d\ 2^h\ 43^m\ 6^s$. M. Otto Struve, from his own observations, deduced 17″.5 for the distance of the satellite, and $3^d\ 22^h\ 10^m$ for the time of its revolution. The hypothesis of the existence of two distinct satellites derives additional support from the fact that Mr. Lassel constantly saw the satellite on the north side of the planet, while M. Otto Struve, on the other hand, invariably saw it on the south side. The latter of these astronomers has suggested that the satellite observed by him, like the fifth satellite of Saturn, might lose much of its light in the northern half of its orbit. Of course a similar remark is applicable, *mutatis mutandis*, to the satellite observed by Mr. Lassel. It would appear, therefore, that, besides the observations of Mr. Lassel and M. Struve being irreconcilable with each other, in so far as the motion of the body is concerned, there exists an essential distinction between the indications which they severally afford, of the physical constitution of the satellite.

The third, or intermediate satellite, of Sir William Herschel, included between the two satellites of 1787, was observed by Mr. Lassel on the 6th of November, 1847. No indications of its existence appear to have since been obtained by any observer. It is clear that much yet remains to be done before anything like a satisfactory view of the Uranian system can be arrived at. In the meantime it offers to the astronomer an interesting field of observation and research; while it holds out in still more distant prospective to the geometer, the grounds of one of the most curious problems in physical astronomy.

The telescopic discovery of the planet Neptune by Dr. Galle, to which allusion has been made in one of the preceding chapters, was soon followed by the discovery of a satellite of the planet. The science of astronomy is indebted for this interesting acquisition to Mr. Lassel, whose important discovery in connexion with the Saturnian system (although subsequent to the present in the order of time) we have already had occasion to mention. On the 10th of October, 1846, Mr. Lassel, while engaged in observing Neptune, perceived a small star near it which he suspected to be a satellite, but as the planet was soon afterwards lost in the rays of the sun, he was unable to obtain an assurance on this point until the following year. Observations made on the 8th and 9th of July, 1847, proved to him, beyond all doubt, that the object he originally perceived was in reality a satellite, and this conclusion was further strengthened by repeated observations of it during the course of the two following months. It was also observed soon afterwards, at Pulkowa, by M. Otto Struve, and at Cambridge, U. S., by Prof. Bond. This inte-

20 feet focal length, and 2 feet aperture, which he had constructed with his own hands. It was the first reward that crowned his successful exertions to provide himself with the means of effectually exploring the heavens, and it was one to which his ingenuity and perseverance had well entitled him *.

As the satellite of Neptune supplies a method for determining the mass of the planet, independent of that founded on the perturbations occasioned by the planet, its periodic time and greatest elongation are elements the ascertainment of whose true values is manifestly an object of great importance. Prof. Pierce of Harvard College, U. S., by combining the observations of Messrs. Lassel and Bond, has found the periodic time of the satellite to be $5^d\ 21^h\ 12^m.4$, and its greatest elongation to be $16''.5$. The corresponding results which M. Otto Struve has deduced from his own observations of the satellite, do not exhibit so close an agreement with these numbers as is desirable. According to that astronomer the periodic time of the satellite amounts to $5^d\ 21^h\ 15^m$, and its greatest elongation to $18''$. These different determinations of the periodic time and distance of the satellite, of course, indicate different values of the mass of the planet. The element which offers the principal difficulty to the astronomer is the distance. Perhaps some time may elapse before its value will be ascertained with sufficient precision, to justify the hope of deducing from it the true value of the mass of the planet, an element of so much importance in all researches relative to the movements of those bodies upon which the planet exercises a sensible influence. From the observations of the satellite, it appears that its orbit is inclined to the plane of the ecliptic at an angle of about 35°, but it has not yet been ascertained whether the motion is direct or retrograde.

Neptune offers an interesting analogy to the Earth, arising from the circumstance of its being attended by only one satellite. When, however, the rapid advancement of astronomical science in recent times is taken into consideration, it will readily be admitted, that it would be unsafe to reckon upon the permanence of this analogy. Huyghens upon his discovery of a satellite revolving round Saturn alluded with conscious exultation to the resemblance existing between the planet and the earth in this respect, as furnishing an argument in favour of the Copernican system of the world of a much more conclusive kind than that supplied by the satellites of Jupiter †. Saturn, however, had only accomplished half a revolution in his orbit after this remark was made, when Cassini, by his discovery of two additional satellites, put a term to the fancied resemblance between that planet and the earth. It must be admitted that on this occasion at least, the advocate of the immobility of the earth—for such Cassini may be accounted to be—gave an effectual reply to the reasoning of his unwary antagonist.

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* In connexion with the brief allusion in the text to Mr. Lassel's labours, it may not be out of place to cite the following passage from the Report of the Council of the Astronomical Society, setting forth the grounds upon which it was resolved to award to him the gold medal of the Society for the year 1848.—"The simple facts are, that Mr. Lassel cast his own mirror, polished it by machinery of his own contrivance, mounted it equatorially in his own fashion, and placed it in an observatory of his own engineering. A private man, of no large means, in a bad climate, and with little leisure, he has anticipated, or rivalled, by the work of his own hands, the contrivance of his own brain, and the outlay of his own pocket, the magnificent refractors with which the Emperor of Russia and the citizens of Boston have endowed the observatories of Pulkowa and the Western Cambridge."—*Month. Proc. Ast. Soc., Feb.* 1849.

† Opera Varia, tom. ii., p. 531.

Of the eighteen satellites which at present are recognised as belonging to the planetary system, seventeen would have for ever remained unknown, were it not for the aid afforded to human vision by the telescope. Nine of these satellites have been discovered exclusively with the refracting, and seven exclusively with the reflecting telescope. The remaining satellite was discovered by the independent use of two telescopes, one of which was constructed upon the refracting, and the other upon the reflecting principle. Of the satellites discovered with the refracting telescope, four (and these by far the most important of the seventeen) were discovered by the aid of a little instrument resembling in principle the modern opera glass. Five were discovered with telescopes of immense focal length, constructed upon the principle originally recommended by Kepler. The achromatic telescope can boast of only one triumph, and even that is shared with the reflecting telescope. Of the satellites discovered with the reflecting telescope, two were discovered with an instrument of 40 feet focal length, and 4 feet aperture; two, including the satellite discovered also with the refracting telescope, with an instrument of 20 feet focal length, and 2 feet aperture; and four with an instrument of 20 feet focal length and 18 inches aperture.

There is no department of astronomical science which has been cultivated more assiduously in recent times than that relating to the movements of Comets. The highly-improved state of the methods of calculation relative to this subject, enable the astronomer with comparative ease to trace back the motions of comets throughout anterior ages, and by confronting the results so obtained with the contemporary records of ancient comets, in many instances to deduce conclusions of an interesting and valuable character. In this manner, it has been recently found that the famous comet of Halley was observed by the Chinese astronomers on the occasion of its passage of the perihelion in the year 1378. The earliest authentic account of this comet, contained in European records, relates to its passage of the perihelion in the year 1456. Halley suspected that a comet which appeared in 1380 might be identical with the one of 1682, but from the vague terms in which the ancient comet was alluded to, he found it impossible to arrive at any trustworthy conclusions on this point. No doubt, however, can exist with respect to the comet recorded in the Chinese annals. It appears from the researches of M. E. Biot, that a very conspicuous comet was observed in China in the year 1378. It first appeared on the 26th of September, and it continued to be visible during forty-five subsequent days. In the work containing the account of this comet, there is also a minute description given of the route which it traversed among the fixed stars during the period of its visibility. In order to ascertain whether this comet was identical with Halley's, M. Laugier reduced the elements of the latter comet to the perihelion passage previous to that of 1456. The most delicate point of this enquiry was the determination of the time of the passage of the perihelion, which, after various trials, he finally fixed on Nov. 8.77, 1378. Having then calculated the position of the comet on the 26th of September, 1378, and also various positions corresponding to the period included between that date and the 10th of November (on which day the recorded comet was last visible), he found that the apparent path pursued by it agreed exactly with that indicated by the observations of the Chinese Astronomers *. The interval embraced between the perihelion passages of 1378

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* Comptes Rendus, tome xvi., p. 1004; Connaissance des Temps, 1846.

and 1456, is longer than any other period of the comet that has been hitherto determined by observation *.

Besides the comets to which allusion has already been made in connexion with the theory of gravitation, there are several other bodies of the same class which have also been found to revolve in elliptic orbits. It may be remarked that the question whether a comet returns periodically to perihelion, or whether, after having once visited the solar system, it then passes off definitively into the boundless regions of space, may admit of being resolved in two different ways. The elements of two or more comets may be compared together, and if they should present a close resemblance, it may be inferred that each set of elements refers merely to a different return of the same body. This circumstance will, therefore, serve to establish the periodicity of the comet, and the interval included between two *consecutive* apparitions will indicate the time of revolution. It was by means of such a comparison that Halley succeeded in establishing the periodicity of the famous comet which bears his name, and in predicting the time of its next return to perihelion. This was the only mode of determining the time of a comet's revolution which Newton considered to be practicable, for he was of opinion that all comets, without exception, revolve in very elongated ellipses, although for the sake of simplicity it might be assumed that towards the perihelion the path of each comet did not deviate sensibly from a parabola. The great improvements which have been effected in the methods of calculation since Newton's time have emboldened astronomers in many instances to announce the periodicity of a comet, and to assign the time of its revolution by means of observations made on the occasion of only one apparition. This method is undoubtedly more direct and more generally applicable than that founded on the comparison of different apparitions; but the results obtained by the employment of it have seldom turned out to be satisfactory. The errors of observation bear so large a proportion to the deviations from parabolic motion, which, in this case, form the real data of the problem, and the arc described by the comet, during the period of its being visible, is usually so small, that the values assigned to the periodic time by different computers, have frequently exhibited a great discordance. With respect to many of those comets to which elliptic orbits have been assigned, their movements admit of being equally well represented by parabolas. It is only in those cases in which it is found to be impossible to reconcile the actual motion of the comet with any parabola whatever, that the application of the elliptic method may be expected to lead to trustworthy results.

The great comet of 1680 has been supposed to revolve in an elliptic orbit. Halley, by comparing it with several ancient comets, was induced to conclude that it accomplished a complete revolution in 575 years. Euler and Pingré, severally, calculated its period from the observations of 1680; but the results at which they arrived differed materially

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* The following are the periods which have been derived from the recorded observations of the comet.

From 1378 to 1456	77.58 years.
1456 „ 1531	75.21
1531 „ 1607	76.15
1607 „ 1682	74.91
1682 „ 1759	76.49
1759 „ 1835	76.68

The mean of these periods is 76.1 years. *Comptes Rendus*, tome xvi. p. 1006.

from each other, as well as from the period which Halley had deduced by the light of historical records.

In the year 1264, A. D., there appeared a splendid comet, which was visible both in Europe and China. From the resemblance of its apparent path to that of a comet observed in 1556, it has been concluded that the two bodies are identical. Such is the view of the subject which has been taken by Pingré, Lalande, and various astronomers of the present day. Mr. Hind, by a skilful discussion of the recorded observations, has arrived at results which render still more probable the identity of the two comets. If this be true, the comet ought to return again to perihelion about the year 1848, the period being somewhere about 292 years. Such would be the case if the motion of the comet was effected solely by the central action of the sun. It is necessary, however, to take into account the effects of planetary perturbation in determining the precise time of return to perihelion. This arduous task has been recently executed by M. Bomme, a geometer of the Netherlands, who has found that the action of the planets will retard the comet's arrival in perihelion until the year 1858 or 1860 [*]. It is to be hoped that the expected stranger will, in due time, respond to the elaborate calculations of which it has formed the subject.

The great comet of 1811 was found by Bessel to have a period of 3383 years. Several other astronomers who have calculated the elliptic elements of this comet have obtained results agreeing very nearly with each other and also with that deduced by Bessel. Comets which take such an immense length of time to accomplish a single revolution, lose much of the interest which the periodic character of their movements is otherwise calculated to excite. The case is quite different with respect to such comets as those of Encke, Biela, Faye, and De Vico, which may be expected frequently to return to perihelion during the average period of human life. Two other comets have recently been found to revolve in elliptic orbits of comparatively small dimensions. One of these was discovered by M. Brorsen, on the 26th of February, 1846. Its period has been determined to be about five years and a half. The last passage of the perihelion took place on the 25th of January, 1846. The other comet of short period was discovered by M. Peters, on the 26th of June, 1846. It has been found by M. D'Arrest that the observations of this comet are capable of being represented by an elliptic orbit, with a period of 5804 days, or about sixteen years.

The most splendid comet of the present century was that which appeared in the early part of the year 1843. The following elements of its motion, calculated by Sig. Capocci, will serve to give an idea of the orbit in which it moves. They have been selected for this purpose on account of the perihelion distance coinciding almost exactly with the mean of the perihelion distances arrived at by various other astronomers who have computed the elements of the comet:—

Passage of the perihelion, February 27.5643	
Perihelion distance	0.00538
Longitude of the Perihelion	277° 52′ 35″
Longitude of ascending Node	354° 48′ 50″
Inclination	35° 56′ 55″

Motion Retrograde.

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[*] Mr. Hind's elements of the comet of 1556, when employed as the basis of calculation, fix the passage of the perihelion in the year 1858. Halley's elements make the passage two years later.

It appears from the above value of the perihelion distance that the comet approached nearer the sun than any other comet recorded in history. The great comet of 1680 had been hitherto the most remarkable in this respect of all those comets whose elements had been determined, having approached the sun's surface within one-third of his diameter. The least distance of the comet of 1843 from the sun's surface did not amount to more than one-seventh of the solar diameter. The comet, therefore, approached twice as near the sun as the comet of 1680.

The immense velocity of the comet of 1843, when revolving in the neighbourhood of the sun, arising from the smallness of its perihelion distance, occasioned some extraordinary peculiarities in its motion. Thus between the 27th and 28th of February it described upon its orbit an arc of 292°. Supposing it to revolve in an elliptic orbit, this would leave only 68° to be described during the time which elapses until the next arrival in perihelion. On the evening of the 27th of February it described the whole of the northern part of its orbit, having occupied only $2^h\ 11^m$ in passing from the ascending to the descending node. It was twice in conjunction with the sun on the 27th of February. The first conjunction took place at $9^h\ 24^m$ P. M. The comet was then revolving beyond the sun. The second conjunction took place at $12^h\ 15^m$ P. M. The comet was then passing between the sun and the earth. The time at which this interesting event occurred, prevented astronomers from ascertaining whether the comet would have been visible when projected upon the solar disk *.

It has been suspected that the comet of 1843 is identical with another great comet which appeared in the same region of the heavens in the year 1668, and which was visible at San Salvador in Brazil, at Bologna, and Lisbon. On the evening of the 5th of March the comet was seen at San Salvador by Father Valentine Estancel. It then appeared a little above the western horizon. The tail measured 23° in length, and resembled a huge beam of light, extending nearly in a horizontal direction from west to south. Its light was so vivid that persons standing on the shore were enabled to discern its reflexion on the sea; but the head was so small as to be scarcely visible †. The comet was observed by the same individual on the 7th of March. Cassini observed it at Bologna on the 10th and 14th of the same month. Drawings of the direction of its tail, as determined by him on those days, were afterwards communicated by Maraldi to the Academy of Sciences ‡. It is a remarkable fact that if this comet be supposed to have passed its perihelion on the 27th of February, 1668, the Brazilian observations, as well as those of Cassini, will be represented with sufficient accuracy by the orbit of the comet of 1843. This would assign to the comet a period of exactly 175 years.

The elements of the comet of 1843 presented also a strong resemblance to those of a comet observed in 1689. The most discordant element was the inclination, which, in the case of the latter comet, amounted to 69° 17′ according to the calculations of Pingré. The same astronomer found the passage of the perihelion to have taken place on December 1.6, 1689. Sig. Capocci, however, remarked, that if the comet be supposed to have passed its perihelion on December 3, all the recorded observations will be accurately satisfied by the elements of the comet of 1843.

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* Comptes Rendus, tome xvi., p. 642.

† Cométographie, tome ii., p. 22.

‡ Mém. Acad. des Sciences, 1702, p. 107.

Hence, in order to reconcile this conclusion with that arrived at relative to the comet of 1668, it would be necessary to diminish the period of the comet to 21 years. But there are other comets which astronomers have suspected to be identical with the comet of 1843, especially those of 1618 and 1702. Capocci, therefore, suggested the probability of the period of the comet being somewhere about seven years, remarking that this hypothesis would afford a satisfactory account of numerous apparitions of comets recorded in history. The opinions of this astronomer have received a remarkable confirmation from the subsequent researches of Prof. Pierce and M. Clausen. Prof. Pierce having recomputed the elements of the comet of 1689, obtained 30° 25′ for the inclination of the orbit. This result agreed almost exactly with that obtained for the inclination of the comet of 1843. He also found the passage of the perihelion to have taken place on December 2.1463, 1689. M. Clausen having discussed the totality of the observations of the comet of 1843, found that they would be best represented by supposing the comet to revolve in an elliptic orbit, the period of which he fixed at 6.8 years. It is not easy to reconcile this conclusion with the fact of the comet not having been more frequently recognised on former occasions of its return to perihelion. This circumstance may be accounted for in some instances by the peculiar position of the orbit of the comet, in others, perhaps, by changes in its physical constitution, which may exercise a material influence on its visibility.

The physical constitution of comets forms a subject of research at once so varied and extensive that it has been deemed expedient to devote a separate chapter to an account of the labours of astronomers in connexion with it.

CHAPTER XV.

General Aspect of Comets.—Translucency of Cometic Matter.—Structure and Dimensions of the Envelope.—Description of the Tail.—Its Direction and Curvature.—Peculiarities of Structure.—Dimensions.—Phenomena Observed during the Passage of Comets through their Perihelia.—Comet of Halley.—Comet of 1799.—Variation of the Volume of Comets.—Hevelius.—Newton.—Struve.—Herschel.—Dissolution of Comets.—Historical Statement of Ephorus.—Comet of Biela.—Developement of the Tail.—Comet of 1680.—Comet of 1769.—Anomalous Appearances in the Tail.—Instances of Remarkable Comets.—Hypotheses respecting their Physical Constitution.—Theories of the Variation of a Comet's Volume.—Newton.—Valz.—Herschel.—Theories of the Tails of Comets.—Kepler.—Newton.—Electrical Theory.—Light of Comets.—Appearance of Phases.—Cassini.—Cacciatore.—Polarization of the Light of Comets.—Researches of Arago.—Question respecting the Solidity of Comets.—Newton.—Laplace.—Smallness of a Comet's Mass.—Ultimate condition of Cometary Bodies.—Opinions of Newton, Laplace, and Herschel on this point.

THE question relative to the constitution of Comets is one of the most interesting in the whole range of celestial physics. There is something in the sudden apparition and strange aspect of these bodies which is calculated to arrest the attention of the most careless observer of nature. In former ages they were regarded with superstitious dread, as manifestations of divine displeasure, and the harbingers of impending calamities. Every feature in their appearance was gazed upon with intense anxiety, and was vanced state of civilization has led to juster views of celestial phenomena, it has become an interesting branch of study to enquire into the nature of comets, and the purposes which they fulfil in the economy of the material universe. And although it must be acknowledged that little progress has hitherto been made towards obtaining a satisfactory solution of these questions, still a multitude of facts have been disclosed by the observations of astronomers, which have formed the groundwork of much ingenious speculation, and have tended to throw some degree of light on the mysterious subject to which they relate.

Comets exhibit various peculiarities by means of which they are readily distinguishable from other bodies. The light by which they shine is generally pale, compared with that of the planets or stars. The principal part of their structure is the head, which presents the appearance of a nebulous body, more or less condensed towards the centre. Within the head a bright point is seen, approaching in lustre to a planet. This is called the nucleus of the comet. The nebulosity of the head is surrounded by a hazy contour, termed the coma. But the most remarkable part in the structure of a comet is the tail, which appears as a long train of light issuing from the head. Although these are the more prominent features by which comets are in many instances characterised, they are found actually to exhibit a great variety of aspect. Multitudes of comets, which are visible only in telescopes, present neither tail nor nucleus; but merely resemble a round mass of vapours slightly condensed towards the centre. Even of those comets which are visible to the naked eye there have been some which did not exhibit the usual features of these bodies. The comet of 1585, observed by Tycho Brahé, possessed neither tail nor coma, but appeared perfectly round like a planet *. Various examples of the same sort are to be found in the records of astronomical observation.

The substance of which comets are composed is characterised by an extreme degree of tenuity. This has been inferred from the fact that stars have been seen through them without exhibiting any sensible diminution of lustre. Even as early as the time of Seneca it was remarked that stars were occasionally visible through the substance of comets. It might well be supposed that the rude observations of ancient times would be incapable of indicating delicate alterations of brightness, even although they should exist. Modern astronomers, however, universally agree in representing the translucency of cometic matter to be in most cases so perfect as to offer no obstacle to vision. Numerous observations might be cited in confirmation of this fact. Thus, on the 9th of November, 1795, Sir William Herschel observed the comet of that year pass centrally over a telescopic double star of the eleventh or twelfth magnitude; but, notwithstanding this circumstance, the smaller of the two component stars continued, during the whole time of the transit, to be distinctly visible †. Olbers perceived a star of the tenth magnitude through the comet of 1802; but he was unable to persuade himself that it exhibited any diminution of its usual brightness. On the 7th of November, 1828, M. Struve perceived a star of the tenth magnitude within a few seconds of the brightest part of Encke's comet; but the light of the star did not seem to be enfeebled in the slightest degree. Sir John Herschel, in a

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* Planè rotunda extitit; nec ullam caudam aut barbam in unam magis quam in aliam partem portendebat. (*Epist. ad Landgrav.* p. 13.)

† Phil. Trans. 1796.

paper on Biela's comet, which appears in the "Memoirs of the Astronomical Society," has mentioned a fact which strikingly illustrates the transparency of the substance of which comets are composed. The comet having passed over a small cluster of stars of the sixteenth or seventeenth magnitude, the appearance presented was that of a nebula, partly resolvable into stars. The most trifling fog would have effaced the stars; but in the present instance they still continued to be visible, although the cometic matter interposed between them and the observer must have been at least 50,000 miles in thickness*. It might be expected that a star when seen through the substance of a comet would suffer an apparent displacement, from the refraction exercised by the cometic matter upon the rays of light proceeding from the star to the observer. No such phenomenon, however, has ever been detected by any astronomer. An important observation was made by Bessel, with the view of deciding this interesting point, on the occasion of the last apparition of Halley's comet. On the 29th of September, 1835, a star of the tenth magnitude approached within 7″.78 of the nucleus of the comet, but its apparent position was not affected in the slightest degree†. It is manifest from this circumstance that, unless the matter of which a comet is composed differs in its essential properties from other material substances, it must possess a degree of tenuity which is quite inconceivable.

The nebulosity in which a comet appears enveloped is, in some instances, separated from the head by a dark space, indicative of a transparent atmosphere. This formed one of the most striking features of the great comet of 1811‡. On the side turned towards the sun there appeared a semicircular ring of light enveloping the head, but maintained quite distinct from it by a dark interval of uniform breadth, through which small stars were visible. The opposite extremities of the luminous semicircle extended beyond the head in two slightly-diverging streams of light, occasioning the appendage of the tail. By an attentive examination of the different parts of the comet during the period of its visibility, Sir William Herschel was conducted to a remarkable conclusion respecting its structure. From the circumstance of the semicircular envelope constantly exhibiting the same appearance, he inferred that it was in reality a hollow hemisphere; for he justly remarked that, if it was similar in structure to the ring of Saturn, it could not fail to undergo a sensible change of apparent form, in consequence of the variable position which it assumed, arising from the combined motions of the earth and the comet. The darkness of the space between the luminous semicircle and the head arose from the line of vision not traversing a sufficient depth of the nebulous matter to render that part of the envelope visible. This will appear manifest when it is considered that the hemispherical envelope was viewed towards the centre in a direction almost perpendicular to its surface, whereas in those parts where the semicircle was visible, the line of vision, being a tangent to the surface of the envelope, traversed a much greater depth of matter, and thereby occasioned a greater concentration of light. From the transparency and uniform breadth of the dark interval which separated the envelope from the head, Herschel concluded that the comet was encompassed by an elastic atmosphere. In accordance with this supposition, the semicircular ring of light would represent a stratum of nebulous matter suspended in the atmosphere of the comet, at a considerable distance from the head.

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* Mem. Ast. Soc., vol. vi., p. 99, et seq.

† Connaissance des Temps, 1840, Addit., p. 97.

‡ The year 1811 was distinguished by the apparition of two comets, of which by far the more conspicuous was that which first became visible. This is the comet to which allusion will be made on every future occasion, when speaking of the comet of 1811,

Observations of many other comets lead to the belief that they essentially resemble in structure the comet of 1811, but in every case the apparent form is modified by circumstances depending on the peculiar constitution of each body. Thus although the dark interval interposed between the envelope and the head was very apparent in the comet of 1811, yet in most instances such a phenomenon is not discernible. Its absence may be accounted for by supposing the stratum of nebulous matter to be of sufficient depth to cause it to be everywhere visible. Hence, although it be suspended at a considerable elevation above the more condensed portion of the comet, the intermediate space will not be anywhere apparent.

The extreme tenuity of the matter composing the head and the surrounding nebulosity of comets has been already mentioned. On the other hand, the dimensions to which these parts swell out is frequently enormous. Herschel found that the head of the comet of 1811 had a diameter of 127,000 miles *. The diameter of the envelope was found by the same astronomer to be at least 643,000 miles †.

It has been stated that within the head of a comet there is usually a bright point termed the nucleus. This is the only part of its structure which excites any suspicion of a solid substance. Some astronomers, however, maintain that the nucleus is composed of nebulous matter, like the other parts of the comet, differing from them only in the superior condensation of its particles. An observation of the comet of 1825, by Sir John Herschel, tends to support this opinion. Having examined the comet with a 10-feet Newtonian telescope, he perceived a stellar point which seemed to indicate a solid nucleus. When he sought, however, to obtain a stronger assurance on this point, by the aid of a 20-feet reflector, the illusion was speedily dispelled. The comet now assumed the appearance of a milky irresolvable nebula, the centre exhibiting a round disk, with a brightness equal almost to that of a planet, but without a definite outline or the least suspicion of a brilliant point ‡. Bessel's observations of Halley's comet conducted him to a similar conclusion. When the comet was examined with a low magnifying power, there appeared within it a bright nucleus of measurable dimensions, but when a higher power was applied to the telescope, no such phenomenon was any longer perceptible §.

It would seem from the observations of astronomers, that any indication of a solid substance presented by cometary bodies is invariably dispelled when the comet is examined with a magnifying power adequate to disclose its nebulous structure. A decisive test of the real nature of the nucleus of a comet would be afforded by an observation of its transit over a star. Phenomena of this kind are of very rare occurrence; but instances have been mentioned by astronomers which go to support the opinion that the nucleus of a comet is composed of a solid substance. Messier has stated, that on the 18th of August, 1774, while engaged in observing the comet

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* Phil. Trans., 1812, p. 121.

† Phil. Trans., 1812, p. 123.

‡ Mem. Ast. Soc., vol. ii., p. 486, et seq.

§ Connaissance des Temps, 1840, Addit., p. 81.

which Montaigne had recently discovered, he perceived a very small star near it, and that after the lapse of about four hours another star became visible in its vicinity. He remarked that the circumstance of the one star being seen before the other could not be accounted for by supposing the second star to have been originally obscured by the glare of the moon's light, since it was equally bright with the first, and he, therefore, came to the conclusion that it must have been actually hid behind the solid nucleus of the comet *. Wartmann has also mentioned that in the year 1828 he witnessed the extinction of the light of a star of the eighth magnitude by the passage of Encke's comet over it. Neither of these observations have been deemed sufficiently trustworthy to warrant the conclusion that the nucleus of a comet is in any instance a real indication of a solid body. On the other hand, there is no decisive proof that the nucleus is wholly of nebulous structure, for in all those cases that have been cited to demonstrate the extreme tenuity of cometic matter, there has either been no appearance of a nucleus at all, or the comet has not passed centrally over the celestial body. Bessel is of opinion that until some unequivocal transit of the nucleus of a comet over a star be observed, it will be impossible to arrive at any positive conclusion respecting its physical structure †.

Attempts have been made to determine the absolute magnitude of the nucleus of a comet by the measurement of its apparent diameter. In this manner Schroeter found that the nucleus of the comet of 1799 had a diameter of 373 miles. Sir William Herschel, by a similar process, found that the nucleus of the comet of 1807 had a diameter of 538 miles. The same astronomer computed that the nucleus of the second comet of 1811 measured 2637 miles in diameter. These results are very uncertain, in consequence of the smallness of the angles which the nuclei of comets usually subtend; moreover, the phenomenon of a nucleus may be wholly illusory, as certain observations already cited would seem to indicate.

It has been stated that the tail of a comet usually appears as an extension of the nebulosity enveloping the head. The two luminous streams composing it continue to appear quite distinct even at some distance from the head, but towards their extremities they gradually become confounded together by means of scattered light pervading the space between them. An attentive examination of the appearance presented by the tail of the comet of 1811, during the period of its visibility, led Sir William Herschel to a conclusion respecting its structure, analogous to that which corresponding observations of the envelope had suggested to him. He found, in fact, that the two luminous streams proceeding from the opposite sides of the envelope continued to form the bounding sides of the tail in every position in which the comet was viewed. This is the consequence that would ensue if the tail had been in reality composed of a hollow cone of matter attached at its narrower extremity to the hemispherical envelope; for the line of vision, being a tangent at the sides, traversed a much greater depth of matter than towards the middle, where it was perpendicular to the surface of the cone.

The direction in which the tail extends relatively to the head is generally opposite to that in which the sun is situated. Appian is the first of European astronomers who remarked this fact, having been led to its discovery by his observations of the comet of 1531, and of four other comets which appeared between 1531 and 1539. Strictly speaking, indeed, the axis of the tail does not coincide exactly with the prolongation of the line joining the sun and the comet. It generally inclines towards the region in which the comet has been recently moving, and as the inclination increases with the distance from the head, it will necessarily exhibit a slight curvature. This deviation of the tail from a rectilinear direction is more especially remarkable when the comet is near the perihelion. It has been generally found, also, that the tail appears brighter and better defined on the convex or preceding side, than it does on the opposite side*.

Although Appian has undoubtedly the merit of being the first of European astronomers who discovered that the direction of the tails of comets is usually opposite to that of the sun, it appears from the researches of the late M. Ed. Biot, that the same fact was known to the Chinese astronomers at a much earlier date. In the annals of the Dynasty of Thong, which reigned in China between the years 618 and 907 of the Christian era, there is an account of a comet which appeared on the 22nd of March, 837, and the following days. The account concludes with this remark, "In general when a comet (literally a broom) appears in the morning, the tail extends towards the west; when it appears in the evening it extends towards the east. This is a constant rule."†

Comets have occasionally been observed, the tails of which have been characterised by remarkable peculiarities; Chésaux states, that the comet of 1744 had six tails spread out in the form of an immense fan. According to Bessel, the comet of 1807 had two tails, one making an angle of 8° with the prolongation of the radius vector; the other, which was fainter, making an angle of 29° with it. The comet which appeared towards the close of 1823 had two tails, one extending in the usual direction, the other turned almost towards the sun. Lateral tails have also been occasionally seen. Thus, during the first three days of March, the great comet of 1843 was observed at Chili to have a lateral tail issuing from the original one at a distance of 10° from the head, and extending to a much greater length than the other ‡.

The tails of comets frequently exhibit an imposing aspect in consequence of their immense apparent lengths. According to Longomontanus, the great comet of 1618 extended over an arc of the heavens equal to 104°. The tail of the comet of 1680 measured 90° at Constantinople. Chésaux states that the six tails of the comet of 1744 varied in length from 30° to 44°. According to Pingré, the tail of the comet of 1769 appeared 97° long in tropical countries. The tail of the comet of 1843 was observed, under the same favourable circumstances, to extend over an arc of 65°.

It is to be remarked that the foregoing measures cannot be considered as possessing the character of definitive results. The tail of a comet is, in fact, found to vary in apparent length in different climates, indicating thereby that its aspect depends, to a certain extent, on the state of the atmosphere through which it is viewed. Thus, at Paris, the tail of the comet of 1680 was 62° long; at Constantinople it was 90°. The tail of the comet of 1769 was 43° long at London on the 9th of September. On the same day it was 55° long at Paris; at the Isle of Bourbon it was 60°; at Teneriffe it was 75°. On the 11th of September it was 90° at sea; at the Isle of Bourbon it was 97°*. The tail of the comet of 1843 did not measure more than 40° in England or France; in tropical countries it was found to extend over an arc of 65°.

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* This phenomenon appears to have been first remarked in the great comet of 1618. Wendelin, in a passage cited by Hevelius, describes it with great perspicuity in the following terms:—"Cauda pro motu capitis tracta, instar cornu incurvabatur modicè, atque ad eam partem quâ findebat sibi ætherem planè glabrâ densior erat ac compactior; retrò vero solutior ac villosior." (*Cometographia*, p. 455.)

† Comptes Rendus des Sciences, tome xvi., p. 751.

‡ Ibid., tome xvii., p. 362.

The absolute lengths of the tails of some comets are still more calculated to excite surprise than their apparent lengths. The tail of the great comet of 1680 was 96 millions of miles long. The tail of the comet of 1769 had an absolute length of 38 millions of miles. According to Herschel, the tail of the comet of 1811 must have been more than 100 millions of miles long on the 15th of October. The tail of the comet of 1843 attained a length of 150 millions of miles.

The apparent breadth of the tail of a comet seldom exceeds a few degrees. Each of the six tails of the comet of 1744 was 4° broad. The tail of the comet of 1811 was found by Herschel, on the 12th of October, to have a maximum breadth of 6° 45′. Small as these measures may appear to be, they indicate absolute dimensions of very considerable magnitude. Thus Herschel found that, on the 12th of October, the greatest annular section of the tail of the comet of 1811 had an absolute diameter of 15 millions of miles†.

It is obvious, from the foregoing results, that the volumes of the tails of comets must be enormous. Nor is there any difficulty in accounting for such vast emanations from the comparatively small dimensions of the head, for such is the excessive tenuity of the substance composing the tail of a comet, that a very small quantity of matter would suffice for its production. Newton, with the view of illustrating this point, calculated that if a globe of common atmospheric air, one inch in diameter, was expanded so as to have an equal degree of rarity with the air situated at an elevation above the earth's surface equal to the earth's semi-diameter, it would fill the whole planetary regions as far as the sphere of Saturn, and would even extend a great deal farther‡.

The phenomena exhibited by one of the more conspicuous comets during the period of its apparition afford unequivocal indications of the powerful influence exercised by the sun upon the physical constitution of these bodies. When a comet first becomes visible previous to its passage of the perihelion, it usually presents the appearance of a pale nebulous body with a point more or less bright in the centre, but without any trace of coma or tail. As it continues to approach the sun the nebulosity becomes more apparent, and the head exhibits an increased degree of brightness, on the side which is turned towards the sun, subject, however, to variations of an exceedingly irregular character. The tail at the same time comes into view, and gradually increases in length. After the passage of the perihelion, the same succession of changes occurs in a reverse order, the comet finally assuming the appearance presented by it when it first became visible, and soon afterwards vanishing altogether from observation. The phenomena witnessed on the occasion of the last apparition of Halley's comet, both before and after the passage of the perihelion, are highly interesting and suggestive. Previous to the 2nd of October, 1835, the comet presented merely a round nebulous disk, with a faint nucleus in the centre. When observed by Bessel on the evening of that day, the nucleus was found to have suddenly acquired a high degree of brilliancy, and from it there appeared to issue, on the side towards the sun, a cone of light which, after extending to a short distance from the head, was observed to curl backwards, as if impelled by a force of great intensity directed from the sun. This outstreaming cone continued to be visible, in the form of a luminous sector of a circle, until the 22nd of October. When observed from night to night it was found to vary constantly in magnitude and brightness. The direction of the axis of the cone was also variable. Bessel discovered, by a strict analytical investigation, founded on its observed positions, that it oscillated to and fro on each side of the line joining the comet and the sun. These oscillations were generally very rapid, the lapse of a few hours in some instances sufficing to render them sensible. It is a remarkable fact, that the night on which these interesting phenomena first manifested themselves, was also signalized by the commencement of the tail of the comet. It is impossible to doubt that this appendage derived its origin from the nebulous matter which had been in the first instance raised from the head by a force directed to the sun, and was subsequently impelled by a powerful force in the opposite direction. While these curious phenomena were in course of being developed, the nucleus of the comet exhibited great variations of brightness. On the 12th of October it presented to Bessel a measurable diameter, when observed with a magnifying power of 179°; it did not even lose its planetary aspect until a power of 290° was applied. On the 14th it suddenly became much fainter. With a power of only 90° it lost the appearance of a solid body [*].

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* Cométographie, tome ii., p. 194.

† Phil. Trans., 1812, p. 124.

‡ Princip., lib. iii., prop. 41.

When the comet was observed by Sir John Herschel at the Cape of Good Hope, after its passage of the perihelion, the appearance presented by it was totally different from what it had been a few months previously. When examined through a telescope on the 25th of January, it did not exhibit any trace of a tail, but simply resembled a round nebulous body about 2′ in diameter, surrounded by a coma of great extent. Within the disk there appeared a small bright point, in a position somewhat eccentric, from which there issued towards the circumference, in the direction opposite to that of the sun, a ray of highly condensed light. The comet, in fact, seemed to contain within it another miniature comet, having a nucleus, head, and tail of its own, perfectly distinct from the surrounding nebulosity [†]. As the comet receded from the sun the head began to dilate, while at the same time its light grew fainter; also, in consequence of successive additions to its length, it gradually assumed the form of a section of a paraboloid. The head, or rather the paraboloidal envelope, continued to enlarge with extraordinary rapidity, maintaining all the while the utmost regularity of form and sharpness of outline. At the same time it gradually diminished in brightness, until at length it disappeared simply from want of light to render it visible. In the direction of the axis of the paraboloid a faint elongation finally appeared, indicative of a tail; but no decided manifestation of such an appendage was witnessed on any occasion subsequent to the passage of the perihelion. The nuclear ray continued to emit the same vivid light, increasing in length and constantly maintaining a position coincident with the axis of the paraboloid, until, after some time, it gradually grew fainter, and finally ceased to be visible. The nucleus, on the other hand, which at first did not seem to undergo any change, ultimately exhibited a decided increase of relative brightness. When the comet was last seen, on the 5th of May, 1836, it presented exactly the same aspect as it did when it first became visible in the month of August of the previous year.

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[*] Connaissance des Temps, 1840, Addit., p. 82.

[†] A similar appearance was remarked in the great comet of 1618. The following account of it, extracted from Kepler's "Treatise on Comets," may, perhaps, not prove uninteresting to the reader:—"Et quid probat evidentius eundem effluxum caudæ à capite, quàm illud mirabile, quòd ut in cometa anni 1618, nucleus quidam, interior, solidior, et luminosior; sic in cauda radius singularis, specie medullæ in arbore, à Romanis inter initia in medio, a me et Schickardo posterioribus diebus ad alterum latus, est observatus; quasi ut tota cauda toto capite, sic illa conspicua pars caudæ à conspicuo capitis nucleo de lapsa sit (*De Cometis*, p. 103). The above description presents a remarkable agreement in several points with Sir John Herschel's account of the curious phenomenon witnessed by him in Halley's comet. From the circumstance of the nuclear ray having been seen by Kepler towards one of the sides, instead of in the middle, as at Rome, Pingré seems disposed to believe that it was no other than the ordinary luminous edge of the comet's tail. Without insisting, as an objection to this conclusion, that there was

Phenomena more or less resembling those above described have been found to manifest themselves on the occasion of the passage of all great comets through their perihelia. It seems to be pretty well established, that the more tumultuous changes usually take place during the period when the comet is approaching the sun. Those which occur after the passage of the perihelion appear to be of a more quiescent nature, indicating the gradual relapse of the body into the condition in which it was when originally seen after returning from aphelion. This circumstance clearly points to the sun as the exciting cause of these wonderful changes in the constitution of comets, whatever be the nature of the forces which are called into operation by his agency. Sometimes, indeed, phenomena of a rapidly fluctuating character are observed a little *after* the passage of the perihelion. Such was the case with respect to the comet of 1799, according to Schroeter. This comet passed its perihelion on the 7th of September. Nothing unusual in its appearance was remarked until the 16th of that month, when Schroeter perceived that the nucleus was suddenly reduced to two-thirds of its former size. Between the 20th and 21st of the same month, the surrounding nebulosity had diminished to the extent of one-fourth. On the 22nd the nucleus burst out with renewed splendour, and continued to exhibit the same brilliant appearance until the 25th, when it again became extremely faint. The fact of these changes occurring after the comet had passed its perihelion is, doubtless, to be ascribed in some degree to the peculiar constitution of the comet, which may have rendered it less susceptible to the influence of the sun than is usually the case; but more especially to the circumstance that the accumulated effect of the sun's action may naturally be supposed not to have attained its maximum until a little after the comet had begun to recede from the sun.

It has been stated that the paraboloidal envelope of Halley's comet was

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the ray from the nucleus, that the phenomenon was one of a totally different nature. It is worthy of notice that the ray was first seen on the 30th of November, 1618. Its appearance was, therefore, subsequent to the comet's passage of the perihelion, which, according to Halley, took place on the 8th of the same month. Here, then, is another point of resemblance between the phenomenon and the appearance of Halley's comet in the early part of the year 1836.

observed to increase rapidly in dimensions after the passage of the perihelion. A similar enlargement of the comet of 1652 was found by Hevelius to take place as it continued to recede from the sun. According to the calculations of that astronomer, the linear diameter of the comet had increased, between December 20 and January 12, in the proportion of 1 to 24[*]. He mentions that when it was about to disappear it almost equalled the sun in absolute magnitude.

The curious fact of a comet swelling out in dimensions as it continued to recede from the sun, did not escape the attention of Newton. While admitting it to be true, upon the authority of Hevelius, he remarked, on the other hand, that comets diminish in volume in the course of their approach to the sun, attaining their minimum dimensions a little after the passage of the perihelion. Thus the comet of 1680, in the month of November, appeared like a star of the first or second magnitude; but in the following month it did not exceed a star of the third magnitude[†].

The interesting fact of a variation of the absolute magnitude of a comet depending on its distance from the sun, does not appear to have attracted any attention for a long time after the publication of the Principia. Pingré, in his great work on Comets, denies the truth of Hevelius' statement respecting the enlargement of the comet of 1652 in the course of its recess from the sun, upon the ground that the observations of the comet, when rightly interpreted, do not lead to such a conclusion[‡]. On the other hand, he appears disposed to admit that comets diminish in volume as they approach the sun[§]. Both the alleged diminution previous to the passage of the perihelion, and the subsequent enlargement, have been confirmed by the observations of modern astronomers. M. Struve established beyond doubt, by a series of micrometrical measures, that Encke's comet continually diminished in volume as it approached the sun, on the occasion of its passage of the perihelion towards the close of the year 1828. On the 28th of October, when the distance of the comet from the sun was 1.4617, the diameter of the head was 79.4. On the 24th of December, when the comet had arrived in perihelion, and the distance was only 0.5419, the diameter of the head was 3.1[‖]. The comet, therefore, during the intermediate period, had diminished in linear dimensions in the ratio of 25 to 1, and consequently it had collapsed into about the sixteen-thousandth part of its original volume. Sir John Herschel's observations of Halley's comet are equally conclusive with respect to an enlargement of volume having taken place simultaneously with the recess of the comet from the sun. During the interval embraced between the 25th of January, 1836, and the 1st of the following February, the volume of the comet was found by that astronomer to have increased in the proportion of 1 to 41.605[¶]. The different explanations which have been advanced of the variation of a comet's volume depending on its distance from the sun will be noticed presently.

Since comets in some instances approach nearer the sun than any of the planets, they experience a degree of heat during their passage of the perihelion of which it is difficult to form an adequate conception. Newton calculated that the great comet of 1680, when passing through its perihelion, was subjected to a heat 2000 times greater than that of red-hot iron. The comet of 1843, which approached still nearer the sun, must have been exposed to a heat of even greater intensity. Sir John Herschel has computed that the heat received by its surface during the passage of the perihelion was equal to that which would be received by an equal portion of the earth's surface, if it were exposed to the influence of 47,000 suns, placed at the common distance of the actual sun. He has also shown that the heat to which the comet was subjected on the same occasion must have exceeded the heat concentrated in the focus of Perkins' great lens in the proportion of 24½ to 1; although the heat in the latter instance was so great as to have melted carnelian, agate, and rock crystal[*]! It is not easy to conceive how a flimsy substance such as that of which a comet appears to be composed, can effectually resist such an intense heat so as not to be dissipated in space. The ingenious views of Laplace on this point will be mentioned when we come to speak of the various hypotheses which have been advanced relative to the physical constitution of comets.

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[*] Cometographia, p. 331.

[†] Principia, lib. iii., prop. 41.

[‡] Cométographie, tome i., p. 125.

[§] Ibid., tome ii., p. 193.

[‖] Annuaire, 1832.

[¶] Results of Astronomical Observations

A signal manifestation of the influence of the sun is sometimes afforded by the breaking up of a comet into two or more separate parts on the occasion of its approach to the perihelion. Seneca relates that Ephorus, an ancient Greek author, makes mention of a comet which, before vanishing, was seen to divide itself into two distinct bodies[†]. The Roman philosopher appears to doubt the possibility of such a fact; but Kepler, with characteristic sagacity, has remarked that its actual occurrence was exceedingly probable[‡]. The latter astronomer further remarked that there were some grounds for supposing that two comets, which appeared in the same region of the heavens in the year 1618, were the fragments of a comet that had experienced a similar dissolution. Hevelius states that Cysatus perceived in the head of the great comet of 1618 unequivocal symptoms of a breaking up of the body into distinct fragments. The comet, when first seen in the month of November, appeared like a round mass of concentrated light. On the 8th of December it seemed to be divided into several parts. On the 20th of the same month it resembled a multitude of small stars[§]. Hevelius states that he himself witnessed a similar appearance in the head of the comet of 1661[||].

The foregoing statements respecting the dissolution of comets cannot be admitted without a certain degree of reserve, in consequence of the imperfect nature of astronomical observation in early times. The case is different with respect to Biela's comet, which was observed to separate itself into two parts on the occasion of its approach to perihelion in 1846. The circumstances relating to this remarkable event have been already alluded to in a former chapter[¶]. It is impossible to doubt that it arose from the divellent action of the sun, whatever may have been the mode of operation.

The developement of the tail of a comet is usually regulated by the changes which occur in the head and nucleus. Its formation generally commences when the comet is descending towards the sun, but in most instances it does not acquire its greatest length, nor does it shine with its full splendour, until a little after the passage of the perihelion. This is what might naturally be expected, if the sun be supposed to be the principal agent in the production of the tail. The following account of the progress of the tail of the great comet of 1680 is extracted from Newton's Principia. This comet passed its perihelion on the 8th of December. On the 6th of November it exhibited the aspect of a round nebulous body.

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[*] Outlines of Astronomy, p. 370.

[†] Quæst. Nat., lib. vii., cap. xvi.

[‡] De Cometis, p. 50.

[§] Cometographia, p. 341.

[||] Cometographia, p. 417.

[¶] See p. 136.

On the 11th the tail just became visible. When observed through a ten-feet telescope it appeared half a degree long. On the 17th it was observed at Rome to be more than 15° in length. On the 18th it appeared 30° long in New England. On the 12th of December it was observed at Rome to be 70° long. On the 5th of January its length was found by Newton to be 40°. On the 25th it measured only 6° or 7°, and appeared very faint. On the 10th of February it was only 2° long. On the 25th the comet was seen without a tail, and shortly afterwards disappeared entirely from observation.

The following account of the progress of the tail of the great comet of 1769 will afford a still more striking illustration of the agency of the sun in the developement of these singular appendages, inasmuch as all the observations of the comet were made at the same place and by the same individual. On the 8th of August, 1769, Messier, while engaged in exploring the heavens with a two-feet telescope, perceived a round nebulous body which turned out to be a comet. On the 15th of the same month the tail became visible to the naked eye, and appeared to be about 6° in length. On the 28th it measured 15°. On the 2nd of September it was 36° long. On the 6th it was 49°. On the 10th it was 60°. The comet having now plunged into the rays of the sun, ceased to be visible. On the 8th of October its passage of the perihelion took place. On the 24th of the same month it reappeared, after emerging from the rays of the sun. The tail now measured only 2° in length. On the 1st of November it measured 6°. On the 8th of the same month it was only 2½° long. On the 30th it measured 1½°. The comet henceforward ceased to be visible*.

Sometimes the luminous streams which form the bounding sides of the tail of a comet appear to undergo variations in their length of considerable rapidity and magnitude. Phenomena of this nature were noticed by Herschel in the tail of the comet of 1811. He suspected that they arose from a rotatory motion of the tail, which caused its different parts to be transported in succession to the apparent sides, whence, by supposing the hollow cone of which it was composed to be irregularly terminated, a succession of apparent changes would take place, resembling those actually perceived. Similar variations, indicative of a rotatory motion, were also witnessed in the tail of the comet of 1825, by Mr. Dunlop, at Paramatta.

Phenomena of a still more fleeting and irregular nature are alleged to have been observed in the tails of comets. Kepler states that the tail of the comet of 1607, which at one time appeared short, would, in the twinkling of an eye, become very long†. According to Cysatus, the tail of the comet of 1618 exhibited undulations, as if it had been agitated by the wind. Hevelius has mentioned that he perceived similar undulations in the tails of the comets of 1652 and 1661; and Pingré has still more recently asserted the same thing with respect to the comet of 1769. To this class of phenomena may be referred a remarkable appearance witnessed in the great comet of 1843. On the 11th of March Mr. Clerihew, who observed the comet at Calcutta, found that since the previous evening it had darted forth a new tail, nearly twice as long as the original one, and forming with it an angle of 18°. This supplementary tail was not seen on any other night during the visibility of the comet.

Before attempting to describe the various hypotheses that have been formed respecting the physical constitution of comets, it may not be out of place to give a brief account, by way of illustration, of some of the more remarkable apparitions of bodies of this class which have been recorded in history.

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* Mém. Acad. des Sciences, 1775, p. 392, et seq.

† De Cometis, p. 102.

Diodorus Siculus has stated that in the first year of the 102nd Olympiad (or the year 371 A.C.) there appeared in the heavens a train of light of extraordinary splendour. This remarkable phenomenon was believed to have presaged the destruction of the Achaian cities Helix and Buris. Aristotle alludes to the same comet in his treatise on Meteors. He says that on the first night the head was not seen, having been too near the sun. On the second night it had removed a little from the sun, and was visible for a short time in the evening. After it set the tail was still seen as a brilliant train of light, extending over a third of the heavens, or, in other words, over an arc of 60°.

In the year 43 A.C., during the celebration of the games in honour of Venus, there appeared a comet at Rome, which could be discerned before sunset. This celestial prodigy continued to be visible for eight successive days. The poets flattered Augustus with the belief that it was the departed soul of his great relative Julius Cæsar, who had been recently assassinated. It has been already mentioned that Halley suspected this to be an apparition of the great comet of 1680.

In the year 1106, A.D., there appeared a magnificent comet, which was visible over all Europe. Matthew Paris says that it was seen at a distance of only one cubit from the sun. It is easy to conclude from this remark that the comet was visible in the daytime. The head was small and obscure, but the tail is stated by various writers to have been an object of terrific splendour, which was seen like a fiery beam stretching from the west towards the north-east regions of the heavens. This is also believed to have been an apparition of the comet of 1680.

In the summer of 1264 a great comet appeared, which is mentioned by almost every contemporary historian of Europe. An account of it is also contained in the Chinese Annals, which presents a satisfactory agreement with the statements of the western writers respecting it. This magnificent comet is said to have been accompanied by a tail 100° long. It was generally believed that the object of its apparition was to announce the death of Pope Urban IV., who expired in the following October. We have already mentioned that this comet is supposed to be identical with a comet which appeared in 1556, and that its return in the present day is expected with a considerable degree of confidence by astronomers.

The year 1402 was distinguished by the appearance of two of the most splendid comets recorded in history. The first became visible about the middle of February. As it continued to approach the sun it increased in magnitude and splendour, until at length, towards the end of March, it became visible in broad daylight. The tail when first seen was short, but it increased with great rapidity, and ultimately exhibited an immense apparent length.

The second comet of 1402 appeared in the month of June. It was observed in Italy, Germany, and the countries of the East. Like the first comet of the same year, it was visible in the daytime. After sunset the tail was seen extending from the horizon to the zenith. It is said to have been so bright as to have eclipsed the light of the stars, but this is a manifest exaggeration.

In the year 1456 there appeared a magnificent comet, which was visible over all the countries of Europe. The tail is stated to have been 60° long.

This comet spread universal consternation, in consequence of its apparition being simultaneous with the capture of Constantinople by the Turkish army. In order to ward off the evil consequences which might ensue from its influence, Pope Calixtus II. ordered prayers to be offered up in all the Western churches. He also issued a bull, in which he anathematized both the Turks and the comet. It is hardly necessary to state that while the followers of the crescent did not fail to acquire permanent possession of the ancient capital of the Eastern Empire, notwithstanding the means that were employed by the Papal Church to arrest their progress, so in like manner the comet continued with the same tranquillity as formerly to pursue its path throughout the heavens. This is now known to have been an apparition of the famous comet of Halley.

According to Cardan, a comet which appeared in 1532 was seen by the inhabitants of Milan in full sunshine. This comet is supposed to be identical with one of those which appeared in 1402, and also with a comet which subsequently became visible in 1661.

The comet of 1577 was one of the most conspicuous of modern times. It was first seen by Tycho Brahé before sunset, as he was returning home after taking some fish out of a pond. This comet is remarkable for being the first that was demonstrated to revolve beyond the moon's orbit.

The seventeenth century is peculiarly fertile in great comets. It was distinguished by two apparitions of Halley's comet, namely, those of 1607 and 1682. The third comet of 1618 was one of the most splendid of modern times. Its more remarkable features have been already mentioned. The comet of 1652 is said by Hevelius to have been of such magnitude as to have resembled the moon when half full; only it shone with a pale and dismal light. This comet is otherwise interesting to the astronomer on account of the minuteness with which the various phenomena relating to it have been described by the assiduous observer just mentioned. An account has already been given of the huge comet of 1668, which was seen in the countries of the south of Europe and in Brazil. The comet of 1680 is memorable for the magnificent tail by which it was accompanied; for its near approach to the sun; but above all, for having furnished the data by means of which the immortal Newton succeeded in demonstrating that comets are guided in their movements by the same principle as that which controls the planets in their orbits. The comet of 1689, which was visible only in southern countries, had a tail 68° long. It has been already mentioned that there exist good grounds for believing it to have been identical with the great comet of 1843.

Although the eighteenth century is less prolific in apparitions of great comets than that immediately preceding, it is notwithstanding distinguished by two of the most remarkable recorded in history. The comet of 1744 is one of the few comets which have been seen in full sunshine. On the 1st of February it appeared more brilliant than Sirius. On the 8th it equalled Jupiter. On the 1st of March it was visible to the naked eye at one o'clock in the afternoon, five hours only having then elapsed since its passage of the perihelion. The singular appearance of the tail of this comet has been already mentioned. The comet of 1769 is especially memorable for the immense tail by which it was accompanied.

The first comet of 1811 was one of the most conspicuous of the present century. It is especially remarkable for the length of time during which it continued to be visible. The comet of 1843, to which allusion has been so frequently made, was in many respects one of the most remarkable of modern times. Like a few other comets, it was visible in broad daylight. At noon on the 28th of February it was distinctly seen without the aid of glasses, by numerous persons congregated in the streets of the city of Bologna in Italy. It then appeared to the east of the sun, at a distance from his disk of about two diameters. On the same day it was seen under similar circumstances, in various parts of the world. When it had sufficiently extricated itself from the rays of the sun to be visible in the evening, it appeared with extraordinary magnificence, especially in tropical countries. The head was very conspicuous, but the most striking part of the comet was the tail, which resembled an immense beam of light, extending over an arc of 60° or 70°. Unfavourable weather prevented this magnificent comet from being visible in England or any of the northern countries of Europe previous to the 17th of March. On the evening of that day, a little after sunset, the tail became visible in the western horizon, but the head had already set. On the following evening the whole of the comet was seen. The head was small, but the tail was a brilliant object, extending over an arc of the heavens of about 40°. It continued to appear with great splendour on the following evenings, but it rapidly grew fainter, and finally disappeared altogether from observation about the beginning of April*. It has been already mentioned that this comet is remarkable for having approached nearer the sun than any other comet recorded in history.

It now remains to give some account of the various hypotheses that have been formed respecting the physical constitution of comets, and of the explanations that have been founded upon them, of the more prominent phenomena exhibited by these bodies. Although Tycho Brahé, by proving that comets traverse the regions beyond the moon's orbit, transferred them from the category of substances generated in the terrestrial atmosphere, he still left the question undecided whether they are bodies forming permanent members of the solar system, or whether they are mere masses of vapour, liable to be dissipated into space by the action of the sun or any other similar body near which they may happen to pass in the course of their erratic movements. That comets are either wholly or partially composed of a gaseous substance, appears to be admitted by every philosopher who has sought to arrive at some definitive conclusion respecting their physical structure. Kepler supposed them to be bodies of transient duration, which spin out their fleeting existence by a process of evaporation conducted through the medium of their tails. Newton, on the other hand, was of opinion that comets are composed of a partially solid substance. He imagined that the matter susceptible of evaporation forms an extensive atmosphere round the comet, which is gradually dissipated into space by the action of the solar heat, the vaporised particles, as in Kepler's theory, occasioning the appendage of the tail in the course of their recess from the head. According to another hypothesis, the nebulous matter thus raised from the comet is not wholly projected into space, the greater portion being again precipitated upon the head, in consequence of the diminution of temperature which takes place during the comet's recess from the sun.

It has been already mentioned that the assertion of Hevelius, respecting the variation of the volume of comets depending on their distance from the sun, has been confirmed to a certain extent by the observations of modern astronomers. Newton's theory of the projection of the cometic atmosphere into the tail, and its subsequent dispersion into space, sufficiently accounts for the diminution of volume previous to the passage of the perihelion; but it is difficult to explain by the same theory the enlargement observed to take place during the comet's recess from the sun. It was probably with a view to obviate this difficulty that he threw out the conjecture of the comet being enveloped in a dense smoke during its passage of the perihelion, arising from the intense heat to which it was then exposed. The presence of such a smoke would cause the comet to appear less in perihelion than it really was; while its gradually less abundant emission as the comet continued to recede from the sun would give rise to an apparent enlargement of volume. It is hardly necessary to state that this is a mere surmise to which no importance can be attached as an explanation of a fact established by accurate observation. M. Valz has recently attempted to account for the diminution of the volumes of comets on approaching the sun, and their subsequent enlargement, by the compression which they undergo in the course of traversing the solar atmosphere. This view of the origin of the phenomenon necessarily implies that the comet is absolutely impermeable to the highly-attenuated fluid through which it is assumed to revolve, a condition which is utterly incompatible with all that observation has revealed to us respecting the structure of these bodies. A more probable explanation has been suggested by Sir John Herschel. According to that astronomer, as the comet approaches the perihelion the action of the solar heat will be constantly transforming the nebulous matter of which it is composed into the condition of a transparent invisible gas; and as this process necessarily commences at the exterior of the nebulosity, where the solar rays impinge, the immediate consequence will be a diminution of the volume of the comet. After the passage of the perihelion, the radiation of heat from the surface of the more condensed portion of the comet will not be sufficiently compensated by the solar heat, and the diminution of temperature hence arising will occasion a precipitation on the surface of the nebulous matter suspended in a gaseous state in the atmosphere of the comet. This precipitation of nebulous matter will continue to go on under the influence of the cooling process occasioned by the increasing distance of the comet from the sun, and the manifest result will be a rapid enlargement of the visible dimensions of the comet. According to the laws of equilibrium the lighter particles of the precipitated vapour will arrange themselves so as to form the superior stratum of the enveloping nebulosity of the comet. It is evident also that as this bounding stratum continues to diminish in density it will attain a higher and higher elevation, while at the same time its increased tenuity will cause it to assume a more and more filmy aspect *. This view of the physical condition of the nebulosity of a comet subsequent to the passage of the perihelion has received a strong confirmation from the observations of Halley's comet by Sir John Herschel, who found that simultaneously with the increase of its dimensions the envelope continually grew fainter, until at length from this cause alone it ceased to be visible.

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* For a great number of interesting particulars relative to this remarkable comet, see the *Comptes Rendus*, tome xvi., pp. 597, 605, &c. &c. &c.

According to the above theory the growing faintness of the envelope is, in a great degree, occasioned by the gradual absorption of the vaporous matter composing it, into the nucleus of the comet. This was clearly indicated in the case of Halley's comet by the brilliant appearance of the

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* Mem. Ast. Soc., vol. vi., p. 99, et seq.

nuclear ray during the earlier observations of the comet, and the increase of relative brightness exhibited by the nucleus itself when the ray finally disappeared. The conclusion naturally suggested by these physical changes was, that the nebulous matter composing the paraboloidal envelope of the comet was conducted again to the nucleus along the axis of the paraboloid, and that the ultimate brightness of the nucleus was due to the increased condensation of its constituent molecules, resulting partly from the accession of the matter composing the envelope, and partly from an actual diminution of volume consequent on its diminishing temperature.

The foregoing theory of the variation of the volume of a comet depending on its distance from the sun, is mainly founded on the principle that the nebulous matter of the comet is susceptible of being transformed by the heat of the sun into the condition of a transparent invisible gas; and that so long as the comet is in the neighbourhood of its perihelion it is actually encompassed by such an atmosphere. It has been already mentioned that the observations of the comet of 1811 afforded unequivocal evidence of the existence of a transparent atmosphere; for upon no other supposition could the dark interval which separated the envelope from the head be satisfactorily explained. When the precipitation of the nebulous matter of the envelope rendered this interval no longer visible, it was impossible to obtain any direct assurance respecting the atmosphere; but that it still continued to surround the comet Herschel received an indubitable proof in the re-appearance of the envelope on the 9th of December, 1811 *. This indication of a second stratum of nebulous matter lasted only a few days, and was doubtless occasioned by some circumstance peculiar to the constitution of the comet.

In all speculations on the physical constitution of comets the most interesting point of enquiry is that relating to the origin of the tails of these bodies. The earlier astronomers, including Appian, Cardan, and Tycho Brahé, supposed the phenomenon of a comet's tail to arise simply from the passage of the solar rays through the nebulosity of the head, comparing it to the appearance presented by a beam of light which has been transmitted through a small aperture into a dark chamber. It does not seem to have occurred to these theorists, that the beam of light in the latter instance becomes visible only in consequence of the presence of floating molecules of matter which obstruct the solar rays and reflect them to the eye of the spectator †.

The opinion of the Cartesians, although based on a more rational principle, involved conclusions totally inconsistent with observation. They imagined the tail of a comet to be occasioned solely by the refraction which the ether of the celestial regions exercised upon the rays of light proceeding from the comet to the observer. If this were true, the light of the tail ought to exhibit the various colours of the prismatic spectrum. Upon the same supposition the planets and fixed stars should also be accompanied by tails. Neither of these conclusions, however, are borne out by the observations of astronomers. Moreover, according to this hypothesis, the deviation of the tail from the prolongation of the

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* Phil. Trans., 1812, p. 129.

† Pingré, alluding to this absurd hypothesis, cites Newton and Gregory as having remarked that it was at variance with the fundamental principles of optical science. The same objection, however, had already been clearly pointed out by Kepler.—(*De Cometis*, p. 101.)

radius vector should always be in the same direction when the comet appears in the same region of the heavens. Newton, however, has remarked that, although the comet of 1577 occupied the same apparent position on the 28th of December which the comet of 1680 occupied on the 29th of the same month of the year, and although the earth, in both cases, was consequently in the same part of her orbit, the tail of the comet of 1577 deviated 21° towards the *south*, whereas on the other hand the tail of the comet of 1680 deviated 4½° towards the *north**. It follows, therefore, that the supposition of the phenomenon being due to the refraction which the light of the comet suffers in passing through the celestial regions is inconsistent with observed facts, and is consequently untenable. Another opinion respecting the tails of comets is that which supposes them to be appearances similar to the aurora borealis. A theory, founded upon this view of the subject, was proposed by Mairan, but its inconsistencies were too glaring to secure even its partial adoption.

According to the various hypotheses hitherto considered, the tail of a comet is merely an optical illusion, having no real foundation in nature. Another class of hypotheses is that according to which they are imagined to be composed of a material substance similar to the nebulosity of the comet. Some persons, adopting the maxims of the Aristotelian philosophy, respecting light and heavy bodies, supposed that the matter composing the atmosphere of comets was an essentially light substance; that in virtue of its inherent levity it had a constant tendency to recede from the sun; and that hence originated the appendage of the tail. It is hardly necessary to state that this hypothesis is at variance with the fundamental property of the inertia of matter and the recognised principle of universal gravitation.

The first approach to anything resembling a rational explanation of the tails of comets is unquestionably due to Kepler. According to that illustrious astronomer a comet is composed wholly of a nebulous substance, the constituent parts of which are gradually broken and dispersed by the incessant action of the solar rays upon them. The lighter particles yield to the impulse of the rays, and, proceeding to an immense distance from the head of the comet, occasion the appendage of the tail. The denser particles remain behind and form the nebulosity surrounding the head†. This hypothesis furnished a satisfactory account of the general direction of the tails of comets. It also afforded an explanation of their curvature and their concavity with respect to the region which the comet was leaving; for it is manifest that as the comet continued to revolve in its orbit, the nebulous particles impelled by the solar rays would lag somewhat behind the prolongation of the radius vector, and that the effect so produced would be greatest for the particles which had ascended the earliest, or, in other words, for the particles most remote from the head. Another advantage of this hypothesis was that of referring the phenomenon to a true physical cause; for it can hardly be doubted, whatever be the mode of the propagation of light, that the solar rays, if interrupted in their progress by a material substance, must communicate to it an impact of *some* degree of intensity. On the other hand it must be admitted that the cause assigned, although founded in nature, cannot, with any degree of probability, according to Kepler's view of it, be considered sufficient to produce the observed appendage of the comet. A material improvement of the hypothesis of that astronomer consisted in introducing the solar heat as one of the exciting causes of the phenomenon. The first person who appears to have explained the formation of the tails of comets upon this more enlarged view of the action of the sun was Claude Comiers, a French writer, who flourished about the middle of the seventeenth century. He supposed the particles composing the nebulosity of the comet to be rarefied to so great a degree by the heat of the sun, as to yield with facility to the impulse of the solar rays, and, acquiring from this cause a motion in the direction opposite to the sun, to form an appendage to the comet represented by the phenomenon of the tail*. The theory of Kepler, thus improved, presented itself to the mind of the enquirer under a much more favourable aspect than previously; for, however feeble might be the dynamical influence of the solar rays, it was possible, without exceeding the bounds of a rational probability, to ascribe the phenomenon to their agency, by supposing an adequate attenuation of the nebulous matter of the comet, arising from the calorific power of the rays. No further notice appears to have been taken of this theory until the time of Whiston, who explained it with great clearness in his work entitled "A New Theory of the Earth."† It was soon afterwards referred to by Euler as the most satisfactory explanation of the phenomenon that had been hitherto devised; and upon the same ground it has been favourably mentioned by many subsequent astronomers and mathematicians, including in more recent times Sir William Herschel, Laplace, Delambre, and Arago.

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* Princip., book iii., prop. 41.

† De Cometis, p. 100.

Newton, in the Principia, has entered into some interesting speculations on the physical constitution of comets, in the course of which he discusses the various hypotheses that had been formed with a view to account for the origin of the tails of these bodies‡. Of these, the hypothesis of Kepler, founded on the impulsion of the solar rays, appeared to him to be the only one which offered any degree of probability; but his mind was too much pre-occupied with an hypothesis of his own upon the subject, to allow him to give the full sanction of his authority to it. Newton's view of the origin of the tail of a comet was this: he supposed the rays of the sun, by their calorific influence, to raise the temperature of the nebulous particles of the comet, which, in their turn, communicated a portion of the heat thus acquired to the contiguous particles of the ethereal fluid composing the solar atmosphere. This increase of temperature being accompanied by a corresponding diminution of density, the particles of the ether ascended to a greater distance from the sun, carrying along with them the more volatile particles of the comet in the same manner as an upward current of air causes smoke to ascend in the terrestrial atmosphere§. The general direction of the tail, its deviation from a straight line, and its convexity with respect to the region towards which the comet was advancing, were explained by this theory with the same facility as by Kepler's. Notwithstanding these advantages, it met with a reception from men of science, which forms a striking contrast with that experienced by the other physical theories of its author. This circumstance is, perhaps, in some degree attributable to Newton's assumption of a principle whose existence was unsupported by any positive evidence, a fault which that illustrious philosopher so cautiously avoided in all his other speculations. It may be remarked, however, that cometary phenomena are not altogether wanting in indications of a solar atmosphere, or some such ethereal fluid, pervading the celestial regions. It has been already mentioned that the motion of Encke's comet seems to indicate the existence of such a fluid*. Another fact, which has been more generally established, tends also to suggest the suspicion of the comet moving through a resisting medium. It has been mentioned that the tail of a comet is usually brighter and better defined on the convex than on the concave side. Now, this is the result which might naturally be expected to be produced by the motion of the comet through a resisting medium; for as the convex side of the tail is also the preceding side, it is the part that would be mainly exposed to the pressure of the ethereal fluid; and being, in consequence, more condensed, it ought to appear more luminous, and to present a sharper outline than the concave side. Newton, indeed, gave a different explanation of this phenomenon. He considered it to arise from the circumstance that the particles composing the preceding side of the tail, had more recently ascended from the head of the comet than the other particles, and therefore that less time was allowed for their diffusion in space†. It is not improbable that the superior brightness and clearer definition of the convex side of the tail may be due to the combined operation of both these causes.

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* Traité de la Nature et Presage des Comètes, p. 81.

† See p. 52 of the work cited.

‡ Prin., lib. iii., prop. 41.

§ "Newton," says Lalande (Ast., tome iii., art. 3212), "supposed the tails of comets to be emanations from their atmospheres. He remarked that smoke and vapours may ascend from the comet either in consequence of the impulse which they receive from the solar rays, or, more probably, from the rarefaction which the solar heat produces in their atmospheres." It does not seem to have occurred to the astronomer just cited, (unless indeed he threw the responsibility of the omission upon Newton,) that without the additional supposition of a solar atmosphere, the vapours of the comet, if rarified by heat, would extend equally in all directions from the head. In the very next article of the work cited, he gives a clear description of Newton's theory, ascribing it, however, not to that philosopher, but to Boscovich, and stating that it first appeared in a work published by him at Rome in the year 1746.

Newton's theory of the tails of comets may perhaps be considered equally satisfactory with that which refers them to the action of the solar rays upon the more volatile particles of the cometic atmosphere; but neither the one nor the other can be regarded as anything else than a vague hypothesis, which, although it affords an explanation of a few of the more prominent features of the phenomenon, is incapable of conducting to results of any precision, and totally fails to render an account of minute details. Thus, although the general direction of the tails of comets is accounted for sufficiently well either by the dynamical action of the solar rays upon the cometic atmosphere, or by their calorific agency, assuming the existence of a solar atmosphere, it is utterly impossible to explain, by either of these principles, such a phenomenon as that presented by the comet of 1823, which had one tail extending in the usual direction, and another turned almost completely towards the sun. Many other anomalous appearances are equally inexplicable by either of the two hypotheses just mentioned. A more recent view of the subject is founded upon the supposition of the phenomenon being due to electrical agency. One of the earliest advocates of this mode of explanation was the German astronomer Olbers. In more recent times it has been favourably noticed by Bessel, Biot, and Sir John Herschel. The limits of this work prevent any further allusion to it here. It may be remarked that in the present state of our knowledge respecting the nature and mode of operation of the electrical principle, it is utterly hopeless to arrive at any reliable conclusions by means of a theory founded upon the supposition of its agency.

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* See p. 135.

† Princip., lib. iii., prop. 41.

The question whether comets are self luminous, or whether they are indebted for their light solely to the sun, has often been discussed by astronomers. The high magnifying powers that these bodies in some instances support, tend to favour the conclusion that they are self luminous. It was upon such grounds that Herschel concluded that the comet of 1807, and the first comet of 1811, shone by a light essentially inherent in their respective substances*. Bessel was of opinion that the sudden variations of brightness exhibited by Halley's comet in the month of October, 1835, could with difficulty be accounted for by any other supposition than that of a developement of light by the substance of the comet†. It is manifest that the appearance of phases in comets, if established beyond doubt, would afford a decisive proof that these bodies shine only by reflection. It has been asserted that phases, in some instances, have been actually observed, but such statements have not been supported by the observations of contemporary astronomers. Delambre mentions that the registers of the Royal Observatory of Paris exhibit unequivocal indications of phases in the comet of 1682. It is to be remarked, however, that neither Halley, nor any other astronomer who observed this comet, makes mention of such a fact. Again, James Cassini states that the comet of 1744 exhibited phases which would have been as distinct as those of Venus if the disk had been somewhat larger‡. On the other hand, Heinsius and Chésaux, who both observed the comet with especial attention, have explicitly denied the existence of any indications of such a phenomenon. In more recent times Cacciatore, the celebrated Italian astronomer, expressed his positive conviction that the comet of 1819 presented the appearance of a crescent. It was found, however, that the position he assigned to the line joining the horns of the crescent was incompatible with the supposition that the comet shone by the light of the sun. Sir William Herschel was unable to discover anything resembling a phase in the comet of 1807, although he assured himself, that a considerable portion of the disk could not have been illuminated by the sun at the time of observation§. The observations of subsequent astronomers have also been generally unfavourable to the existence of phases‖.

Although observations indicating the existence of phases in comets would establish beyond all doubt that these bodies shine by reflected light, it must be admitted on the other hand that the absence of such phenomena does not warrant the conclusion that comets are self luminous. With reference to this point it is to be remarked, in the first place, that if a comet consists of a mere globular mass of vapours, it will offer no effectual opposition to the passage of the solar rays, and, consequently, it will reflect light with equal facility from every part of its surface. Nay, if the comet should offer unequivocal indications of the existence of a nucleus, since in every such case the surrounding nebulosity is of immense extent, it is difficult to conceive how a different result can take

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[*] Phil. Trans., 1808, p. 157; ibid., 1812, p. 119.

[†] Connaissance des Temps, 1840. Addit. p. 98.

[‡] Mém. Acad. des Sciences, 1744, p. 303.

[§] Phil. Trans., 1808, p. 156.

[‖] Sir John Herschel has remarked that nothing which could bear the least resemblance to a phase was perceptible in Halley's comet.—(*Results of Ast. Obs. at the Cape of Good Hope*, p. 397.)

place, even if the nucleus be composed of a solid substance. But, in fact, the nuclei of comets are so very small, that even under the most favourable circumstances the discovery of phases might naturally be expected to elude the most delicate observations *.

A very different method has been devised by M. Arago for ascertaining the nature of the light by which comets shine. It is well known that when light is reflected from a body at certain angles, it acquires properties different from those which characterize light emitted directly from a self luminous body; or, in other words, the light thus reflected becomes polarized. By means of experiments on the light of the comet of 1819, M. Arago found that it contained polarized, and therefore reflected light. Similar experiments on Halley's comet, made on the occasion of its apparition in 1835, afforded still clearer indications of the existence of reflected light †. In other instances, as in the case of the great comet of 1843, no trace of polarized light was discernible ‡. It may be remarked that experiments of this nature, however unequivocal their results may be, are not capable of deciding the question with respect to the nature of the light by which comets shine, since a body, although self luminous, does not with less facility on that account reflect the light of other bodies. There is a question of another kind, however, upon which such experiments are calculated to throw a decisive light. It has been mentioned as a proof of the translucency of the matter of which comets are composed, that the smallest stars have been occasionally seen through them without undergoing any diminution of lustre. This, however, has not been the invariable result that has occurred, under such circumstances, to the observation of astronomers. In some instances the star has appeared sensibly fainter from the interposition of the comet. For example, on the 31st of October, 1807, Sir William Herschel found that small stars, seen through the tail of the comet of that year, exhibited a considerable diminution of brightness as they became more involved in the nebulosity of the comet. This diminution of brightness is readily accounted for by the obstruction which the particles of the cometic matter offer to the rays of light proceeding from the star to the observer. It is to be remarked, however, that according to a principle of optics, a feeble light when projected upon an illuminated ground, suffers an apparent diminution of brightness; and the question, therefore, arises whether the faintness of a star, when seen through a comet, is not wholly referable to this cause. In the instance above cited, Herschel felt a disposition to adopt this as the true explanation of the phenomenon, on the ground that the brightness of the tail of the comet was a fact palpable to observation, whereas, on the other hand, there was no evidence to prove the existence of floating particles of matter interposed between the star and the observer §. Now the experiments of M. Arago on the light of comets establish beyond doubt that the substance of which the tails of these bodies are composed, is sufficiently dense to obstruct the rays of light proceeding from a luminous body; for the phenomenon of reflected light necessarily implies that the cometic particles

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* According to Sir William Herschel the nucleus of the comet of 1807 had an apparent diameter of 1″—(*Phil. Trans.*, 1808, p. 156). The same astronomer found the apparent diameter of the nucleus of the first comet of 1811 to be only 0″.775.—(*Phil. Trans.*, 1812, p. 118.)

† Comptes Rendus, tome i., p. 257.

‡ Ibid., tome xvi., p. 597.

§ Herschel at this time was inclined to believe that the tail of a comet was merely an optical phenomenon resembling the aurora borealis.—(*Phil. Trans.*, 1808, p. 159.)

possess this power of obstruction. It results, therefore, that the faintness which small stars occasionally exhibit, when perceived through any part of the substance of a comet, *may*, and in all probability *does*, to some extent, arise from the resistance which the cometic particles oppose to the rays of light proceeding from the star to the observer *.

When the flimsy nature of the substance of a comet is taken into consideration, it is difficult to conceive how such a body is not entirely dissipated in space by the enormous heat of the sun during its passage of the perihelion. Mention has already been made of Newton having calculated that the great comet of 1680, when it arrived at its least distance from the sun, was subjected to a heat 2000 times greater than that of red-hot iron †. He considered that the circumstance of a comet being able to maintain its existence after passing through such a terrible ordeal, formed an irresistible argument in favour of its being a solid body. Laplace, availing himself of Black's beautiful discovery of latent heat, shewed that the durability of the existence of a comet might be accounted for, without having recourse to a principle which, to say the least respecting it, did not receive any support from observation. It was established by the eminent philosopher just cited, that when a body is in the course of passing from the liquid to the gaseous state, the particles, as they become successively volatilized, abstract from the body a large quantity of caloric which continues insensible to the thermometer. Laplace supposed that the heat thus carried off by the volatilized particles of the comet during its passage of the perihelion, would serve to moderate the temperature of the more condensed portion; and conversely, the heat given out by the same particles, in the course of their return to the liquid state, would have the effect of counteracting the intense cold to which the comet would be exposed in the more distant parts of its orbit ‡.

Whether a comet be composed of a partially solid substance, or whether it consist of a mere collection of vapours, is a question which has not yet been resolved to the satisfaction of astronomers; but one thing is certain, that the masses of comets must be very small. This was strikingly evinced in the case of Lexell's comet by its passage through the middle of the system of Jupiter's satellites, in the year 1779, without occasioning the slightest perceptible derangement in the motion of either of those bodies.

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* It seems difficult to account for the extreme faintness of the stars seen by Sir William Herschel through the comet of 1807, without supposing it to have been in some degree produced by the interposition of the cometic substance. On the other hand, Sir John Herschel has remarked that, although innumerable stars of all magnitudes, from the ninth downwards, were seen by him through the substance of Halley's comet, there never appeared the least ground for presuming any extinction of their light in traversing it. "Very small stars were, indeed, obliterated," says that eminent astronomer, "as they would have been by an equal illumination of the field of view; but in no case to a greater extent than they would have been by so much lamp-light, artificially introduced. (*Results of Ast. Obs. at the Cape*, p. 401.)

† As this result of Newton's is often cited, it may not be out of place briefly to state the grounds upon which he established it. When the comet was in perihelion, on the 8th of December, its distance from the centre of the sun was to the earth's distance as 6 to 1000. Now, since the intensity of the sun's heat is reciprocally as the square of the distance from his centre, it follows that the sun's heat on the comet was to the heat of the summer sun as 1,000,000 to 36, or as 28,000 to 1. But he also found by experiment that the heat of boiling water is about three times greater than the heat which dry earth acquires from the summer sun; and he moreover conjectured that red-hot iron is about three or four times hotter than boiling water. His final conclusion consequently was that the comet must have been subjected to a heat 2000 times greater than that of red-hot iron.

‡ Systême du Monde, tome i., book ii., chap. v.

The question with respect to the end which comets are designed to serve in the economy of creation, appears to be involved in a degree of obscurity greater even than that which surrounds any other enquiry connected with these mysterious bodies. Newton asserted that all those comets which descend so low as to come within the solar atmosphere, would suffer a retardation of their motion on each occasion of their passage through their perihelia, and being, in consequence, less capable of resisting the attraction of the sun, would gradually approach that body until they ultimately fell upon his surface. Generalising this idea, he supposed that the fixed stars might be occasionally recruited by the falling of comets into them, and that the conflagration hence arising might account for those temporary stars which, at different times, have appeared with great splendour in the heavens. With respect to the tails of comets he was of opinion that after being dissipated in space they were absorbed by the planets, and entering into a multitude of chemical combinations with other substances, tended thereby to repair the waste of fluids occasioned by the evaporating influence of the sun. It would, perhaps, be as difficult to disprove these surmises as to demonstrate their truth, for in fact, they can only be regarded as mere sallies of the imagination into regions of thought, beyond the reach of legitimate reasoning. The speculations of succeeding astronomers on this subject do not lead to conclusions of a more satisfactory kind than those above hinted at. Sir William Herschel was of opinion that a comet on the occasion of each perihelion passage acquires a more perfect state of condensation in consequence of the action of the solar heat upon the nebulous matter of which it is partially composed. This hypothesis pointed out to the probability of a comet eventually acquiring the consistency of a solid body and assimilating itself in all respects to a planet. A similar view of the ultimate state of comets was adopted by Laplace, who remarked that the comet of 1759 was the only one which had hitherto exhibited any indications of having arrived at a fixed condition. He was probably led to this conclusion by a comparison of the recorded apparitions of the comet, from which it would seem that it had been diminishing in splendour on the occasion of each return to perihelion, until at length, in 1759 it almost ceased to exhibit the more striking peculiarities of a cometary body. Much of the awful magnificence of the comet on the occasion of its apparition in 1456 is doubtless attributable to the effect upon the imagination of a phenomenon that was universally regarded with feelings of terror, as a visible manifestation of divine displeasure; but it is an indisputable fact that the comet was a much more conspicuous object in 1607, than it was in 1684 or 1759. Thus, in 1607, Kepler distinctly perceived the tail with the naked eye, thirty days before the comet's arrival in perihelion*. In 1682 the comet, even for some time after it became visble to the naked eye, did not exhibit any vestige of a tail, for Cassini has remarked that its disk was as round, as well defined, and as devoid of nebulosity as that of the planet Jupiter†. Perhaps the circumstance of its appearing so soon after the great comet of 1680 may have caused its cometic features to be in some degree overlooked by astronomers. This could not be said on the occasion of the return of the comet in 1759, for although Messier carefully observed it with a telescope twenty-six days before the passage of the perihelion, he was unable to discern the slightest trace of a tail. So far, therefore, the remark of Laplace appears to be supported by observation. Unfortunately, however, the comet during its last apparition exhibited a decidedly less planetary aspect than it did in 1759; for on the 12th of October, 1835, the tail was already visible to the naked eye as a very conspicuous object, although the passage of the perihelion did not take place until 35 days afterwards.

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* The comet with its tail was seen at Prague by many persons as well as Kepler on the 26th of September (*De Cometis*, p. 25). According to Halley it passed its perihelion on the 26th of October.

† Mém. Acad. des Sciences, 1699, p. 39.

The foregoing account of speculations on the physical constitution of comets may serve to shew how much yet remains to be done in this interesting department of astronomy. It is clear that a more extensive collection of facts than that at present in the possession of astronomers, must be formed by a long course of accurate observation; and that more mature views of the great agents of nature must be arrived at by an assiduous cultivation of the various branches of physical science, before any hopes can be entertained of coming to a definitive conclusion respecting the more essential properties of these mysterious bodies, or the purposes they are designed to accomplish in the economy of the material universe.

CHAPTER XVI.

Importance of Facts in the Cultivation of Physics.—Astronomy a Science of Observation.—Inequalities which affect the apparent positions of the Celestial Bodies.—Precession.—Its Discovery by Hipparchus.—Researches of Modern Astronomers on its Value.—Bessel.—Peters.—Otto Struve.—Refraction.—Its effect upon the Place of a Celestial Body first remarked by Ptolemy.—Opinion of Tycho Brahé respecting its Nature.—The first Theory of Refraction due to Cassini.—His Table of Refractions.—Newton.—His Correspondence with Flamstead on the subject of Refraction.—Formula of Bradley.—French Tables of Refraction.—Researches of Bessel.—Aberration.—Its discovery by Bradley.—Modern Determinations of its Value.—Nutation discovered by Bradley.—Its most Approved Value.—Researches on Parallax.—Methods for facilitating the Reduction of Observations.—Method of Bessel.—Physical Causes which more especially affect the Aspect of the Celestial Bodies.—Diffraction.—Irradiation.

It does not require a profound aquaintance with the history of any branch of physical science, to arrive at the conviction that its advancement has been invariably effected by reasoning upon facts whose existence had been already established either by observation or experiment. Astronomy is essentially a science of observation. Even in the earliest stages of its progress some interesting results were deduced, by simply noting the periodical recurrence of the more obvious phenomena. It was by pursuing a process of this sort that the Chaldeans succeeded in obtaining rude approximations to the times of revolution of the sun and moon, and in predicting the occurrence of lunar eclipses. The earlier philosophers of Greece, misled by erroneous views with respect to the mode of discovering truth, imagined that it would be inconsistent with the dignity of the human mind, to recognise any alliance between the lofty speculations of abstract science and the monotonous task of observation. It followed, as a necessary consequence, that during many years of Grecian history, not even excepting the palmy days of Athenian civilisation, no progress was made in the study of astronomy. It was only when Alexandria became the capital of the civilised world, and learning in all its departments was liberally patronised by the Ptolemies, that the phenomena of the heavens were observed with regularity and care by the aid of instruments invented for that express purpose. Accordingly, astronomy, considered as a science of strict calculation, was during this period established on a durable basis. It cannot be asserted, indeed, that even yet the metaphysical notions of the speculative philosophers had been wholly banished from the science. The Aristotelian dogmas respecting the essential nature of the celestial movements were still regarded as indisputable axioms, between which on the one hand, and nature on the other, a sort of compromise was effected by means of the famous mechanism of epicycles. So long, indeed, as the discordances between its results and the actual phenomena of the heavens did not exceed the probable errors of observation, this system, however complicated, might fairly be regarded as a legitimate representation of established facts. It was only when the advanced state of practical astronomy allowed the repudiation of discordances of such magnitude, that the arbitrary creations of the human mind, and the immutable laws of the physical universe, might be said to have come into direct collision. The triumphant establishment of the true system of nature by the immortal Kepler, led to the complete emancipation of astronomical science from the thraldom of the Schools, and its subsequent history has in consequence been one of uninterrupted progress down to the present day.

Since the laws which regulate the movements of the celestial bodies constitute the principal subject of research in the study of astronomy, it is manifest that the establishment of a series of facts relating to their apparent positions, forms an indispensable preliminary to all such enquiries. Accordingly, the sun, moon, and planets have in all ages been carefully observed with this object in view; and all the resources of mechanical skill, as well as the most profound investigations of physical science, have been applied towards assuring the accuracy of the results. The stars, too, have been observed with equal care, not merely on their own account, but also because they form fixed points, to which the positions of the various bodies of the solar system may be on all occasions referred.

But the simple determination of the apparent positions of the celestial bodies does not suffice to produce results immediately available towards the purposes of astronomy. Certain inequalities of small, but variable magnitude, affect the position of every celestial body, the values of which must be carefully ascertained for each observation, in order to arrive at a knowledge of the mean position of the body, which alone can be employed in forming the basis of ulterior research. These minute displacements arise from the combined operation of various distinct principles, the investigation of the laws of which forms one of the most important departments of astronomical science. When considered with respect to their origin, they admit of a threefold division. In the first place there are inequalities which depend upon the principle of gravitation; such are the phenomena of Precession and Nutation. The second class of inequalities includes those which are explicable by reference to the properties of light; such are the phenomena of Refraction and Aberration. Lastly, there is the displacement occasioned by Parallax.

It appears from the foregoing remarks that every celestial body is subject to an apparent displacement arising from the combined influence of five distinct inequalities, of which four derive their origin from physical causes; while, on the other hand, the fifth depends upon considerations of a purely mathematical nature. The apparent position of a body is reduced to the mean by applying to it, with an opposite sign, the numerical value of each inequality, corresponding to the time of observation. A brief account of the researches of astronomers in connexion with each of these five inequalities or *corrections*, as they are technically termed, may, perhaps, not prove uninteresting to the reader. This will be best effected by alluding to each correction according to the order of its discovery.

Of the various inequalities which require to be taken into account in reducing the apparent position of a heavenly body to its mean position, the one which first became known to mankind is the increase of longitude, arising from a slow regression of the equinoctial points upon the plane of the ecliptic. As this constant shifting of the intersection of the ecliptic and equator causes the annual arrival of the sun in either of the equinoxes to be a little earlier than it would otherwise be, it has in consequence been denominated "the Precession of the Equinoxes." The discovery of this apparent movement is due to Hipparchus, who arrived at it about the year 125, A. C., by a comparison of his own observations with those of Timocharis, made about 170 years earlier. Its existence was afterwards established beyond doubt by Ptolemy, between whom and Hipparchus there elapsed an interval of nearly 300 years. It has been already mentioned that Copernicus was the first who gave the true explanation of this phenomenon. The discovery of its physical cause by Newton, and the researches of his successors on its laws, have also been briefly noticed. It only remains to give some account of the successive determinations of its quantitative value by astronomers.

The earliest statement of the value of precession is to be found in the Syntaxis. Ptolemy mentions, in the seventh chapter of that work, that having observed several bright stars in the zodiac, he found that while their relative positions were the same as in the days of Hipparchus, they had all increased in longitude to the extent of 2° 40′ during the interval that elapsed between that astronomer and himself. He hence inferred that the increase of longitude amounted to 1° in 100 years, which implies an annual precession of 36″; he moreover stated that Hipparchus had arrived at the same result. This was a very erroneous determination, for, according to the researches of modern astronomers, the annual amount of precession is a little in excess of 50″. The interval between Hipparchus and Ptolemy comprehended a period of 267 years, so that the total increase of longitude must in reality have amounted to 3° 37′, a quantity greater nearly by 1° than that assigned by Ptolemy. As the discordance seems too great to be accounted for by errors of observation, except by adopting an extravagant supposition with respect to their probable magnitude, many eminent astronomers have come to the conclusion that Ptolemy made no observations at all; that in fact his catalogue of the stars is no other than the catalogue of Hipparchus reduced to the epoch of 137 A.D., by increasing all the longitudes to the extent of 2° 40′. Unfortunately there are circumstances which strongly tend to justify this serious charge. Delambre compared together the longitudes of 312 stars as assigned by Ptolemy with the longitudes of the same stars inserted in Flamstead's catalogue, and supposing the interval between these two astronomers to comprehend a period of 1553 years, he hence deduced 52″.4 for the annual value of precession. This result exceeds the true value by rather more than 2″; but such a discordance would necessarily ensue if Ptolemy simply derived his catalogue from that of Hipparchus, since in reducing the longitudes to his own epoch he supposed the quantity of precession to be too small. In order to obtain a stronger assurance on this point, Delambre diminished Ptolemy's longitudes of the same stars by 2° 40′, and supposing the results to be the longitudes of Hipparchus, he instituted a comparison between them and Flamstead's longitudes. Assuming the interval between Hipparchus and Flamstead to include a period of 1820 years, he now obtained 50″.12 for the resulting value of precession, a quantity agreeing almost exactly with the modern determination. Delambre obtained results of a similar nature by pursuing the same process with respect to several other sets of stars common to the catalogues of Ptolemy and Flamstead.

The Arabian astronomers generally estimated the quantity of precession at 1° in 66 years. This gave 54″ for the annual precession, a result which formed a much closer approximation to the true value than that which Ptolemy had arrived at.

The efforts of modern astronomers have been constantly directed towards obtaining a more accurate value of this element. Tycho Brahé fixed the annual precession at 51″. Flamstead made it 50″. Lalande, by comparing the longitude of *Spica Virginis* as assigned by Hipparchus with its longitude deduced from observations made in 1750, obtained 50″.5 for the resulting value of precession. Delambre, by a comparison of the observations of Bradley, Mayer, and Lacaille with his own observations, was induced to fix the annual precession at 50″.1.

As the theory of gravitation began to acquire a more complete state of developement, it became apparent that the precession of the equinoxes is a phenomenon of a much more complicated nature than it had hitherto been supposed to be. It has been already mentioned, that the action of the planets on the earth occasions a secular displacement of the terrestrial orbit, in virtue of which the equinoctial points have a constant tendency to *advance* with a very slow motion upon the plane of the ecliptic, and that hence arises a distinction between lunisolar precession, which refers exclusively to the action of the sun and moon, and general precession, which is equal to the lunisolar precession diminished by the small effect of an opposite nature, arising from planetary perturbation. Now as the diplacement of the terrestrial equator, by constantly altering the position of the zero points, to which the celestial bodies are referred on the ecliptic, affects their longitudes but not their latitudes; so the displacement of the plane of the ecliptic affects the right ascensions of all the celestial bodies, but does not exercise any influence upon their declinations. It is manifest, therefore, that the variation of the right ascension of a star is an effect produced principally by the action of the sun and moon, but in some degree also by the action of the planets; whereas, on the other hand, the change of declination depends exclusively upon lunisolar action. Hence by comparing together the mean right ascensions of a great number of stars, as determined at two distant epochs, the quantity of general precession may be ascertained, and by instituting a similar comparison with respect to mean declinations, the resulting quantity is the value of lunisolar precession. Both the *regression* of the equinoctial points, occasioned by the conical motion of the earth's axis and the *progression* due to the displacement of the ecliptic are in a state of slow variation, and consequently same value. The researches of the illustrious Bessel led to a more accurate determination of the constants of precession than any which had been hitherto arrived at. His earliest investigation, which obtained for him the prize of the Berlin Academy, appeared in the year 1815. He returned to the subject on several subsequent occasions, and his final results are contained in the *Tabulæ Regiomontanæ*, which was published in 1830. The materials of his researches were the observations of Bradley, which formed the most ancient reliable data that were available to him; and those of Piazzi, as well as many of his own observations, with which he compared the determinations of the English astronomer. The values which he assigned to the constants of lunisolar and general precession have, until very recently, been universally used by astronomers. The annual value of lunisolar precession at the beginning of the year 1750 was fixed by him at 50″.37572, and the annual value of general precession at 50″.21129.

The importance of an accurate knowledge of precession in determining by observation the positions of the celestial bodies has been the main cause of those repeated investigations that have been undertaken by astronomers in modern times, for the purpose of obtaining further corrections of the constants upon which its annual value for any given year, and its total magnitude corresponding to any assigned distance from a given epoch, depend. The truth of this remark will appear evident from a consideration of the mode by which the absolute position of a celestial body is usually ascertained. This object is effected, not by a direct process, which would be generally impracticable, but by determining the relative position of the body with respect to certain fundamental stars whose absolute position has been already ascertained with great care at some anterior epoch. Now the absolute position of a body in the celestial sphere is usually expressed by means of its right ascension and declination; but, as both these co-ordinates have reference to the position of the vernal equinox, they are in a state of continual variation from the effects of precession. It is of no use, therefore, for the astronomer to be acquainted with the original right ascension and declination of the star with which he compares the object of observation, unless he possesses a sufficient knowledge of the extent to which the equinoctial points have retreated upon the ecliptic during the intermediate period, to enable him to compute the values of the same co-ordinates corresponding to the time of observation.

But there is another circumstance which in recent times has rendered an accurate knowledge of precession indispensable. The researches that have been prosecuted in the present day, with the view of establishing the motion of the solar system in space, have suggested the necessity of ascertaining with the utmost precision the real nature of those secular variations which affect the apparent positions of the stars. It is not difficult to see that a small error committed in the determination of the numerical values of the constants of precession, would, by always acting in the same direction on the position of a star, be liable to become confounded with the proper motion of the star, which is also characterised by a similar peculiarity. The elaborate researches of the Russian astronomers, MM. Peters and Struve, have led to a modification of Bessel's constants of precession, which, although very slight, may not improbably exercise an important influence on the delicate investigations of sidereal astronomy.

Ptolemy was the first who remarked that a ray of light proceeding from a star to the earth underwent a change of direction in passing through the

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atmosphere, and that, in consequence of the deflection, the star would appear to be elevated above its true place. He further asserted that the displacement would be less according as the altitude of the star increased, and that it would vanish altogether when the star was in the zenith; but he did not attempt to determine its magnitude in any instance, although he made some excellent experiments on the refractive powers of glass and water. Alhazen, the Arabian astronomer, reasoned very judiciously on the same subject, in his "Treatise on Twilight," and Waltherus, a German astronomer, who flourished towards the close of the fifteenth century, made some attempts to determine the magnitude of refraction near the horizon. Tycho Brahé was the first astronomer who applied refraction as a correction to the apparent positions of the celestial bodies. Having found that the latitude of Uranibürg, as deduced from the least and greatest altitudes of circumpolar stars, differed to the extent of 4′ from the latitude indicated by observations of the sun at the solstices, he was led to attribute the discordance to the effect of refraction, which he supposed to have been mainly influential at the winter solstice when the sun attained only a very low altitude. Pursuing the subject, he deduced from his observations a table of refractions, which, although by no means remarkable for accuracy, possessed the merit of being the first of the kind that had been constructed by any astronomer. He supposed the refraction to be 34′ at the horizon, and 5″ at 45° of altitude. His views of the nature of refraction were by no means so accurate as those of Ptolemy or Alhazen, with whose works on the subject he does not seem to have been acquainted. Under an impression that the refraction of the rays of light proceeding from a heavenly body, was occasioned by the vapours which accumulate near the horizon, he asserted that it did not extend so far as the zenith. He also supposed that the refraction of the stars was different from that of the sun or any of the planets; and that while in the former case it ceased at 20° of altitude, in the latter it was sensible as far as 45°. These opinions, although erroneous in an extreme degree, did not admit of being corrected by an appeal to observation, on account of the imperfect condition of practical astronomy.

Kepler, by means of an empirical rule of his own invention, calculated a table of refractions, which was found to be considerably more accurate than Tycho's table. Delambre has shown that for all zenith distances less than 70° the errors do not surpass 9″[*].

The researches of philosophers on the subject of refraction were facilitated in a vast degree by the discovery of Snell, that when a ray of light enters a transparent medium the sines of the angles of incidence and refraction bear a constant ratio to each other. By means of this principle it was easy, in all cases of a ray of light entering a homogeneous medium, to ascertain the subsequent direction of the ray, whatever might be the angle of incidence, when the direction corresponding to any given angle was once determined by experiment. The first theory of astronomical refraction is due to Cassini. He supposed the atmosphere to possess a uniform density, and to extend to a definite height above the surface of the earth. According to this hypothesis, the refraction of a ray of light proceeding from a celestial body, takes place wholly at its entrance into the spherical shell of air which encompasses the earth; whence it follows that the path pursued by the ray in its subsequent passage through the atmosphere must necessarily be a straight line. In order to adapt his hypothesis to the calculation of a table of refractions, it was necessary for Cassini to determine the height of the homogeneous atmosphere, and the refraction corresponding to a given angle of incidence. The data which he used for this purpose were the observed refractions for two given zenith distances. These were, the horizontal refraction, which he made equal to 32′ 20″, and the refraction at 80° of zenith distance, which he determined to be 5′ 28″. Being now in possession of the elements of his theory, he calculated a table of refractions extending from the horizon to the zenith. Besides this table, which was designed to represent the refractions in summer, he calculated two other tables, one of which was adapted to the winter season, and the other to the intermediate seasons of spring and autumn. It was found that by estimating the solar parallax at 1′, agreeably to Kepler, the triple table was indispensable when the object was to represent the observed refractions throughout the year. Cassini, however, was induced to suspect from observations of the moon when in the dichotomies, that the solar parallax fell considerably short of 1′; nay, he even did not scruple to assert that in point of fact it might be regarded as insensible. By adopting this supposition, he found that the table which he had calculated for summer, was likewise applicable to the other seasons of the year; but it was impossible to pronounce with absolute certainty whether his view of the subject was the right one or not. The table, in its triple form, first appeared in the Solar *Ephemerides*, published by Malvasia, at Bologna, in the year 1662. The results were found to represent the observed refractions with a remarkable degree of accuracy. This will appear somewhat surprising when the erroneous nature of the hypothesis from which they were deduced is taken into account; for it is manifest that, so far from the atmosphere being homogeneous, the successive strata of air increase in density towards the surface of the earth, by reason of the pressure of the superincumbent strata, and the ray of light being in consequence deflected continually from a rectilinear direction, the path pursued by it through the atmosphere will, in reality, be a curve which is concave with respect to the earth.

Notwithstanding the success of Cassini's attempt to represent the derangements in the apparent positions of the celestial bodies, arising from the combined influence of parallax and refraction, it was still desirable, for the advancement of astronomical science, to arrive at a precise knowledge of the independent effects of each of these two elements. To accomplish such an analysis, however, by means of observations made at Paris, was found to be impracticable, in consequence of the refractions at the solstices being so considerable as to render uncertain any conclusion with respect to the effect occasioned by parallax. In order to obtain a satisfactory solution of this question, the Academy of Sciences resolved to determine the distance between the tropics by means of observations of the sun at some place near the equator, where, the refractions being insensible on account of the great zenith distance of the object, the effects of parallax alone would become apparent. In pursuance of this design they selected a small island on the coast of Africa, lying about 5° north of the equator, as a suitable locality for the proposed observations, and assigned the conduct of them to Richer, one of the members of their own body. Richer executed his task in the year 1672. The result of his observations afforded a complete verification of Cassini's views, by shewing that the solar parallax was insensible. Cassini, in consequence, adopted the table of refractions for summer as true for the whole year, and communicated them to the Academy of Sciences as the definitive result of his researches *. This table was inserted in the *Connaissance des Temps*, and continued for a long time to be published annually in that *Ephemeris*.

It appears from the foregoing account of Cassini's researches, that the calculation of a triple table of refractions adapted to the different seasons of the year, was forced upon him by the erroneous value of the solar parallax, generally prevalent in his time, and that the adoption of his own views with respect to the value of that element, made the refractions invariable throughout the year. It is, therefore, inconsistent with the facts of the case, to assert, as some writers have done, that we owe originally to Cassini the remark that the refractions vary with the season of the year. The first suspicion of this fact is undoubtedly due to his contemporary, Picard. At one of the meetings of the Academy of Sciences, held in 1669, having been invited to state the objects of research which appeared to him to be most conducive to the advancement of astronomical science, he suggested the desirableness of calculating a table of refractions for Paris, adapted to the different seasons of the year, and even according to the different changes of the weather, marking, on each occasion, the winds and the state of the thermometer, in order to ascertain whether the variations in the quantity of refraction are not accompanied by phenomena affording unequivocal indications of their existence †. Picard, on a future occasion, gave a more distinct explanation of his views on the subject. Having found from observations of the sun, that the refractions are more in excess after the winter solstice than before it, he attributed the difference to the greater degree of cold which usually prevails in the more advanced part of the winter season. Upon the same grounds he concluded that the refractions during the night are greater than those which take place during the day ‡. These views must be regarded as highly creditable to the sagacity of Picard, especially when it is considered that, with the exception of Newton, no other person appears to have formed an adequate conception of the importance of taking into account the influence of temperature upon refraction until towards the middle of the eighteenth century.

The subject of refraction is too closely connected with physical astronomy not to have occupied the attention of Newton. In the fourteenth section of the first book of the Principia he has shewn that when a ray of light enters a transparent medium, the attraction of the molecules composing the medium will deflect it from its original direction, and, by supposing them to act according to a given law, he has deduced the fundamental theorem of Snell, that the sines of the angles of incidence and refraction bear a constant ratio to each other. In his Treatise on Optics, he has alluded more particularly to atmospheric refraction §, but he does not attempt to determine the amount of deflection experienced from that cause by a ray of light proceeding from a celestial body to the surface of the earth. In the year 1721 Halley communicated to the Royal Society a table of refractions which he asserted to have been calculated by Newton ‖. This table exhibits the refractions from the horizon to 75° of apparent altitude. The refraction at the horizon is assigned equal to 33′ 45″; the refraction at 45° amounts to 54″. No information was given by Halley with respect to the way in which the table was constructed, so that it long continued to be an interesting question with astronomers, whether it rested solely upon observation, or whether it was derived from some physical theory of refraction. There existed indeed strong reasons to suspect, from Newton's mode of considering refraction as a result of dynamical agency, that the table was constructed upon theoretical principles. This has been established beyond all doubt in recent times, by the publication of Baily's Life of Flamstead, which contains some interesting particulars relative to Newton's researches on the theory of astronomical refraction*. It appears from certain correspondence between Newton and Flamstead, published in that work, that during the period which elapsed between October, 1694, and March, 1695, Newton was engaged in a profound investigation of the subject referred to, and that he finally transmitted to Flamstead a table of refractions, which, there is every reason to suppose, was identical with the one subsequently communicated by Halley to the Royal Society. A short notice of this correspondence, the publication of which has had the effect of throwing additional lustre around the genius of Newton, already immortalised by so many great discoveries, may perhaps not prove uninteresting to the reader.

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* Anc. Mém. Acad. des Sciences, tome viii., p. 81.

† Histoire Céleste, p. 17.

‡ Ibid., p. 19.

In a letter, dated October 11, 1694, Flamstead points out to Newton the discordances between the refractions near the horizon, as assigned by various astronomers, and the corresponding results which he had deduced from his own observations. He remarks, also, that, at the same altitudes, he found that the refractions were different on different nights. He invites the attention of Newton to the subject, and requests that he will communicate to him the result of his reflections upon it. The reply of Newton is valuable, inasmuch as it shews that he had already formed a true conception of the physical principles upon which atmospheric refraction depends. In a letter, dated October 24, 1694, he thus explains his views on the subject:—"The reason of the different refractions near the horizon, in the same altitude, I take to be the different heat of the air in the lower region. For when the air is rarefied by heat, it refracts less; when condensed by cold, it refracts more. And this difference must be most sensible when the rays run along in the lower region of the air for a great many miles together; because 'tis this region only which is rarefied and condensed by heat and cold; the middle and upper region of the air being always cold. I am of opinion also, that the refraction in all greater altitudes is varied a little by the different weight of the air discovered by the baroscope. For when the air is heavier, and by consequence denser, it must refract something more than when 'tis lighter and rarer. I could wish, therefore, that in all your observations, where the refraction is to be allowed for, you would set down the weight of the baroscope and heat of the air, that the variation of the refraction by the weight and heat of the air may be hereafter allowed for, when the proportion of the variation by those causes shall be known."†

Flamstead was unable to appreciate the importance of the sagacious hints thrown out by his illustrious contemporary. Although possessing many of the qualities requisite for constituting an accurate observer of celestial phenomena, he was naturally unfitted for the loftier task of investigating the various physical causes upon whose combined operation they depend. It is due to him, however, to state that, in the present instance, he does not appear to have been insensible to the advantages which might accrue to astronomical science from the theoretical researches of Newton, although he failed to profit by the counsel of that philosopher, in so far as his own practice as an observer was concerned.

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* Account of the Rev. John Flamstead, compiled from his own manuscripts and other authentic documents never before published. 4to. London, 1835.

† Ibid., p. 137.

The subsequent progress of Newton's labours on the subject of refraction is clearly exhibited in his letters to Flamstead. It has been already mentioned that he succeeded in explaining the refraction of a ray of light by the attraction of the transparent medium into which it enters. In the present case the medium was composed of an infinite number of concentric spherical strata of air, increasing in density towards the surface of the earth; so that, in point of fact, it was necessary to investigate the effect produced by an infinite number of media, all of which possessed different degrees of refractive power. This circumstance rendered the problem of refraction vastly more difficult than it had been on the hypothesis of Cassini; but on the other hand it was manifest that the condition of a variable density was that which accorded with the actual constitution of the atmosphere. Newton, by a happy application of dynamics to his doctrine of refraction, reduced the problem to a form which tended greatly to facilitate its solution. As each stratum of air acted uniformly upon the incident ray[*], the resultant attraction of the particles was a force directed to the centre of the earth, since no reason could be assigned why it should deviate to one side more than to another. Hence the investigation of the path pursued by the ray of light through the atmosphere, was brought under the theory of central forces, the principles of which Newton had already fully established in the first book of the Principia. One of the conditions of the problem was, the relation between the density of the air and its refractive power; another of equal importance was, the law according to which the density of the air diminished in ascending from the surface of the earth. Newton rightly supposed the refractive power of the air to be proportional to its density. The ascertainment of the *law* of density in ascending from the earth's surface was attended with much greater difficulty, since it depended upon a knowledge of the physical constitution of the atmosphere. Newton commenced his researches by supposing the density to diminish by equal degrees with equal increments of altitude. Upon this hypothesis he calculated three tables of refraction, one for winter, another for summer, and a third for the intermediate seasons of spring and autumn. These tables do not, by any means, exhibit a satisfactory accordance with the corresponding results indicated by observation. Newton, in fact, soon afterwards discovered that the law of density upon which they rested was erroneous, inasmuch as it supposed "the refracting force of the atmosphere as great at the top as at the bottom."[†] He accordingly abandoned it, as manifestly at variance with the actual state of nature, and proceeded to the consideration of another hypothesis, founded upon the constitution of the atmosphere, described by him in the 22nd proposition of the second book of the Principia. In this proposition the density of the air is supposed to be proportional to the pressure, and hence is readily deduced the theorem, that, if the distances from the centre of the earth increase in arithmetical progression, the densities of the corresponding strata of air will diminish in geometrical progression *. This hypothesis is more conformable to the real constitution of the atmosphere than that adopted by Newton in the first instance, for it has been demonstrated by experiment, that the density of air is proportional to the compressing force. On the other hand it is to be remarked, that the influence of temperature is left wholly out of consideration, although it is manifest that the superior heat of the lower regions of the atmosphere, by rarefying the air, tends inevitably to derange the law of density which would otherwise ensue. It may, therefore, easily be imagined that the new hypothesis of Newton did not yield results much more satisfactory than those derivable from the original one. In fact, he found that for low altitudes the refractions assigned by it were all by far too great. The cause of this discordance did not fail soon to disclose itself to his penetrating intellect. "I have found," says he, "that if the horizontal refraction be 34′, the refraction in the apparent altitude of 3° will be 13′ 3″; and if the refraction in the apparent altitude of 3° be 14′, the horizontal refraction will be something more than 37′. So that, instead of increasing the horizontal refraction by vapours, we must find some other cause to decrease it. And I cannot think of any other cause besides the rarefaction of the lower region of the atmosphere by heat." Having calculated a table of refractions upon this hypothesis, he transmitted a copy of it to Flamstead, inclosed in a letter dated March 15, 1695. The letter is preserved in the library of Corpus Christi College, Oxford; but the table of refractions which accompanied it has nowhere been found.

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[*] The forces of the molecular particles are supposed to be sensible only at insensible distances.

[†] Flamstead's Life, p. 147. The truth of this remark will appear from the following consideration. When a ray of light passes from one stratum of air into another, it is attracted in opposite directions by the two strata with forces proportional to their respective densities. It is clear, therefore, that the whole force which is effectual in refracting the ray is proportional to the difference of densities of the strata. Now, according to the hy-

The question naturally arises, was the table of refractions which Newton transmitted to Flamstead, similar in all respects to the one communicated by Halley to the Royal Society? In order to establish this point in the affirmative, it is necessary, in the first instance, to shew that both tables were calculated upon the same hypothesis respecting the constitution of the atmosphere. M. Biot was the first who proved by strict calculation, that a theory of atmospheric refraction, in which the densities of the successive strata of air are supposed to be proportional to the pressures, is capable of furnishing results identical with the refractions contained in Halley's table †. This was soon afterwards demonstrated still more unequivocally by Ivory ‡. It appears, therefore, that both tables in question were constructed upon the same principles. The sole object that still remained to be accomplished, in order to dispel all doubts respecting their common origin, was to establish the identity of their numerical results. This has been effected by the aid of a passage of Newton's letter to Flamstead, wherein he mentions that if the refraction at 3° of altitude be 13′ 20″ the table may be relied upon as exact to a second for all altitudes

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* Strictly speaking, the theorem requires that the distances from the centre of the earth should be in harmonical proportion; but on account of the immense magnitude of the terrestrial radius, compared with the altitudes of the successive strata of air, they may, without any sensible error, be taken in arithmetical progression.

† Connaissance des Temps, 1839. Addit., p. 105.

‡ Phil. Trans., 1838, p. 183.

above 10°. Now it appears by reference to Halley's table that the refraction at 3° is therein also equal to 13′ 20″. The conclusion, therefore, is unavoidable, that the refractions communicated by Newton to Flamstead and Halley, were both copies of the same original table, and that the latter was calculated upon the hypothesis that the densities of the successive strata of air are proportional to the pressures. This hypothesis necessarily implies a uniform temperature of the atmosphere.

It may well excite surprise, that, after Newton had established a theory of refraction which agreed in so many of its fundamental principles with the actual constitution of the atmosphere, and had also succeeded in accurately working out its results, he declined, notwithstanding, to communicate an account of his researches to the world. He was probably induced to adopt this course from a consciousness of the inadequacy of his theory to represent the actual refractions with all the accuracy demanded by the existing state of practical astronomy. We have seen that he did not fail to perceive that the rarefaction of the air in the lower regions of the atmosphere, would cause the refractions near the horizon to be in reality less than those assigned by theory. It does not appear, however, that he attempted to correct his results by taking into consideration the influence of temperature. He, doubtless, was of opinion that the law of the variation of this element in ascending from the surface of the earth, was too imperfectly known to justify the hope of ascertaining its effect upon refraction by a process of strict calculation. Indeed it may be asserted that the question with respect to the mean influence of temperature upon the constitution of the atmosphere, has proved a stumbling-block to every astronomer and mathematician who has undertaken to investigate the laws of refraction, from Newton's time down to the present day.

Although Cassini's table of refractions was generally adopted by astronomers, on account of its satisfactory agreement with the results of observations, it was well known that the hypothesis of a homogeneous atmosphere, upon which it rested, did not truly represent the actual state of nature. In 1702 La Hire attempted to determine the path pursued by a ray of light through the atmosphere, by taking into account the variable density of the air; but his investigation not being grounded upon accurate physical principles, the conclusion at which he arrived was necessarily erroneous*. In 1714, J. Cassini sought to effect the same object by arbitrarily assuming several curves as representatives of the path of a ray of light through the atmosphere, and trying which of them agreed best with the observed refractions. He found that by supposing the path of the ray to be circular, the refractions were represented better than by his father's theory†. It is hardly necessary to remark, that a problem so intricate as that of atmospheric refraction, does not admit of even an approximate solution by a process so purely tentative as that above hinted at.

The first investigation of the problem of astronomical refraction upon sound physical principles, or at any rate the one which was first communicated to the world, is due to the celebrated mathematician, Brooke Taylor. At the end of his treatise, "Methodus Incrementorum," published in 1715, he employs Newton's doctrine of refraction, as expounded in the sixth section of the first book of the Principia, to investigate the curve described by a ray of light in its passage through the atmosphere.

Assuming that the refractive power of the air is proportional to its density, and that the densities of the successive strata of air are proportional to the pressures, he succeeded, by the aid of analysis, in obtaining a rigorous solution of the problem; but the results at which he arrived were not exhibited in a form that was commodious for calculating a table of refractions. It is a remarkable fact, that while Taylor deduced the expression for the differential of refraction with all desirable accuracy, he stood in this respect almost alone, until Kramp published his researches on refraction towards the close of the eighteenth century*.

In 1729 Bouguer investigated the theory of astronomical refraction in a memoir which obtained for him the prize of the Academy of Sciences of that year. Instead of adopting the Newtonian doctrine of refraction, he imagined the incessant deflection of a ray of light proceeding from a celestial body through the atmosphere, to be occasioned by the action of a substance pervading the aerial fluid, which he termed the refractive matter, and which he supposed to dilate gradually in ascending from the surface of the earth. Assuming a certain hypothesis with respect to the dilatation of the refractive matter, he deduced a theorem for computing the refraction corresponding to any assignable altitude. The values of the constants upon which the practical application of this theorem rested, were determined by means of two observed refractions. These were the horizontal refraction, which he fixed at 33′, and the refraction at 26° of altitude, which he made equal to 2′ 12″. He then computed a table of refractions extending from the horizon to the zenith, which was found to exhibit a tolerably satisfactory agreement with observation.

In 1743 Thomas Simpson published his "Mathematical Dissertations," which contained an investigation of the problem of astronomical refraction. He remarked that the hypothesis respecting the constitution of the atmosphere which assumed the densities of the successive strata of air to be proportional to the pressures, was manifestly erroneous; for although it was quite true in the case of a uniform distribution of heat, it was inapplicable to the actual state of nature, in consequence of the air in the upper regions of the atmosphere being colder, and therefore less elastic, than the air near the surface of the earth. Hence, instead of supposing the densities of the successive strata of air to diminish in geometrical progression, which is the law corresponding to a uniform temperature, he supposed them to diminish by equal degrees as the altitude above the earth's surface increased, or, in other words, he supposed them to diminish in arithmetical progression relative to the altitude. Assuming the Newtonian doctrine of refraction as the basis of his investigation, he deduced a very concise theorem for calculating the refraction corresponding to any assigned zenith distance. The constants involved in this theorem were determined by means of two observed refractions, for which he was indebted to Dr. Bevis, a contemporary astronomer. These were the horizontal refraction, which was valued at 33′, and the refraction at 30° of altitude, which was fixed at 1′ 30″.5. By the aid of these data Simpson calculated a table of refractions, which was found to agree sufficiently well, under certain restrictions, with the results of observation in all those cases wherein the altitude was considerable. Another table was calculated by him, in which the fundamental refractions were somewhat larger than those above stated. This modification of the original table was intended to meet the case of a low temperature of the atmosphere. M. Matthieu was the first who shewed that Simpson's theorem is in point of fact identical with the one which Bouguer had deduced a few years previously, by means of an investigation founded on a totally different conception of the nature of refraction.

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* M. Matthieu, in his interesting note on the theory of astronomical refraction, inserted at the end of Delambre's "Histoire de l'Astronomie au Dixhuitième Siècle," has remarked that the expression for the differential of refraction assigned by Taylor is not rigorously true. M. Plana, however, has very plainly shewn that this remark is not correct, being quite irreconcilable with the process of investigation, as well as the numerical data of Taylor. (*Mém. Acad. Tur.*, vol. xxxii., p. 61.) M. Matthieu's mistake arose from attaching too literal a signification to a verbal expression employed by the English mathematician.

While mathematicians were engaged in researches on the theory of atmospheric refraction, astronomers were endeavouring, by the aid of observation, to calculate tables which should be capable of assigning the magnitude of refraction at any altitude above the horizon. It was long, however, before they perceived the necessity of taking into account the corrections depending upon the variations of the pressure and temperature of the air. Halley was the first who remarked that the quantity of refraction must necessarily vary with the weight of the atmosphere. In 1720 he read before the Royal Society a paper relating to J. Cassini's researches on the parallax of Sirius, wherein he asserted that Hawksbee having demonstrated by experiment that the refractive power of the air is proportional to its density, it followed that if the barometer were supposed to vary from 28 to 30 inches, or one-fifteenth part, the quantity of refraction at every altitude would undergo an equal degree of variation, and hence he inferred that the refraction of Sirius, which was 1′ 55″, might fluctuate from this cause to the extent of 7″ or 8″ *. We have already seen that Picard found reason to suspect that the refractions were affected by the variations of temperature. La Hire denied that observation afforded any proof of the refractions being liable to fluctuate from such a cause. No further notice of the subject appears to have been taken by any astronomer until Le Monnier directed attention to it on the occasion of his publication of the *Histoire Céleste*, in the year 1741. In the introduction to that work he asserted that the variations in the temperature of the atmosphere exercised a sensible influence on the refractions of the celestial bodies. From a great number of observations of circumpolar stars, made by him during the greatest heat of summer and the most intense cold of winter, he inferred that, at 4° 41′ of altitude, the variation of refraction was 2′, corresponding to a variation of the thermometer amounting to 36°.

The three celebrated astronomers, Bradley, Lacaille, and Mayer, whose labours, in so many respects, form the commencement of a new era in the history of astronomy, simultaneously introduced the practice of applying to a mean table of refractions, the corrections depending upon the daily fluctuations of the barometer and thermometer. From observations of the least and greatest altitudes of circumpolar stars, and of the altitude of the sun at the equinoxes, Bradley deduced a theorem of great simplicity and elegance for computing the mean quantity of refraction corresponding to any given altitude. The variable effect depending on the pressure of the atmosphere offered no difficulty, being simply proportional to the oscillations of the barometer. The correction for the variation of temperature was a delicate point of investigation. By instituting a careful comparison between the actual refractions, as determined at the extreme degrees of heat and cold throughout the year, Bradley deduced a rule for computing its values, which was found to present a remarkable agreement with the results of observation. The application of the corrections for pressure and temperature to the theorem for computing the mean refractions, furnished him with a general rule for computing the quantity of refraction, corresponding to any apparent altitude, and under every condition of the atmosphere, with respect to pressure and temperature. The refraction at 45°, when the barometer stood at 29.6 inches, and the thermometer at 50° Fahrenheit, was fixed by him at 56″.8. The general rule assigned by him was found to give results of remarkable accuracy, as far as 80° of zenith distance, and until about the beginning of the present century it was almost universally used in the computation of astronomical refractions.

Mayer's formula for refraction appeared in a posthumous work, containing his solar and lunar tables, which was published at London in the year 1770*. The main peculiarity of this formula was the thermometric correction, which, although somewhat different from Bradley's, was found to represent the effects produced by the variations of temperature with very great accuracy. Lacaille's researches on refraction were founded on a comparison of a great number of observations made by him at the Cape of Good Hope with corresponding observations made at Paris. By a comprehensive and skilful discussion of these observations he succeeded in forming a table of refractions extending from the zenith to a distance of 6° from the horizon. He made the refraction at 45° equal to 66″.5, the barometer being supposed to stand at 28 French inches, and the thermometer at 10° Reaumur. This determination was manifestly too great by 6″ or 7″, a circumstance which seemed inexplicable when the acknowledged accuracy of Lacaille, as an observer, was taken into account. Maskelyne was the first who traced the error to its true source, namely, to a defect in the instrument of Lacaille†. Delambre, by various calculations, has fully verified the assertion of the English astronomer‡.

The theory of astronomical refraction continued throughout the whole of the eighteenth century to exercise the talents of the most eminent geometers of Europe. Its slow improvement is mainly attributable to the imperfect knowledge that prevailed respecting the actual constitution of the atmosphere. In 1799, Kramp, a German mathematician, published a work containing a more complete investigation of the subject than any which had hitherto appeared. It had been already ascertained that, except for low altitudes, there were several hypotheses respecting the law of the density of the air that were capable of representing the observed refractions with almost the same degree of accuracy, provided the values of the constants were determined with sufficient precision. Kramp commenced his researches by supposing that the densities of the successive strata of air diminish in the same ratio with the pressures, an hypothesis which implied that the temperature of the air is uniform. By a rigorous analysis he deduced an expression for refraction involving two constants, the values of which it was possible to determine without having recourse to astronomical observation. These were the refractive power of the air, and its density at the surface of the earth corresponding to a given height of the barometer. We have seen that Cassini, Bouguer, and Simpson succeeded in determining the elements of their respective theories, only by means of two observed refractions of a celestial body. Kramp imagined that the formula he had deduced from the hypothesis of a uniform temperature would serve to represent the observed refractions as far as 84° of zenith distance. This, however, was awarding to it a greater degree of accuracy than it was really entitled to. For the refractions near the horizon he adopted a more comprehensive view of the subject, by taking into account the influence of temperature in modifying the law of the density of the air.

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* Tabulæ Motuum Solis et Lunæ. Lond., 1770.

† Phil. Trans., 1787, p. 173.

‡ Hist. Ast. au Dixhuitième Siècle, pp. 485–89.

The illustrious Laplace was the next person who contributed by his labours to the improvement of the theory of astronomical refraction. By pursuing a method of investigation founded upon the omission of small quantities, the influence of which, under certain conditions, would be insensible, he deduced a formula for refraction, which possessed the advantage of being independent of any hypothesis respecting the constitution of the atmosphere. The constants were the same as those which Kramp had introduced into his results. The importance of such a formula as that discovered by Laplace must be very obvious, when the difficulty of ascertaining the law of the density of the atmosphere is taken into consideration. It is but just, however, to state that the illustrious geometer referred to was anticipated on this occasion by Oriani, who had been conducted, by an investigation of the subject, to a similar result several years previous to the publication of the *Mécanique Céleste* *.

Laplace found, by strict investigation, that no appreciable error would be entailed on the final results by the peculiar mode of procedure which he employed in deducing the above-mentioned formula, so long as the zenith distance did not exceed 74°. The investigation of the law of refraction towards the horizon was attended with extreme difficulty, since it depended upon the peculiar constitution of the atmosphere, which in a great degree was unknown, on account of the uncertainty that prevailed respecting the precise influence of temperature. Having remarked that an atmosphere whose density diminished in geometrical progression made the refractions towards the horizon too great, while, on the other hand, one whose density diminished in arithmetical progression made the same refractions too small, Laplace was led to believe that an hypothesis which should combine both laws of progression would probably afford an accurate representation of the true constitution of the atmosphere. Pursuing this idea he investigated the law of refraction for low altitudes, and deduced a formula which he adapted so as to represent the observed refraction at the horizon. By means of this formula, which was designed to represent the refractions of all zenith distances greater than 74°, and the formula already referred to, which was applicable to the remaining part of the quadrant, a table of refractions was calculated under the superintendence of the French *Bureau des Longitudes*, and was first published in the year 1806. The refractive power of the air was determined astronomically, and also by direct experiments with the prism. Delambre, by a discussion founded upon a great number of Piazzi's observations of circumpolar stars, and also several hundreds of his own observations of the sun, determined the refraction at 45° of altitude to be equal to 57″.58, the barometer being supposed to stand at 29.6 inches, and the thermometer at 50° Fahrenheit. MM. Arago and Biot, by means of experiments with the prism, obtained a result indicating that, at the same altitude, and under the same conditions of temperature and pressure, the refraction of the atmosphere was equal to 57″.65. The agreement of this quantity with that found by Delambre must be regarded as very satisfactory, especially when the widely different nature of the methods employed in determining them is taken into account.

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* The investigation alluded to in the text, according to Plana, appeared in the Milan *Ephemeris* for 1788. (*Mém. Acad. Tur.*, tome xxvii.) Oriani's remarks upon the expression deduced by him are very clear in reference to the question of its generality. The following are his words, as cited by the philosopher just mentioned:—“Quæ expressio à nullâ pendet hypothesi, vel circa caloris legem in atmosphærâ, vel circa aeris

The table of refractions calculated upon the basis of Laplace's formula was found to represent the observed refractions better than any other that had been hitherto used by astronomers. It has since been published annually in the *Connaissance des Temps*, and has always been held in great esteem for its accuracy. From the zenith to a distance of 80° the results agree with the observed refractions to within 1″. For lower altitudes they exhibit a small error in excess, which increases continually towards the horizon *. In the *Connaissance des Temps* for 1851, M. Caillet has calculated the table anew upon the same basis, the results being worked out to a greater degree of accuracy, so as to adapt them to the present advanced state of astronomical science.

From the relation between the density and pressure of the atmosphere, assigned by his theory, Laplace deduced the law of the diminution of temperature in ascending from the surface of the earth. He endeavoured to verify his theory in this respect by comparing it with a thermometric experiment made by Gay Lussac at an altitude of 6980 mètres above the level of the Seine, on the occasion of his famous ascent from Paris in a balloon. The result of this comparison was considered by Laplace to afford a satisfactory confirmation of his theory, for a near agreement between the actual and calculated temperatures could not be expected to take place, on account of the oscillations that incessantly occur in the atmosphere. It must be admitted, however, that in the present instance the representation of the actual constitution of the atmosphere was far less satisfactory than in the case of the refractions, even after assigning their due weight to such temporary causes of discordance †.

In 1814, Dr. Brinkley communicated to the Royal Irish Academy a paper on the theory of astronomical refraction. Assuming, as facts established by experiment, that the sines of the angles of incidence and refraction bear a constant ratio to each other when the media remain the same, and that for media of different densities the refractive power is proportional to the density, he deduced the formulæ for refraction corresponding to various hypotheses respecting the constitution of the atmosphere, without having recourse to the Newtonian doctrine relative to the attraction of particles at insensible distances. From 525 of his own observations of circumpolar stars, he determined the refraction at 45° to be 57″.42, the barometer standing at 29.6 inches, and the thermometer at 50° Fahrenheit. Tables were given by him in the same paper to facilitate the calculation of refractions ‡.

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* Phil. Trans., 1838, p. 186.

† See Méc. Cél., tome iv., p. 265.

‡ Trans. Roy. Irish Acad., vol. xii., p. 77, et seq.

In 1823 the late Mr. Ivory communicated to the Royal Society an important paper containing an account of his researches on the theory of astronomical refraction *. Its chief peculiarity consisted in the mode in which the influence of temperature was considered. Laplace, as we have seen, did not introduce this element explicitly into his researches. He merely assumed an hypothesis respecting the constitution of the atmosphere, which he conceived would be adequate to account for the effects depending upon the combined variations of temperature and pressure in ascending from the surface of the earth. But although the results calculated on this hypothesis exhibited a remarkable accordance with observation, in so far as regards the refractions of the celestial bodies, still it was desirable to attain the same degree of accuracy by means of a theory in which the temperature of the air appeared in an explicit form as one of the fundamental elements upon which the ultimate results were based. In the outset of his researches Mr. Ivory supposed the temperature of the air to diminish at a uniform rate corresponding to equal increments of altitude. In order to calculate the refractions upon this hypothesis, it was necessary to ascertain the actual diminution of temperature corresponding to a given elevation above the surface of the earth. Mr. Ivory determined the value of this element by a comparison of thermometric experiments made at various altitudes by Gay Lussac, Ramond, and Humboldt. The resulting refractions agreed very well with the corresponding quantities deduced from observation; but as the hypothesis upon which they rested, assigned to the atmosphere a height considerably less than was otherwise probable, Mr. Ivory rejected it, and adopted another founded upon a somewhat different law of temperature, the accuracy of which he verified by shewing its agreement with the actual temperature and pressure of the air as deduced from experiments made at various altitudes above the level of the sea. The initial rate of the diminution of temperature, being a fact established by experiment, was necessarily the same in this as in the previous hypothesis. Mr. Ivory having deduced the results of his theory by a process in which his mathematical genius is strikingly conspicuous, employed them in calculating a new table of refractions. The elements were identical with those of the French tables, except that the constant representing the initial rate of the diminution of temperature was used instead of the horizontal refraction. The resulting refractions were found to agree with observation as nearly as any that had been hitherto calculated.

Mr. Ivory's theory of refraction possessed the advantage of being founded upon elements, the values of which it was possible to ascertain without having recourse to astronomical observation. It was also found to present a closer, as well as a more general, agreement with the actual diminution of temperature in ascending from the surface of the earth than any previous theory had done. Moreover, the results were obtained by employing an expression for the variation of the density of the atmosphere that was characterised by an extreme degree of simplicity. M. Plana is inclined to doubt whether any new theory of refraction, whatever peculiar advantages it may seem to hold forth, will be found which shall be capable of assigning results of the same degree of accuracy by means of an equally simple law of density †. The same distinguished philosopher has shewn that

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* Phil. Trans., 1823, p. 409, et seq.

† Mém. Acad. Tur., vol. xxxii. p. 173

by applying Ivory's law of the diminution of temperature to the superior regions of the atmosphere, the result agrees very nearly with that deduced by M. Fourier, relative to the mean temperature of the planetary regions depending upon the radiation of heat from the celestial bodies. Mr. Ivory developed his theory more fully in a paper which he communicated to the Royal Society in the year 1838*.

No essential improvement has been made in the theory of astronomical refractions since the publication of Mr. Ivory's remarkable results. The labours of those eminent geometers who have subsequently directed their attention to the subject, serve only to shew that the most consummate mathematical skill, apart from experimental researches, is no longer adequate to this end. A review of the progress of this interesting branch of physical enquiry cannot fail to suggest the conclusion that the steps which have led to its present advanced condition are mainly due to the successive labours of Newton, Taylor, Oriani, Kramp, Laplace, and Ivory. With respect to Newton, we allude solely in this remark to his exposition of the doctrine of refraction generally upon physical principles, since his unpublished researches on astronomical refraction, considered as a special branch of enquiry, could not possibly exercise any influence on the labours of his successors. On the other hand it is impossible to peruse without feelings of intense admiration the account of that illustrious philosopher's researches, as exhibited in his correspondence with Flamstead. The accurate conception which he forms of the relation between the phenomenon of refraction and the density of the refractive medium, the acuteness which he displays in detecting the various sources of its incessant fluctuations, and the sagacity with which he combines the physical elements upon which it depends, so as to form the groundwork of a profound mathematical investigation—are all unequivocal indications of a genius of the most exalted order, and such as proclaim, in no unworthy language, the immortal author of the Principia. Previous to the publication of his correspondence with Flamstead, it was supposed that his researches on the subject did not extend beyond the explanation of the general phenomena of refraction by mechanical principles, and the determination of the law of the density of the atmosphere upon the hypothesis of a uniform temperature. It now appears that, by a beautiful application of his theory of central forces, he completely solved the problem of atmospheric refraction upon more than one hypothesis respecting the law of density. The only real step in advance that has been made by Newton's successors consists in the introduction of the element of temperature into the researches on the subject†. The intimate connexion of this principle with the density of the atmosphere, and its consequent influence upon the refraction of a celestial body, did not fail to be discerned by him; but the imperfect state of thermotics in his time rendered hopeless any attempt to investigate the effects produced by it.

Whatever improvements may be effected in the theory of astronomical refraction by future enquiry will, in all probability, depend upon the success of experimental researches on the constitution of the atmosphere, instituted mainly with a view to ascertain, with a greater degree of precision, the mean law of the density of the air and the influence of the aqueous vapour diffused throughout the different strata. It is a fortunate circumstance that, except in those cases wherein the zenith distance is considerable, theory is capable of representing the observed refractions independently of a knowledge of the law of density. The question relative to the influence of the aqueous vapour suspended in the atmosphere is still involved in great obscurity, notwithstanding the researches of several eminent modern philosophers on the subject*.

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* Phil. Trans., 1838, p. 169, et seq.

† It is worthy of remark that those geometers who refused to admit the Newtonian principle of the attraction of the molecules of matter, failed by means of any other hypothesis to deduce the rigorous expression for the differential of refraction. Thus Plana has shewn (*Mém. Acad. Tur.*, tomes xxvii, xxxii.) that the expressions assigned by D. Bernoulli, Euler, Lambert, and Lagrange were all more or less erroneous. Kramp, Laplace, and all modern enquirers, except Brinkley, have employed the Newtonian theory.

On account of the imperfect state of the theory of refraction, many astronomers have been induced to rely mainly upon their own observations for the means of constructing tables which shall assign with precision the numerical value of this correction, corresponding to any given altitude. Allusion has already been made to the labours of Bradley and his contemporaries, with a view to the attainment of this end. In more recent times the subject has occupied the attention of many eminent astronomers. Piazzi, while engaged in the construction of his great catalogue of stars, employed a table of refractions which he deduced wholly from his own observations. Groombridge, Carlini, and many other astronomers of the present age, have distinguished themselves by researches of a similar nature. The labours of Bessel on this subject were worthy of his commanding powers of investigation. In the *Berlin Ephemeris* for 1813, he determined the refractions deducible from Bradley's observations. He resumed the subject in the *Fundamenta Astronomiæ*, employing the suggestions of theory to aid him in his researches. In the *Tabulæ Regiomontanæ* he has given a table of refractions extending from the zenith to a distance of 88½°. This table, being founded upon a skilful discussion of a great number of accurate observations, is regarded as one of the most trustworthy that has been hitherto calculated, and in consequence is generally used by the astronomers of the present day.

No table has yet been calculated which is capable of representing the refractions near the horizon with a degree of precision adequate to the requirements of astronomical observation. The physical constitution of the atmosphere, which fails to exercise any sensible influence on the refractions at considerable altitudes, plays an important part when the rays of light from the celestial bodies enter the refracting medium in very oblique directions; and, being liable to incessant oscillations from meteorological causes, it occasions thereby corresponding variations in the refractions†. Irregularities in this manner arise which are found to be altogether independent of the usual indications of the barometer and thermometer. Such anomalous effects are doubtless attributable to changes in the upper regions of the atmosphere, the nature and extent of which the observer is unable to ascertain even with the smallest degree of success. It would, therefore, appear to be a work of supererogation to calculate the mean refractions for low altitudes when the appropriate corrections to be applied to them do not exhibit sufficient indications whereby their values may be determined. The mode of procedure that appears to be best adapted for overcoming this difficulty, consists in an extensive study of meteorological phenomena, with a view to ascertain the laws of those oscillations which affect the mean condition of the atmosphere at different altitudes above the surface of the earth. It is gratifying to think that this branch of physical enquiry is at length beginning to receive an amount of attention adequate in some degree to its paramount importance.

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* The refractive power of aqueous vapour exceeds that of air of the same density; but it has been found that under the same pressure of the atmosphere, the density of air exceeds that of the vapour suspended in it, nearly in the same proportion. Hence the influence of aqueous vapour upon refraction has been generally supposed to be insensible.

† If the earth was bounded by a plane surface, and the air was arranged in strata parallel to it, the refraction suffered by a ray of light proceeding from a celestial body, would be the same as if it had at once passed from the ethereal regions into the air at the earth's surface. Hence, in such a case, the quantity of refraction corresponding to any given altitude might be deduced simply from researches on the density of the air at the earth's surface, independently of a knowledge of the law according to which the density varied. Now, when a ray of light proceeds from a celestial body near the zenith, it is manifest that, although the strata of air are in reality disposed in concentric spherical shells, they are nearly parallel to each other at the points where the ray impinges upon them. On the other hand, if the ray proceeds from a celestial object, situate near the

The grand discovery of the Aberration of Light resulted in an attempt to detect the annual parallax of the fixed stars. Although the facts revealed by the invention of the telescope and the discovery of gravitation, had the effect of establishing beyond all doubt the truth of the Copernican system of the world in the minds of astronomers, still it was desirable to obtain an assurance on this point, founded upon an argument which appealed more directly to the senses than any that had been hitherto adduced. The absence of any appreciable change in the positions of the fixed stars, when observed at opposite extremities of the terrestrial orbit, was one of the earliest, as it was unquestionably the most serious objection that had been urged against the earth's motion ; and it was always considered that the detection of such a change by observation would furnish an irrefragable proof that the earth is not the centre of the solar system. Astronomers in various ages had attempted to accomplish this object, but all their efforts had hitherto proved fruitless. One of the methods which seemed to be best adapted for this purpose was that employed by Hooke, of observing stars whose zenith distance is small, since the effects arising from any uncertainty in the value of refraction are thereby avoided. That astronomer had selected for the object of his enquiry the bright star γ *Draconis*, which passes within a few minutes of the zenith of Gresham College, where his instrument was erected; and by observing it carefully at different seasons of the year, he came to the conclusion that it had a sensible parallax. It was with the view of verifying the interesting result announced by Hooke that, towards the close of the year 1725, Molyneux, an amateur of astronomy, resolved to undertake a series of observations of the same star. In pursuance of this design he caused a large zenith sector to be constructed by Graham, the famous mechanician, which he erected at Kew, his place of residence. On the 3rd of December, 1725, the instrument was for the first time employed in determining the position of the star as it passed the zenith. Similar observations were also made on the 5th, 11th, and 12th of the same month, but no change in the position of the star having been detected, a circumstance which was naturally accounted for by the position which the earth then occupied in its orbit, it was deemed unnecessary to make any further

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at the points of impact rapidly diverge from parallelism. It is manifest, therefore, that while in the former case the law of the density of the atmosphere may be dispensed with, in computing the quantity of refraction, without entailing any material error, the same course of procedure cannot be pursued with equal safety in the latter case.

observations until the season of the year should arrive when the effect produced by parallax would be most readily apparent *. It was, therefore, more from curiosity than any other motive, according to his own statement, that Bradley, who was then at Kew, was induced to observe the star on the 17th of December. On this occasion the position of the star was found to be somewhat more southerly than it appeared to have been from the previous determinations. Suspecting that the discordance arose from an error in the observations, Molyneux and Bradley again determined the position of the star on the 20th of December, when to their great surprise they discovered that it had advanced still further towards the south †. This circumstance was the more surprising, as the direction of the star's apparent motion was opposite to that which would have resulted if the effect had been occasioned by parallax. The star continued to advance towards the south until about the beginning of March, 1726, when it attained a distance of 20″ from its original position. It now appeared for some time to be stationary, no sensible change of position having been indicated by several successive observations. About the middle of April it appeared to be returning towards the north, and by the middle of June it again passed the meridian at the same distance from the zenith as it did in the beginning of December. It continued to pursue the same direction until September, when its displacement towards the north became as great as it had been towards the south in the beginning of March. After remaining for some time stationary, it again reversed its motion, and finally arrived, in the beginning of December, in the same position which it occupied twelve months previously, allowing for the small change of declination, occasioned by the precession of the equinoxes.

The great regularity with which the phenomenon passed through its successive phases, clearly indicated that it was not accidental, but, on the contrary, that it arose from the uniform operation of some unknown physical cause. A nutation of the earth's axis suggested itself as probably capable of explaining the observed motion; but although this hypothesis accounted sufficiently well for the change of declination of γ *Draconis*, it was soon found to be inconsistent with similar observations of other stars. This was more especially true with respect to a small star opposite in right ascension to γ *Draconis*. In this case the direction of the apparent motion of the star was indeed such as would have resulted from a nutation of the earth's axis; but the actual change of declination was equal only to half that of γ *Draconis*, whereas, if the phenomenon had been really due to a nutation of the earth's axis, the change of declination would have been nearly the same for both stars. Their attempt to explain the phenomenon by refraction was also equally unsuccessful ‡.

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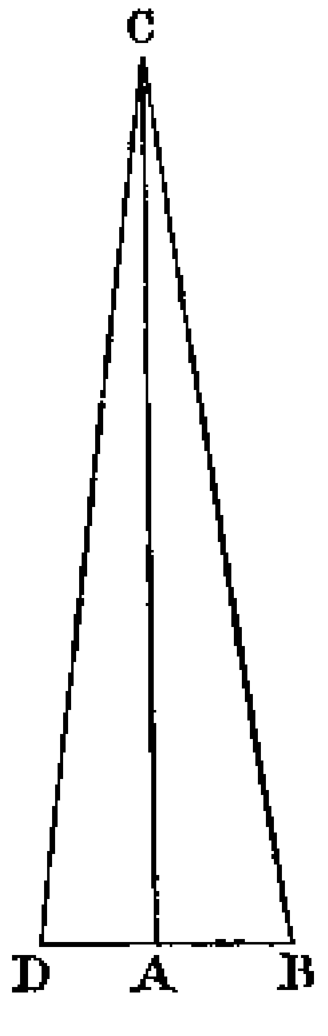
* The annual parallax of a fixed star in latitude, is at its maximum when the earth is situate in the plane of a great circle to the ecliptic passing through the star. In such a position it is manifest from the mathematical nature of a maximum or minimum, that the variation of parallax must be insensible. Now, since the star γ *Draconis* is situate near the solstitial colure (in 1700 its right ascension was about 267°), it is easy to see that the above condition would be nearly fulfilled about the time when Molyneux commenced his observations.

† The loose piece of paper upon which Bradley made the memorandum of this famous observation, has been found among his manuscripts. A fac-simile of this highly interesting relique is given by Rigaud in his *Miscellaneous Works and Correspondence of Bradley.*

‡ Their conjecture respecting the probability of the displacement being occasioned by

Being anxious to ascertain the laws of this singular phenomenon and to discover its physical cause, Bradley resolved to procure an instrument for himself, so that he might make observations for that purpose with greater convenience. He therefore made Graham construct for him a zenith sector which measured 12½ feet in length, and had an arc of 6¼°. By means of so large an arc he was enable to make choice of more than 200 stars of different magnitudes contained in Flamstead's Catalogue*; but his principal reason for having recourse to such a construction was, that he might observe *Capella*, which was the only star of the first magnitude that passed near his zenith.

The instrument having been erected at Wansted in Essex, on the 19th of August, 1727, Bradley proceeded forthwith to make observations with it, selecting such stars as appeared to be best adapted for throwing light upon the phenomenon†. Of these there were twelve that could be observed at all seasons of the year, being bright enough to be seen in the day-time when nearest the sun. He had not long continued his observations when he found that only those stars which were situated near the solstitial colure were farthest north and south when the sun was about the equinoxes; and he finally discovered it to be a general law that each star was farthest north when it passed the zenith about six o'clock in the evening, and farthest south when it passed about six in the morning. When his observations had extended over a whole year, he proceeded to compare them together, and having obtained a satisfactory knowledge of the laws of the phenomenon, he began to enquire into its probable cause. He had already arrived at the conviction that it could not be due to a nutation of the earth's axis. He had also ascertained that it could not in any way arise from refraction. He now considered the effect of an alteration in the direction of the plumb line, but he soon found it was inconsistent with his observations. At length the happy idea occurred to his mind, that the phenomenon might be completely accounted for by the gradual propagation of light, combined with the motion of the earth in its orbit. The following is a brief exposition of the mode in which he conceived it to originate.



Let C A be a ray of light falling perpendicularly upon the line D B. Then, if the eye is at rest at A an object at C must appear in the direction A C whether light be propagated in time or in an instant. But if the eye is moving from B towards A, and if light at the same time is propagated with a velocity that is to the velocity of the eye as C A to B A, then the particle of light which enters the eye at A must have been at C when the eye was at B. Hence, if B C be a tube capable of admitting only one particle of light, it is manifest that the particle at C will descend along the tube if it be inclined at the angle A B C, but that it will not reach the eye if the tube be inclined at any other angle. In like manner, if B C represent the tube of a telescope through which the object is viewed, it must be inclined at

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refraction, was founded on the supposition of an annual change in the figure of the atmosphere, produced by the motion of the earth through a resisting medium.

[*] The instrument of Molyneux had a range of only a few minutes; its length, however, was twice that of Bradley's.

[†] Molyneux assisted Bradley in erecting his sector. On the 22nd of August, 1727,

the angle A B C, in order that the particle of light entering the eye at A, shall have descended along the optical axis of the telescope. If the eye had been moving with the same velocity but in the contrary direction, viz., from D towards A, then the tube must have been inclined at the angle A D C, which is equal to the former angle. Although therefore, the true place of the object at C, when viewed from A, is perpendicular to the direction in which the eye is moving, it will appear in the direction of the tube, and the magnitude of the displacement hence arising will manifestly depend upon the relation between the velocity of light and the velocity of the eye. If light moved 1000 times faster than the eye, the angle A B C would be equal to 89° 56½′, and the displacement of the object would consequently amount to 3½′.

Admitting that the earth revolves round the sun in an annual orbit, and that the velocity of light exceeds that of the earth, in the ratio of 1000 to 1, it follows from what has been already stated, that an object situate in the pole of the ecliptic would appear during the course of a year to describe a small circle whose radius is equal to 3½′, its true place being the centre of the circle. Bradley further ascertained that an object situate in any other position of the celestial sphere would describe annually a small ellipse round its true place, the minor axis of which would coincide with the circle of longitude passing through the object. He also inferred from his hypothesis that the major axis of the ellipse would be equal to the radius of the small circle which an object situate in the pole of the ecliptic would describe, and that the minor axis would be to the major axis as the sine of the latitude to radius.

Since the radius of the small circle, which an object situate in the pole of the ecliptic would thus seem to describe, depends upon the ratio of the velocity of light to the orbitual velocity of the earth, it is manifest that if the radius of this circle be determined by observation, the ratio in question may hence be readily ascertained. Now Bradley found from observations of γ *Draconis* that the radius of the small circle amounted to 20″.2, whence he concluded that the velocity of light exceeds the velocity of the earth in its orbit in the ratio of 10,210 to 1. Knowing the velocity of the earth he hence obtained $8^m\ 12^s$ for the time which light occupies in passing from the sun to the earth. By means of similar observations of various other stars he found that the major axis of the apparent ellipse described by each star, or in other words the diameter of the little circle which a star situate in the pole of the ecliptic would describe, was contained somewhere between 40″ and 41″. Assuming its true value to be 40½″ he hence inferred that light occupied $8^m\ 13^s$ in passing from the sun to the earth. This appeared to him to afford an interesting confirmation of his hypothesis, being a mean of the results deduced at different times from the eclipses of Jupiter's satellites.

Having determined the maximum value of the inequality, Bradley next

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he thus writes in the journal of his observations at Kew:—"Now Mr. Bradley's instrument is set up and we go on comparing notes from this time." This plan of observing together was not, however, realised to any great extent. Only six observations subsequent to the erection of Bradley's instrument, are recorded in the Kew Journal. The last is dated the 29th of December, 1727. Molyneux, having been appointed one of the Lords of the Admiralty a few months previously, doubtless found it to be incompatible with the due discharge of his official duties, to continue his astronomical observations. He died in April, 1728. (Rigaud's *Miscellaneous Works and Correspondence of Bradley.*)

proceeded to compare its variations throughout the year, with the corresponding numbers assigned by his hypothesis. The result of this comparison was in the highest degree satisfactory. Of seventy declinations of γ *Draconis* determined in the course of a year, one only differed more than 2″ from the hypothesis, and even in this case the observation was marked doubtful on account of clouds. The comparison of the observed and computed declinations of η *Ursæ Majoris* exhibited an equally gratifying accordance *.

The phenomenon referred to in the foregoing account received from Bradley the name of *Aberration.* Its discovery is universally regarded as one of the most important accessions which astronomical science has ever received. The sagacity which Bradley evinced as well in the establishment of its laws, as in the detection of its physical cause, would entitle him to a place among the greatest philosophers of ancient or modern times, even if no other triumphs had adorned his career.

It has been mentioned that Bradley estimated the maximum of aberration at 20″.25. Upon further consideration he was induced to fix it at 20″ †. The delicacy of modern researches on astronomy has rendered a closer approximation to the true value indispensable, even although the corrections assigned by the various enquirers who have devoted their attention to the subject, do not in any case exceed a few tenths of a second. Delambre, by comparing together observations of a thousand eclipses of Jupiter's first satellite, deduced 20″.255 as the most probable value of the coefficient of aberration. This result, as we have seen, agrees very nearly with that originally obtained by Bradley. Various determinations of this element, have been assigned by modern astronomers, which generally fluctuate between 20″.7080 and 20″.2116. In 1844, Baily, having compared together all those that were known to him, deduced 20″.4192 as the most probable value. This result has received a strong confirmation from the recent researches of MM. Peters and Struve. M. Peters, by means of a great number of observations made at Pulkowa, with a vertical circle constructed by Ertel, has determined the maximum value of aberration to be 20″.481. M. Struve makes it 20″.445 by a discussion of observations made also at Pulkowa, with a transit fixed in the prime vertical ‡.

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* Bradley's account of this brilliant discovery is contained in the volume of the Philosophical Transactions for 1729. It will be seen that his researches are wholly founded on observations of the declinations of the stars. It does not appear that he attempted to verify his theory by means of right ascensions. At any rate he makes no especial allusion in his paper to the effect which the displacement would produce upon those coordinates, although the fact announced by him, that every star described an apparent ellipse necessarily implied variations in right ascension, corresponding to those in declination. It is evident that he deemed observations of right ascension not to be sufficiently trustworthy for so delicate an investigation. In 1730 Manfredi asserted that his observations, although subject to occasional irregularities, exhibited a motion in right ascension, analagous to that which Bradley had detected in declination. (Comm. Acad. Bonon, vol. i. p. 634.)

† Phil. Trans. 1748, p. 23.

‡ Struve's Etudes d'Astronomie Stellaire, p. 96. In the same work it is stated that M. Peters obtained 20′.676 for the maximum value of aberration with a probable error of 1″.11, by a discussion of certain observations of Flamstead's which are contained in a letter from that astronomer to Wallis, published in the third volume of the Opera Mathematica of the latter. Flamstead was under the impression that the variations of the star observed by him (*Polaris*) were occasioned by parallax.

When Bradley was engaged in investigating the phenomenon of aberration, he found that in the case of some stars near the equinoctial colure, the change of declination was greater than that which would have resulted from a mean annual precession of 50″. Similar observations of stars near the solstitial colure, indicated an effect of a contrary nature, the change of declination being less than the quantity due to the same value of precession. It was manifest, from this circumstance, that the phenomenon could not be attributed to an error in the motion of the equinoctial points. Bradley made repeated observations with the view of discovering its real nature from the year 1727, when his instrument was first erected at Wansted, until 1732, the year of his removal to Oxford. During this interval of time some of the stars near the solstitial colure had changed their declination 9″ or 10″ less than the quantity due to a precession of 50″; while others near the equinoctial colure had exhibited an equal change of declination in the opposite direction. The north pole of the equator appeared to have approached the stars which came to the meridian with the sun about the vernal equinox and the winter solstice, and to have receded from those which came to the meridian with the sun about the autumnal equinox and the summer solstice.

After pondering for some time on the phenomenon, Bradley began to suspect that it might arise from the variable action of the moon on the matter accumulated round the terrestrial equator. On account of the motion of the moon's nodes, the inclination of the lunar orbit to the earth's equator is in a state of perpetual change, whence it might readily be inferred that the efficacy of the moon to disturb the earth would be subject to a corresponding variation. During a revolution of the moon's nodes the inclination of the lunar orbit passes through all its values, increasing during the half of this period to the extent of 10°, and then returning during the other half to its original state. It therefore occurred to Bradley that the motion of the equinoctial points, which partly depends upon the disturbing action of the moon, would be subject to an oscillatory movement of sensible magnitude, the period of which would coincide with the time of a complete revolution of the moon's nodes. Upon comparing the observations of stars near the solstitial colure that were almost opposite in right ascension, he found that the change of declination, though opposite in direction, was equal in quantity; for while γ *Draconis* had advanced northward, the small star 35 *Camelopardali* appeared to have moved as far towards the south. A nutation of the earth's axis was, therefore, in this case, capable of explaining the phenomenon, although it was found to be inadequate to that end, when the question referred to aberration. The same conclusion followed from a comparison of observations of other stars that, taken two and two, were nearly opposite in right ascension.

In 1727, when Bradley commenced his observations, the moon's ascending node was in Aries, and the inclination of the lunar orbit was at its maximum. In 1736 the lunar node was in the beginning of Libra, having completed half a revolution in virtue of its retrograde motion, and the inclination of the orbit had consequently now descended to its minimum. During the intermediate period the change in the declination of stars near the solstitial colure, was less by 18″ than that which would have resulted from an annual precession of 50″; for γ *Draconis*, which ought to have advanced about 8″ farther towards the *south*, was observed in 1736 to be situate 10″ more to the *north* than it was in 1727. If quantity greatly exceeded the rate of diminution which had hitherto been deduced from the observations of astronomers, Bradley was induced to ascribe the phenomenon to an oscillation of the earth's axis, occasioned by the disturbing action of the moon upon the terrestrial equator. Being unable, however, by means of observations, extending only over a period of nine years, to judge whether the axis would recover its former position, he found it necessary to continue his observations throughout a whole revolution of the moon's nodes. In the year 1745, when the moon's ascending node had again arrived in Aries, he had the satisfaction to find that the stars returned to the same position which they would have occupied, if the inclination of the terrestrial axis had been the same as it was in 1727. This circumstance appeared to him an indubitable indication that he had detected the true cause of the phenomenon.

Machin, to whom Bradley communicated an account of his discovery, devised a geometrical theory of the phenomenon, by the aid of which he computed a table of its numerical values. He supposed that while the mean pole of the equator described a circle round the pole of the ecliptic, at the rate of 50″ in a year, occasioning thereby the uniform motion of precession, the apparent pole of the equator described round the mean pole, a small circle whose radius was equal to 9″, being in that part of the circle which is farthest from the pole of the ecliptic when the moon's ascending node was in the beginning of Aries, and in the opposite point when the same node was in Libra.

Bradley proceeded to establish a strict verification of his hypothesis, by comparing it with a great number of his observations. The result of this comparison afforded a complete confirmation of his views. Having computed the mean declination of γ *Draconis* from 300 distinct observations, by applying to each observation the appropriate corrections of precession, aberration, and nutation, he obtained a series of results which presented a remarkable agreement with each other. Eleven only were found to differ so much as 2″ from the mean of the whole, while not one differed so much as 3″ from it. The result was equally satisfactory in the case of several other stars, which he employed in testing his hypothesis *.

It has been stated that Bradley fixed the coefficient of nutation at 9″. The researches of subsequent enquirers seem to indicate that the true value exceeds this quantity by a few tenths of a second. Maskelyne made it 9″.55. Laplace, by the aid of theory, deduced 9″.40 as its most probable value. Lindenau, by a discussion of a vast number of observations, extending from 1750 to 1815, and consequently including three complete revolutions of the moon's nodes, obtained 8″.989 for the coefficient of nutation. This result is now generally supposed by astronomers to fall below the true value. Dr. Brinkley, by comparing 1618 observations of different stars, made it 9″.25. Henderson, from the value of the moon's mass, indicated by his researches on the lunar equatorial parallax, determined the coefficient of nutation to be 9″.28. Dr. Robinson, from upwards of 6000 observations with the mural circle, made by Pond at Greenwich, between the years 1812 and 1835, deduced 9″.23913 as its most probable value. Various similar evaluations have been effected by modern astronomers. Baily, by a careful comparison of all the results, came to the conclusion that the most probable value of the coefficient is 9″.25.

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* See the original Memoir of Bradley (Phil. Trans., 1748, p. 1 et seq.).

In order to render observations of the planets available as elements of theoretical research, it is necessary to reduce them from the place of observation to the sun, the physical centre of the planetary system. Two steps are requisite for this purpose. The first consists in reducing the apparent position of the planet from the place of observation to the centre of the earth. To this end it is indispensable to obtain a knowledge of the diurnal or geocentric parallax, in other words, the apparent displacement occasioned by viewing the body from a point on the surface of the earth instead of its centre. The second step in the reduction of a planetary observation, consists in transferring the position from the centre of the earth to the centre of the sun. This object is effected by means of the annual or heliocentric parallax. When once the absolute magnitude of the terrestrial orbit has been determined by means of the solar parallax, the heliocentric parallax of any planet, is readily ascertained by the aid of Kepler's third law. The reduction of a planetary observation is thus ultimately made to depend upon the evaluation of the solar parallax. The progress of researches on this important subject has already been briefly alluded to in one of the preceding chapters.

After the astronomer has succeeded in tracing the laws of the various principles which exercise an influence on the apparent positions of the celestial bodies, and has determined their maximum effects with all desirable precision, there still remains the laborious task of computing the displacement occasioned by them corresponding to any assigned instant. Various expedients have been devised with a view to abbreviate the toilsome calculations attendant upon this operation. In the *Connaissance des Temps* for 1760, there appeared tables having for their object, to facilitate the computation of aberration and nutation for a few of the principal stars. Similar tables continued to be published in the volumes for the subsequent years, the numbers of stars to which they were adapted, gradually increasing, until at length it amounted to 510 in the volume for 1806. In the same year tables designed to facilitate the calculation of aberration and nutation were published by Cagnoli and De Zach. Cagnoli's tables appeared in the form of an appendix to a catalogue of 501 stars which he published at the same time *. They were applicable only to the stars contained in the catalogue. De Zach's tables were attached to a catalogue of 1830 zodiacal stars; but their application was confined to 494 of the principal stars in the catalogue †. In 1812, the last-mentioned astronomer gave an important extension to his previous labours by the publication of tables of aberration and nutation, adapted to a catalogue of 1440 stars ‡. A defect of these tables consisted in the omission of the solar nutation, an element of displacement which, although of inconsiderable importance compared with the other corrections, is capable of exercising a very sensible influence on the delicate investigations of sidereal astronomy.

It cannot fail to excite surprise that, until a very recent period, no general methods were devised by astronomers, having for their object to facilitate the reduction of observations, by combining together in one homogeneous system of calculation, as far as was practicable, the separate processes for determining the various inequalities which affect the apparent positions of the celestial bodies. With respect to refraction, its magnitude in any particular instance depends upon the *altitude* of the object above the horizon, and the state of the *barometer* and *thermometer* at the time of observation. It is obvious, therefore, that it cannot be conveniently combined in calculation with precession, aberration, or nutation, the respective values of which depend on the *position* of the object in the celestial sphere and on the *time* of observation. An objection of a similar nature occurs, when the question relates to the combination of the geocentric parallax with any other correction.

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* Catalogue de 501 étoiles, suivi des Tables relatives d'Aberration et de Nutation; Mod., 1807.

† Tabulæ Speciales Aberrationis et Nutationis, &c.; 2 tom. 8vo., Gotha, 1807.

On the other hand, the inequalities of precession, aberration, and nutation, offer obvious facilities for the invention of a method which may embrace them all in one uniform process of calculation. For a long time, however, no vigorous effort appears to have been made with a view to the attainment of this desirable end. Astronomers continued to calculate the three inequalities by separate processes, notwithstanding the analogy they bore to each other when considered merely in reference to the determination of their numerical values. The reduction of any single observation, could, therefore, be effected only by executing a variety of distinct processes of calculation; and it was doubtless owing to the labour necessarily demanded by such an operation, that vast masses of observations came to be accumulated without being reduced at all.

About the year 1820 the subject began to attract serious attention, and methods designed to facilitate the reduction of observations upon the principle of combination above referred to, were suggested by various astronomers, both in this country and on the Continent. Bessel at length proposed a method which, on account of its superior advantages, has supplanted all the others. The complete correction of a star's place (in so far as precession, aberration, and nutation are concerned), whether in right ascension or in declination, is by this method expressed in terms of four products, each product being composed of two factors, one of which depends on the *place* of the star in the celestial sphere, and the other on the *time* of observation. The factors which depend on the time of observation are the same both for right ascension and declination, but those depending on the place of the star are different. Hence the reduction of an observation, at least as far as relates to the three corrections above-mentioned, demands the computation of twelve constants, eight of which depend on the *place* of the star, and four on the *time* of observation. Now, when the mean place of a star is once determined, the constants of the first-mentioned group may be computed by an easy process, and it is manifest that when this is once accomplished, the results will serve equally well for all observations of the star. On the other hand the four constants which depend on the time of observation, will vary for every day of the year, and will even be different on the same day of each successive year. Hence these constants, unlike the former, will require to be calculated for each day of observation. They possess this peculiar advantage, however, that when once calculated for any day of the year, they will apply equally well to any star in the heavens on that day.

The facility with which this method adapts itself to the computation of the apparent places of the stars by the aid of a catalogue of their mean places is very obvious. The eight constants depending on the place of each star, may be calculated once for all, and inserted in the catalogue. The four constants depending on the time of observation, cannot, of course, be included absolutely in any catalogue; but their successive values may be calculated for a definite peroid of time, so as to be applicable to any day of observation comprehended within that period. These objects being accomplished, the determination of the complete correction of any star in the catalogue for any day of the year, and the consequent ascertainment of its apparent place on the same day, is an operation which may be performed in two or three minutes. The advantage which the theoretical astronomer must possess, in being thus relieved at the outset of his researches from a vast amount of tedious calculation, is too obvious to require further notice. The values of the four constants depending on the time of observation, are calculated for each day of the year, and are published annually in the *Nautical Almanac*[*]. Mr. Airy has recently proposed a modification of Bessel's method, designed to obviate certain inconveniences which in some respects attend its practical application[†].

The Uranographical principles, to which allusion has already been made in this chapter, exercise a direct influence on the apparent positions of the celestial bodies, and therefore an accurate knowledge of the effects produced by them is indispensable in all those questions which concern the relations of space and time. Moreover, the principle of refraction, by affecting the aspect of phenomena, demands attention in various researches relating to celestial physics.

But the principles of Diffraction and Irradiation are also known to exercise an influence similar to that ascribed to refraction in the latter instance. These, indeed, by occasioning an alteration in the apparent magnitudes of the celestial bodies might be supposed in some cases ultimately to affect their apparent positions. Their influence in this respect, however, has been hitherto found to be inappreciable. On the other hand it cannot be doubted, that they sensibly affect the aspect of eclipses both of the sun and moon, as well as transits, occultations, and various other phenomena. A brief historical account of researches on these principles, considered only in so far as their influence on astronomical science is concerned, may, perhaps, not be out of place on the present occasion.

The discovery of the Diffraction of Light is due to Grimaldi, an Italian philosopher of the seventeenth century[‡]. Having admitted a beam of light through a very small aperture into a dark room, he found that the shadows of bodies placed in the cone of light thus formed, when received upon a screen, were larger than they would have been if the rays of light had passed by the edges of the bodies in right lines. He perceived also around the shadows three fringes of coloured light. The fringe nearest the shadow in each case was the broadest; the one most distant from it was the narrowest and faintest of the three. Grimaldi concluded from these facts that light was deflected when it passed by the edges of bodies. He first gave an account of his experiments in a work published at Bologna, in the year 1665[§].

The phenomena of diffraction did not escape the attention of Newton. In the third book of his Optics he gives some account of his researches on the subject. He mentions, however, that he was interrupted in the midst of his experiments, and that at no subsequent period could he find leisure to resume them. This part of the researches of the illustrious philosopher must therefore be considered as left in an unfinished state.

Newton repeated Grimaldi's experiment, by admitting a beam of light into a dark room, through a small hole about the 42nd part of an inch in diameter, and he found, like that philosopher, that slender bodies, placed in the cone of light, threw upon a screen, shadows of much larger dimensions than they ought to have done if light had passed in right lines. A human hair, equal in breadth to the 280th of an inch, placed at a distance of 12 feet from the hole, threw a shadow upon a screen 4 inches beyond it, which was equal to the 60th part of an inch in breadth *, and consequently was more than four times broader than the hair itself. Newton established, by a variety of experiments, that the phenomena of diffraction were absolutely independent, both of the nature of the substance which threw the shadow, and of the form of its edge. They appeared to him, therefore, to depend upon some principle quite distinct from refraction. He made some interesting experiments on the fringes which surround the shadows of diffracting bodies, and concluded his labours on the subject with certain queries suggestive of the nature of diffraction.

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[*] These are the constants denominated A, B, C, D, the numerical values of which are inserted in succession at page xx. of each month, in the first part of the Ephemeris.

[†] Mem. Ast. Soc., vol. xvi., p. 256, et seq.

[‡] This property of light is also sometimes distinguished by the term "Inflexion."

[§] Physico Mathesis de Lumine. Bonon, 1665.

The phenomena of the diffractive fringes have engaged the attention of many eminent philosophers of the present century, but an account of their labours does not come within the province of this work. The principle of diffraction has been supposed in various ways to affect the aspect of celestial phenomena; but the case in which its influence has been most completely established, is that wherein the object-glass of the telescope constitutes the diffracting body. The disks and rings exhibited by the fixed stars, when viewed in telescopes of high power, have been fully accounted for in this manner, by adopting as the basis of enquiry, the principles of the undulatory theory of light. In the next chapter we shall have occasion to mention several phenomena which philosophers have endeavoured to explain by the diffraction of light.

A principle which is supposed to exercise a more varied and extensive influence on celestial phenomena than the diffraction of light, is that which has been distinguished by the term "Irradiation." In virtue of its operation a luminous object projected upon a dark ground is dilated in all directions, so as to appear somewhat enlarged in dimensions. A striking example of the effect of irradiation is afforded by the appearance of the moon when only a few days old. In this case the luminous crescent is seen to form part of a much larger circle than the remaining portion of the disk, the outline of which is rendered distinctly visible by means of the *lumière cendrée*, or reflected light of the earth. As might be expected, a phenomenon the converse of this is exhibited, when an opaque body is seen projected upon a luminous surface. The object then appears to be contracted in dimensions, from the light encroaching upon it on all sides.

Some of the phenomena of irradiation are so palpable to observation, that they could not fail to have attracted notice at a very early age. It appears, however, that Kepler was the first who pointed out the consequence generally arising from the operation of this principle. His remarks on the subject are contained in his "Supplement to Vitellion," a treatise on Optical Science which he published in the year 1604 †. After citing the case of the new moon as above referred to, he remarked that the aspect presented by the lunar eclipse of May 25, 1603, afforded a similar illustration of the effect produced by irradiation. Again, it was found that during eclipses of the sun, the deficient segment of the solar

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* Optics, p. 114 (4to., Lond., 1704).

† Ad Vitellionem Paralipomena quibus Astronomiæ pars optica traditur, 4to., Franc., 1604

disk always appeared too small, in consequence of the light of the visible segment dilating in all directions and encroaching on the dark limb of the moon. As a simple mode of exhibiting the influence of irradiation, he remarked that if a ruler be held before the eye so as to intercept the light of the moon, it will appear narrower at the part where it is projected upon the lunar disk.

We owe also to Kepler an explanation of the physical cause of irradiation. He supposed that the rays of light from remote objects converged before reaching the retina, and then proceeded in a divergent course, so that each pencil of rays depicted upon the optic membrane a small surface instead of a point. It followed as an obvious consequence of this theory, that the object would appear somewhat enlarged. Kepler, however, was of opinion that unless the retina was very sensitive, no impression was produced upon it beyond the central point of the small area corresponding to each pencil of rays. Hence, according to this view of the subject, the phenomena of irradiation would be visible only to a particular class of individuals *.

The foregoing explanation is exceedingly plausible, but a slight examination will suffice to show that it is inconsistent with the results of observation. In the first place, it is not true that the rays of light emanating from remote objects, converge before reaching the retina. The eye in fact possesses a power of adaptation which enables it to bring the rays to a focus upon the optic membrane, however great may be the distance at which the object is placed. Secondly, it is to be remarked that even if this assumption was well founded, the hypothesis would still be inadequate to explain the phenomena of irradiation, since the latter do not fail to present themselves to observation at the distance of distinct vision. Lastly, the phenomena of irradiation are generally visible to all persons, although they exhibit themselves with greater intensity to some persons than they do to others.

The irradiation of luminous bodies was a subject which attracted the attention of Galileo on many occasions in the course of his physical investigations, and a variety of interesting remarks upon it are to be found interspersed throughout his writings. In the "Sidereus Nuncius" he asserts that the fixed stars, when viewed with the telescope, are not enlarged in the proportion of the magnifying power. In explanation of this fact he remarks, that the telescope has the effect of stripping the star of the false light by which it is usually surrounded when viewed with the naked eye. This spurious *corona* is ascribed by him to the influence of irradiation. It generally increases with the brightness of the field upon which the luminous object is projected. Thus at sunset, when the obscurity of the heavens is tempered by the influence of the twilight, the stars, even of the first magnitude, appear excessively minute. So with respect to Venus, notwithstanding the splendour with which she usually shines, she does not exceed a star of the sixth magnitude on those occasions when she happens to be visible at noon †.

Galileo next alludes to irradiation in one of his letters to Welser on the solar spots. Scheiner had remarked that Venus would appear as large on the sun's disk, as she does when she is near her superior conjunction. Galileo, however, denied the truth of this statement. He asserted, in the first place, that the irradiation of the planet usually causes her to appear much larger than she really is. In confirmation of this fact he remarked that even when the planet assumes the form of a very slender crescent, she appears round like any other star, her true figure being masked by the effulgence of her irradiation. On the other hand, when she appears projected on the luminous surface of the sun, she ought to exhibit a corresponding diminution of magnitude, from the irradiation of the solar light encroaching everywhere upon her disk. Upon these grounds he contended that the planet ought to appear considerably less on the solar disk than she does in any other position*.

In his "Discourse upon Comets," he finds occasion to refer to the irradiation of light in refuting an argument used by his opponents, which was based upon an absurd opinion respecting the theory of the telescope†. It was contended that comets are very remote bodies, because when viewed in the telescope they did not exhibit any sensible enlargement. This fallacy was suggested by the analogy of the fixed stars, for as these bodies did not appear sensibly enlarged in the telescope, and at the same time were well known to be very remote, so it was hastily concluded that the small effect produced upon their appearance in the telescope was due to their great distance. Galileo demonstrated by an appeal to experiment, and by reasoning of invincible force, that the element of distance did not exercise the slightest influence on the enlargement of an object seen through the telescope‡. The fact that some bodies were enlarged in a greater degree than others, was ascribed by him to the irradiation of light. The telescope in most cases deprives the body of the false light arising from this cause, so that although it does not fail to enlarge the real angular dimensions of the body in the proportion of the magnifying power, the spurious diminution may so far neutralise the actual enlargement, that the appearance presented by the body in the telescope, may not differ sensibly from that which it exhibits to the naked eye. It is to be remarked, however, that the irradiation exhibited by an object to the naked eye, depends upon the darkness of the ground upon which it appears projected; while on the other hand, since it is the object itself freed from irradiation that we perceive in the telescope, its aspect will not be affected in this case by the ground of projection. Hence the apparent magnitude of a star will vary to the naked eye with the obscurity of the heavens, while in the telescope it will not undergo any change. As an illustration of the truth of this remark, Galileo cites the case of Sirius, the brightest star in the heavens. A few hours before sunrise it does not appear much larger in the telescope than when viewed with the naked eye. As the approaching sun, however, begins to chase away the darkness of night, the star appears less and less to the naked eye, until at length, when the sun is about to ascend above the horizon, it dwindles to a mere point of light, and then disappears. On the other hand, during all this time the star constantly exhibits the same magnitude in the telescope *.

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* Opere di Gal., tom. ii., p. 129; Istoria e Dimostrazioni intorno alle Macchie Solari, p. 110.

† Discorso delle Comete di Mario Guiducci, Firenze, 1619. This dissertation upon comets, although professedly written by Guiducci, was well known at the time of its publication, to have proceeded from the pen of Galileo, and it has always been included in the successive editions of the complete works of the illustrious philosopher.

‡ The following passage referring to this principle, may serve to illustrate Galileo's mode of exposing the fallacies of his opponents:—"Place an opaque disk at a certain distance, and directly behind it, but four or six times farther off, place a white disk of so much greater dimensions than the other, as just to allow its circumference to be seen like a white ring. Now, if the telescope be directed to the two disks, it ought to follow, if the more remote disk be magnified in a less proportion than the nearer one, that the former will be altogether covered by the latter, and the ring will totally disappear. Hence, by applying the same reasoning to eclipses of the sun, it might happen that an eclipse which would appear partial to the naked eye, would become total by viewing it through the telescope; so that while with the optical instrument we found ourselves plunged in the darkness of night, we should be enjoying the brightness of daylight with the unassisted eye!"—*Opere di Gal.*, tom. ii., p. 221; *Discorso delle Comete*, p. 26.

Galileo makes some interesting remarks on irradiation in his letter to Grienberger, on the subject of the lunar mountains †. It had been asserted that, as the *full moon* always presented a well-defined outline, whether when viewed with the naked eye or through a telescope, it was impossible that there could exist any inequalities around her circumference. Galileo, however, maintained that the irradiation of the moon's light, by obliterating any asperities around her edge, might effectually conceal the real nature of that part of her surface. With respect to irradiation generally, he remarked that it increases with the brightness of the object. It is from this cause that the planets near the sun have a greater irradiation than those that are more remote. So intense is the irradiation of Mercury, that it is impossible, even with the most powerful telescope, to deprive him of his brilliant *corona*. The same is true, though in a less degree, with respect to Mars. On the other hand, Jupiter, and especially Saturn, being more feebly illuminated by the solar light, lose their irradiation in the telescope, and disclose their true figures. With respect to Venus, when she is near her inferior conjunction, she resembles the new moon, but such is the intensity of her irradiation, that she appears to the naked eye, like any other star. In this position, however, as the extent of the illuminated surface is small, and the light is at the same time enfeebled by the obliquity of the surface, it is possible, by means of a telescope, to discern the real appearance of the planet. When, however, she is near her superior conjunction, she presents a complete hemisphere of vivid light towards the earth, of such intensity, that even the most perfect telescope does not suffice to destroy her irradiation, and reveal to us her true figure.

Galileo, therefore, contends that since the effect of irradiation is so great, as to conceal from the unaided eye the immense cavity of Venus when she assumes the form of a crescent, it is much more probable that even the telescope will fail so completely to efface the irradiation of the moon, as to disclose the small eminences and cavities which may be situate near the edge of her disk. With the view of illustrating the fact that the irradiation of objects whose apparent magnitude is small, may produce so strong an effect as totally to obliterate any asperities of outline, he describes the following experiment:—Take a thin plate of metal in which are inserted two chinks, the one with smooth and the other with rugged edges. Let the piece of metal be illuminated behind by a brilliant light, and let the observer place himself in front, the only light accessible to him being that which is transmitted through the chinks. If he view the piece of metal at only a short distance, the asperities of the rugged chink will be easily perceptible to the naked eye. If he remove to a distance of a hundred or a hundred and fifty paces, the asperities cease to be visible to the naked eye, but they may still be discovered by the aid of the telescope. Finally, if he view the piece of metal at a distance of about a mile, the asperities of the rugged chink will no longer be discernible even with a telescope, so that the edges of both chinks will assume the same smoothness of outline.

Galileo remarked on frequent occasions, that irradiation produced a more sensible effect according as the apparent magnitude of the object was less*. This, indeed, is manifest to any person, when it is considered that the false light arising from irradiation, being the same whatever be the magnitude of the object, cannot fail to affect small objects in a greater degree than large ones. It was upon this ground he explained the fact that while the aspect of the stars varied with the obscurity of the heavens, the moon continued to retain the same apparent magnitude, whether seen in the brightness of noon or in the darkness of midnight.

With respect to the cause of irradiation, Galileo imagined it to reside in the eye, but he does not seem to have entertained a very decided opinion with respect to its mode of operation. In his " Discourse upon Comets " he conceived the irradiation of luminous objects to arise from the refraction of the rays of light by the humours diffused over the eye†. In his Dissertation, entitled " Il Saggiatore," he enters more fully into the same question. It had been supposed by some persons, that irradiation was occasioned by objects illuminating the surrounding air, so that the *coronæ* of light thus formed were confounded with the objects themselves. Galileo admits that when the sun or moon are near the horizon, they illuminate the vapours through which they are seen; but he denies that the illumination is so intense as to render the vapours liable to be confounded with the actual disks of either of those bodies. He then remarks, that the refraction of the moisture diffused over the eyes, causes a luminous appearance to surround the object, but being very feeble, compared with the original light, it does not affect the apparent magnitude. It will be seen that he here disavows the opinion previously entertained by him, to the effect that irradiation proceeded wholly from refraction. He now considers the irradiation of luminous objects to arise from the rays of light being reflected by the extremities of the eyelids, and then being dispersed over the pupils‡. He reproduces this opinion in his famous " Dialogues on the System of the World,"§ but in a subsequent part of the same work he ascribes the phenomena of irradiation to either of the two causes he had already assigned, or to some other unknown cause residing in the eye‖.

The irradiation of luminous objects being, according to Galileo, attributable to some ocular cause, it was clear that phenomena of this nature could not be affected by the magnifying power of the telescope. He explained by this principle the fact that the irradiation generally disappeared when the object was viewed through a telescope, the spurious light being masked by the enlargement of the apparent dimensions of the object.

The sagacity with which Galileo discusses the subject of irradiation, cannot be sufficiently admired. The least satisfactory part of his speculations is that relating to the physical cause of the phenomenon. It must be acknowledged that his views on this point are very obscure and vague. The reader cannot fail to remark that he concurs with Kepler in placing the source of irradiation in the eye.

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* Opere di Gal., tom. ii., p. 350; Ibid., tom. iv., p. 242.

† Ibid., tom. ii., p. 223; Discorso delle Comete, p. 32.

‡ Ibid., tom. ii., p. 348; Il Saggiatore, p. 212.

§ Ibid., tom. iv., p. 69.

‖ Ibid., tom. iv., p. 242.

Gassendi is the next person who occupied his attention with the subject of the irradiation of light. This philosopher entertained very erroneous views respecting the physical cause of irradiation, but he made some interesting experiments for the purpose of ascertaining the laws by which it is regulated. In order to ascertain the effect produced upon the enlargement, according as the field upon which the luminous object appeared became brighter, he measured the successive magnitudes of the moon's apparent diameter, as visible to the naked eye, from midnight till noon, and obtained the following results. At midnight the apparent diameter was found to be 38′; at dawn it was $36\frac{2}{5}'$; in clear daylight, $34\frac{2}{5}'$; after the sun arose, but before he had emerged above the vapours of the horizon, $34\frac{2}{5}'$; at mid-day, when the sun shone with all his splendour, 33′ *.

Descartes, in his *Dioptrics*, published at Leyden in 1637, at length gave the explanation of irradiation which is now generally received. He supposed that the extremities of the fibres of the optic nerve, though small, have some magnitude, and that in consequence, the impression produced by each pencil of rays which converged upon the retina, had a tendency to propagate itself to a certain distance on all sides, although it would be strongest at the point where the rays converged. When the light of the object was feeble, the impression might practically be supposed to be confined to the central point; but in the case of very bright objects, the impact of the rays might be so intense as to make the lateral impression very sensible. It followed as an obvious consequence of this mode of viewing the subject, that all luminous bodies would appear somewhat enlarged beyond their true apparent dimensions.

Newton, having discovered the unequal refrangibility of light, did not fail to perceive that the aberration arising from this cause alone, would have the effect of vitiating all observations of the apparent magnitudes of bodies, as seen through telescopes. In fact the rays of different colours belonging to each pencil of light, instead of converging to a point by the refraction of the object-glass of the telescope, were so dispersed as to assume the appearance of a small coloured circle at the focus, and hence obviously would arise an enlargement of the image beyond its true magnitude. Newton remarked that the enlargement arising from the unequal refrangibility of light would be less according as the focal length of the telescope was increased. In the Principia, he states that the apparent diameter of Jupiter, at his mean distance from the earth, was found by the aid of Huyghens' telescope of 123 feet focal length to be 39″; while, on the other hand, its value deduced from the times of the satellites crossing the planet's disk, amounted only to $37\frac{1}{4}''$. He, therefore, concluded that the chromatic aberration of the telescope was somewhere about 2″.† Hence in reference to the apparent diameter of Saturn, which was found by means of the same telescope to be 18″, he remarks that "if all false light be rejected, the diameter of the planet will not remain greater than 16″."‡

It is remarkable that Newton makes no allusion to irradiation in the course of his enquiries relative to the true apparent diameters of the planets. It is difficult to say, whether his silence arose from an absolute disbelief in the existence of any enlargement arising from this cause, or from an impression that the enlargement, which generally appeared less in the telescope, was in the present instance totally inappreciable, in consequence of the immense focal length of the instrument by the aid of which the measurements were effected. But whatever may have been the opinion of Newton on this point, it is certain that after his discovery of the unequal refrangibility of light, the subject of irradiation ceased to assume the same degree of importance which induced Galileo so repeatedly to refer to it in the course of his researches; its effects being for a long time confounded with those depending on the aberration of the telescope.

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* Opera Omnia, tom. iii., p. 395; Flor., 1727.

† Prin., lib. iii., Phenom. i.

‡ Prin., lib. iii., Phenom. ii.

The phenomena exhibited during the transits of Venus across the sun's disk in the years 1761 and 1769, had the effect of again directing the attention of astronomers to the subject of irradiation. Lalande found by calculations based on the times which Venus actually occupied in crossing the sun's disk during the transits of 1761 and 1769, that the apparent diameter of the sun, as assigned by the solar tables, was too great by 6″ or 7″ *. Du Séjour was conducted to the same result by his elaborate researches on the annular eclipse of 1764. He found that the observations of the eclipse could not be satisfied, except by supposing that the apparent diameter of the sun, as deduced from the solar tables, was too great by 9″ or 6″.6, according as the tables of Clairaut or those of Mayer were employed †. Lalande seems disposed to consider the enlargement of the solar diameter as due to the aberration of the telescope. Du Séjour applies the term *irradiation* to the enlargement, asserting, however, at the same time, that he does not mean thereby to announce any opinion respecting its physical origin.

In 1782, while Sir William Herschel was engaged in a series of experiments for the purpose of determining the apparent diameter of the planet he had recently discovered, he noticed an interesting fact illustrative of the irradiation of light. The method he employed for measuring the planet's disk consisted in comparing its image as seen through a telescope with an artificial lucid disk, formed by making a circular aperture in pasteboard, and covering it with transparent paper illuminated behind by a flame. The image of the planet was observed in the telescope with the right eye, while the lucid disk was viewed directly with the left, its distance being made to vary until it appeared exactly of the same size as the image in the telescope. Hence, the linear diameter and distance of the artificial disk being known, it was easy to deduce its apparent diameter, and by combining this datum with the magnifying power of the instrument, the apparent diameter of the planet was also readily ascertained. In order to ensure an accurate comparison of the disks, Herschel prepared a series of circles, varying in diameter from 2 to 5 inches, and increasing successively by tenths of an inch. He remarked, that as the brightness of the image in the telescope varied with the altitude of the planet and the magnifying power of the instrument, so it was necessary, in these experiments, to alter in a corresponding degree the brightness of the lucid disk, for he found that the magnitude of its apparent diameter was very sensibly affected by this cause. In fact, having placed several of the artificial disks together and illuminated them at the same time, he found that a very small quantity of additional light was necessary, in order to cause one of the disks to differ in magnitude by one or even two tenths of an inch *.

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* Mém. Acad. des Sciences, 1770, p. 403.

† Ibid., 1775, p. 365.

Dr. Robinson, in a paper communicated to the Astronomical Society in 1831, gives an interesting account of some experiments which he made for the purpose of ascertaining the amount of influence exercised by irradiation on the apparent diameters of the sun and moon †. The method he employed consisted in observing lucid disks of different magnitudes, and measuring their apparent diameters when subjected to different degrees of illumination. A slip of brass with a small circular hole in it was placed in the focus of an object-glass, and the hole being illuminated by a lamp placed behind it, was then viewed *through the object-glass* by means of a telescope directed to it. By this contrivance the image of the hole seen in the telescope was rendered similar to that which would have been formed by a remote object like the sun or moon, since the rays of light transmitted through the hole emerged in parallel directions from the object-glass. On looking through the telescope, the aperture assumed the appearance of a luminous disk about 17′ in diameter; but, on interposing a slip of oiled paper between it and the lamp, it assumed a much duller appearance, resembling the aspect of the moon in a dense fog. In this state Dr. Robinson brought the wires of the micrometer so as to be tangents to the disk; and then, having caused the oiled paper to be removed, he instantly saw the opposite segments of the disk extend beyond the wires. Bringing the latter again into a tangential position with respect to the disk, he determined the apparent diameter anew, the excess of which over the previous measure manifestly gave him twice the breadth of the annular enlargement arising from the increased brightness of the disk. In this way he obtained a series of results, the mean of which indicated an irradiation amounting to about 2″.6. He repeated the experiment with apertures of different apparent diameters, and obtained a similar result in each case. Dr. Robinson was induced to conclude from his experiments that the main source of irradiation is in the eye; at the same time he admitted that the aberration of the telescope might in some degree contribute to the apparent enlargement of the object. The telescopes which he employed on this occasion did not appear to be affected by any sensible aberration, from the distinctness with which they exhibited very close double stars, so that the enlargement indicated by his experiments must be considered as referable almost wholly to irradiation.

In one of his experiments it appeared to Dr. Robinson, that the brightness of the lucid disk was equal to that of the sun as seen in his transit. It might be expected, therefore, that the irradiation would be nearly the same in both cases. The experiment gave him 2″.8 for the difference between the least and greatest apparent diameters of the disk. This result presents a satisfactory agreement with two values of the solar irradiation deduced from astronomical researches of a more recent date than those of Lalande and Du Séjour. By a discussion of the transits of Venus in the years 1761 and 1769, Encke obtained a value of the solar diameter which was less than that of the tables by 2″.95 ‡. Wurm, by a similar discussion of the annular eclipse of 1820, deduced a result indicating that the solar diameter of the tables was too great by 3″.32 §.

In one of his experiments, Dr. Robinson illuminated the field of view for the purpose of ascertaining whether any effect would be produced on the irradiation. When the disk was in its state of greatest illumination no change was perceptible; but on turning off the light from the field of view, when the disk was obscured by the interposition of the oiled paper, a manifest enlargement was immediately apparent. The absence of any sensible enlargement in the first instance, arose doubtless from the circumstance of the brilliant light of the disk effectually overpowering the fainter light diffused over the field of view.

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* Phil. Trans., 1783, p. 1, et seq.

† Mém. Ast. Soc., vol. v., p. 1, et seq.

‡ Mém. Ast. Soc., vol. v., p. 7.

§ Ibid., p. 8

As it appeared evident from these experiments that the irradiation of an object was sensibly influenced by the brightness of the ground on which the object was projected, Dr. Robinson imagined that its effect might be eliminated altogether from observations of the sun and moon by sufficiently illuminating the field of view of the telescope. To effect such an illumination, however, by an artificial process was absolutely impossible, when the question related to observations of the sun, nor was it even generally practicable in the case of the moon. Under these circumstances, Dr. Robinson hit upon the ingenious idea of illuminating the field of view by deriving the light from the luminary itself. In pursuance of this design he proposed to cover the object-glass of the telescope with a semi-transparent diaphragm, leaving a small central aperture to admit the rays which formed the image at the focus, while the rest of the diaphragm served to fill the field with scattered light. This method was applied by Dr. Robinson to observations of the sun, and the results obtained by him in a few instances, appear to have been very satisfactory; the value of the solar diameter determined by means of the transit instrument, exceeding only by a very small quantity, the value assigned by the tables.

One curious fact noticed by Dr. Robinson, in the course of his experiments, is worthy of mention. Having examined the luminous disk with a double image micrometer, he was surprised to find that the contact of the two images was not affected by the intensity of their illumination. He expressed himself as somewhat doubtful of the result, the experiment having been hastily made. It would appear, however, to be supported by experiments of a similar nature made by other astronomers. M. Arago had previously attempted to ascertain the influence of irradiation by measuring with a rock crystal micrometer the apparent diameters of luminous disks, and then comparing them with the corresponding results obtained by combining the absolute diameters with the distance of the disks from the eye; but in all such experiments he found that the irradiation was insensible, even when the illumination of the disks was more intense than that of the full moon*. A similar result was obtained by M. Bessel when he sought by means of his great heliometer to determine the influence of irradiation on the occasion of the transit of Mercury across the sun's disk in the year 1832. Since, in this case, the effect of irradiation is to dilate the apparent diameter of the sun, and diminish that of the planet, it is not difficult to see that if such an effect really existed in any sensible degree, the luminous thread which succeeds the first interior contact of the two bodies ought to acquire *instantaneously* a certain degree of breadth at the point of contact; and similarly the luminous thread which precedes the second interior contact ought to exhibit a sudden rupture at the point of contact as if some protuberance had all at once been formed on the planet. Bessel was unable to discern the slightest indication of either of these phenomena during the transit of the planet above referred to. But he obtained a still more unequivocal proof that the effect of irradiation was insensible. Since the first interior contact of the two bodies is indicated by the sudden closing together of the two extremities of the luminous border of the sun, while the second interior contact is made apparent by the equally sudden rupture of the opposite border, it is manifest that the interval of time included between the two contacts when taken in conjunction with the relative motion of the planet, as assigned by the tables, will serve to determine the apparent diameter of the sun. Now Bessel, by employing this method, arrived at a result which coincided exactly with that obtained by direct measurement of the solar diameter with a micrometer during the transit of the planet.

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* Traité d'Astronomie Physique, par M. Biot; 2ieme edit., tome ii., p. 534.

Thus it appears that the experiments of Bessel concur with those of M. Arago and Dr. Robinson in indicating that the effect of irradiation is insensible when the instrument employed in measuring it is one which gives a double image of the luminous body. We shall presently have occasion to mention that experiments of this nature have been supposed to furnish only particular illustrations of a general law affecting the irradiation of two luminous objects placed in juxtaposition.

In the year 1838 M. Plateau communicated to the Royal Academy of Brussels, an elaborate memoir, containing an account of a variety of experiments performed by him with the view of elucidating the nature and laws of irradiation *. A very simple mode of exhibiting the enlargement arising from this cause was devised by him. A white card was divided into six rectangular compartments, by drawing two parallel lines pretty close to each other down the middle of the card, and then bisecting these by a third line, drawn at right angles to them. The middle rectangle in the upper half of the card, and the broad lateral rectangles in the lower half, were then painted black, so that there appeared in the middle of the card a band composed of two parts, the one black, (in the upper half of the card,) projected upon a white ground, the other white, (in the lower half,) projected upon a black ground. This contrivance manifestly tended to render the effect of irradiation more perceptible; for while the white band in the lower half of the card was liable to dilate from the encroachment of the light upon the black ground, the black band in the upper half was equally liable to contract from the encroaching irradiation of the white light on each side of it. The card being placed vertically near a window, so as to be well exposed to the light, was then viewed at the distance of a few yards, when the effect of irradiation was clearly exhibited by the white band in the middle of the lower half appearing sensibly broader than the black band above it. The following are the more important conclusions to which M. Plateau was conducted by his experimental researches on the subject of irradiation.

1° The quantity of irradiation increases with the brightness of the object, but in a much less rapid proportion. It has very nearly attained its maximum when the brightness is equal to that exhibited by the northern region of the sky.

2° Two irradiations in close proximity tend to neutralize each other. The diminution arising from this cause is greater in each case, as the interval between the luminous objects is less.

3° The quantity of irradiation augments with the time of contemplating the object. It is different for different individuals, and for the same individual it varies from one day to another.

4° When an object is viewed through a telescope, the apparent enlargement exhibited by it arises from two distinct causes, viz., ocular irradiation, and the aberration of the telescope. The part due to ocular irradiation depends on the magnifying power of the telescope, on the brightness of the image, and on the physiological qualities of the eye of the observer. Moreover, the interposition of the eye-glass of the telescope tends to produce a peculiar effect on the enlargement.

5° The part of the enlargement due to the aberration of the telescope varies necessarily with the instrument employed. For the same instrument, it may be considered as constant.

The most remarkable of these propositions is that wherein it is announced that the irradiations of two luminous objects are diminished by their mutual proximity. M. Plateau extends this principle to the phenomena of irradiation seen with the naked eye as well as to those observed in the telescope. He rests its demonstration upon various experiments, some of which are of a very convincing nature, and do not seem to be liable to any objection. He refers to this principle the absence of any sensible irradiation in the experiments of Arago, Robinson, and Bessel, above alluded to. Without expressing a formal opinion on its origin he seems disposed to consider it as due to the fact, that there does not exist in either case a sufficient contrast between the luminous object and the ground upon which it appears projected. M. Plateau concurs with most other philosophers in placing the source of irradiation in the eye, but he does not express any opinion respecting the mode by which the effect is produced.

The most recent account of experimental researches on Irradiation, is contained in a paper communicated by Professor Powell to the Astronomical Society, in the year 1849*. That distinguished philosopher appears disposed to regard the phenomena of irradiation as due, in a great degree, to some cause extraneous to the eye. While admitting that the irregular scintillation of a star may arise from some physiological affection of the organ of vision, and may vary in different individuals, he considers that the enlargement exhibited by a well-defined disk, capable of exact measurement, cannot reasonably be ascribed to a cause of so fluctuating a nature. He then proceeds to describe various experiments which tend to support this assertion. One of the most unequivocal results derived from these experiments consisted in this, that an image of the object, formed in the focus of a lens, is affected by irradiation in an equal degree with the object itself. A card, similar to that employed by M. Plateau, being exposed to a moderately strong light, and its image being then thrown by reflexion upon a ground glass placed in the focus of a lens, the image of the card was seen painted on the ground glass, with precisely the same enlargement as that which it exhibited to the naked eye. It might be urged against this experiment, that the brightness of the light at the focus of the lens might in its turn produce its irradiation, so as to occasion the apparent enlargement of the image. With reference to this objection Prof. Powell remarks, that the image on the ground glass was in general by far too faint to produce any such secondary effect. That the irradiation could not be due to this cause was proved further beyond all doubt by the fact, that when a dark glass was interposed between the eye and the image of the card in the focus, the enlargement still continued to be equally perceptible. Prof. Powell, upon the grounds of such experiments, considers the conclusion to be unavoidable, that the enlargement is occasioned by some optical cause acting upon the formation of the focal image, and independent of any organic affection of the eye.

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* Mém. Ast. Soc., vol. xviii., p. 69, et seq.

Pursuing this view of the origin of irradiation, he conceived that a striking verification of it would be obtained, if a photographic image of the object could be formed, exhibiting the enlargement. An experiment which he made with a view to decide this point, was attended with complete success. The image of the card when formed by a photographic process, either in the light of the bright sky or in the full sun, was found to exhibit a manifest enlargement.

Irradiation may be greatly diminished, or even completely destroyed, by the interposition of a lens between the object and the eye. Prof. Powell found that with ordinary daylight a lens which magnified three or four times was sufficient to extinguish every trace of the phenomenon. He states, as the result of his experience generally, that magnifying powers varying from ten to twenty effectually destroy the irradiation occasioned by the brightest light which the eye can bear.

M. Plateau experienced great difficulty in reconciling the effect produced by the interposition of a lens with the theory of irradiation, which ascribes its origin to some ocular cause. Prof. Powell is of opinion that it may be accounted for by the diffusion of light, resulting from the application of the lens, rendering the enlargement too faint to be perceptible. Upon the same principle he explains the absence of irradiation in experiments made with a double image micrometer. In this case, each of the images possesses only half the original brightness, and being at the same time viewed with a high magnifying power, the effect of irradiation might, by the combined operation of both these causes, be rendered insensible.

In observations with the telescope it has been found, that the regular enlargement occasioned by irradiation is mainly dependent on the aperture, focal length, and magnifying power of the instrument. With respect to the new moon, Prof. Powell remarks, that when viewed with a 30-inch achromatic, and very low powers, the projection of the illuminated crescent beyond the dark part of the disk was distinctly seen; with a power of 50 it was barely visible; and with 80 not at all. The following passage of a letter from Prof. C. Piazzi Smyth, cited by that philosopher, affords an interesting confirmation of the same fact:—"On a particularly fine night at the Cape, when the enlargement of the bright part of the new moon beyond the dark was unusually striking to the naked eye, amounting to two or three minutes of space, a 14-foot reflector was turned to the object, and, with every increase of power, the apparent projection was more and more cut down—definition was very good that night. With power of 300 the projection was barely sensible; it did, however, still absolutely exist, but to perhaps not more than two or three tenths of a second of space." *

It is generally admitted that the irradiation of bodies is affected by the dimensions of the aperture of the telescope. When the aperture is diminished, the apparent enlargement of the object is diminished also. This, doubtless, arises from the less intense brightness of the image. It is to be remarked, however, that when the aperture is reduced to very small dimensions, the diffraction of the object-glass begins to affect the apparent magnitude of the image in the opposite direction, so that when the aperture has reached a certain degree of contraction, the enlargement arising from this cause may compensate for the diminished effect of irradiation. Prof. Powell is of opinion that this may account for Dr. Robinson having found the interval between the transits of the sun's limbs to be unaltered by contracting the aperture of the telescope, and also for the fact that Mr. Dawes, during the transit of Mercury over the sun's disk in 1848, found that the apparent diameter of the planet was the same, when observed with different apertures.

A review of the progress of researches on irradiation cannot fail to suggest the conclusion, that the knowledge we possess respecting its nature and laws is still very imperfect and obscure. On the other hand, no doubt can exist that it exercises a sensible influence on the observations of the astronomer. It is manifest also, that as the art of observation continues to advance towards perfection, it will become more and more imperative on the part of the astronomer to attend to the effects produced by it. In the present state of the subject the inquirer is impeded at the very outset of his researches, by the want of data of sufficient completeness and precision upon which he might establish his reasoning. It is to be hoped that amid the activity which pervades every department of astronomical science, this branch of enquiry will not fail to receive an amount of attention adequate to its growing importance.

We shall now proceed to give a brief account of the researches of astronomers, with a view to the explanation of various interesting phenomena, by means of some of the principles alluded to in this chapter.

CHAPTER XVII.

Eclipses of the Sun and Moon.—Historical Statement of total Eclipses of the Sun.—Annular Eclipses observed in modern times.—Change of Colour which the sky undergoes during an Eclipse.—Its explanation by M. Arago.—Corona of Light observed around the Moon.—Allusions made to it by Ancient Authors.—Explanations of its Physical Cause by different Individuals.—Protuberances on the Moon's Limb.—Their most probable nature.—Observations on the surface of the Moon during Eclipses—Undulations observed on the occasion of the Eclipse of 1842.—Similar Phenomena observed during the Eclipse of 1733.—Explanation of their Origin.—Optical Phenomena observed during Solar Eclipses.—Threads, Beads, &c.—Explanation of their Origin.—Lunar Eclipses.—Transits of Venus.—Physical appearances observed during their Occurrence.—Transits of Mercury.—Spot observed on the Planet's disk.—Its explanation by Professor Powell.—Occultations of the Planets and Stars.

Amid the variety of grand and beautiful phenomena which flow from the operation of nature's laws, exciting alternately the admiration and delight of the attentive observer, there are some whose surpassing magnificence extorts universal homage, and awakens in the spectator a feeling of reverential awe. It is impossible for the most frivolous mind to regard with indifference the irresistible impetuosity of the hurricane as in its wild career it sweeps along every opposing obstacle, or to contemplate without emotion the sublime spectacle of the ocean when its billows are agitated by the fury of the tempest. The everlasting noise of the cataract, the deep roll of the thunder storm, or the sudden apparition of the eccentric comet in the heavens with its pale aspect and "horrid hair," seldom fails to arouse the apathy of the most listless votary of nature. But perhaps on no occasion does the display of stupendous power in the economy of the physical universe exercise so subduing an influence over the mind, or produce so humiliating a conviction of the impotence of all human efforts to control the immutable laws of nature, and arrest the course of events, as when the glorious orb of day, while riding in the heavens with unclouded splendour, begins to melt away from an unseen cause, and soon totally disappears, leaving the whole visible world wrapped in the sable gloom of nocturnal darkness. The scene is rendered still more impressive by the circumstances accompanying so remarkable an occurrence. The heavens assume an unnatural aspect, which excites a feeling of horror in the spectator; a livid hue is diffused over all terrestrial objects; the plants close up their leaves as on the approach of night; the fowls betake themselves to their resting-places; the warbling of the grove is hushed in profound silence. Universal nature seems to have relaxed her energies, as if the pulse which stimulated her mighty movements had all at once stood still.

During the early history of mankind, a total eclipse of the sun was invariably regarded with a feeling of indescribable terror, as an indication of the anger of the offended deity, or the presage of some impending calamity; and various instances are recorded of the extraordinary effects produced by so unusual an event. In a more advanced state of society, when science had begun to diffuse her genial influence over the human mind, these vain apprehensions gave place to juster and more ennobling views of nature; and eclipses generally came to be looked upon as necessary consequences flowing from the uniform operation of fixed laws, and differing from the ordinary phenomena of nature, only in their less frequent occurrence. To the astronomer they have in all ages proved valuable in the highest degree, as tests of great delicacy for ascertaining the accuracy of his calculations relative to the place of the moon, and hence deducing a further improvement of the intricate theory of her movements. In modern times, when the physical constitution of the celestial bodies has attracted the attention of many eminent astronomers, observations of eclipses have disclosed several interesting facts, which have thrown considerable light on some important points of enquiry respecting the sun and moon.

In the present advanced state of astronomy, the records of ancient eclipses have been successfully employed in fixing the dates of contemporary events, and thereby rescuing history, in many instances, from the confusion usually incident to a remote antiquity. It is thus that phenomena, which, at the time of their apparition, were regarded as the mysterious heralds of some impending calamity in the moral world, totally independent of the ordinary course of nature, and which would have been allowed to pass unnoticed, had it not been for the terror which they inspired, have become subservient in illustrating many obscure passages in the writings of the persons who record them, and establishing our knowledge of ancient times upon a more satisfactory basis. This is justly regarded as one of the most remarkable triumphs of modern science.

Before entering upon the history of total eclipses of the sun it may not be out of place to give a brief description of the nature of eclipses in general, and of the various circumstances which determine their occurrence.

appears to describe a great circle in the heavens, inclined at an inconsiderable angle to the ecliptic. Hence it happens that, when the time of her conjunction with the sun coincides with her passage across the ecliptic, she appears projected upon the region of the heavens occupied by the sun, and, being an opaque body, conceals the whole or a portion of his disk from a spectator on the earth. The defect of light arising from this cause is what is termed *a solar eclipse.* It is manifest that the moon cannot wholly intercept the solar light, unless during conjunction her apparent diameter exceed the apparent diameter of the sun. This condition, however, is not always fulfilled, for, as the moon revolves in an orbit of considerable eccentricity, her distance from the earth varies in a sensible degree, and hence arises a corresponding variation in her apparent diameter, causing it sometimes to exceed the apparent diameter of the sun, and other times to fall short of it. Hence, when the centres of the sun and moon are projected upon the same point in the heavens, and the apparent diameter of the moon at the same time exceeds that of the sun, the solar disk will be entirely concealed from the view of the spectator, and *a total eclipse* will be the consequence. If, however, while all other circumstances are alike, the apparent diameter of the moon be less than that of the sun, the solar disk will not wholly cease to be visible, but the opaque body of the moon will appear projected centrally on it, leaving a narrow ring of light exposed to view. In this case, there occurs the interesting phenomenon of *an annular eclipse.* When the moon does not pass centrally over the sun, but merely overlaps a segment of his disk, a portion of his light only is prevented from reaching the place where the spectator is situate, and *a partial eclipse* then ensues.

During a total eclipse of the sun it is manifest that the place on the surface of the earth which happens from this cause to be obscured, is then situated *within* the moon's shadow. When only a portion of the solar light is intercepted by the moon as in the case of an annular or partial eclipse, the place to which the eclipse is visible is situate *beyond* the limits of the lunar shadow in a partially-obscured region termed the *penumbra.*

Since the moon in passing between the sun and the earth intercepts the solar light, either wholly or partially from some place on the surface of the latter body, so when, in the course of her synodic revolution, she arrives in the opposite point of her orbit, the earth being now interposed between her and the sun, she is deprived in her turn of the solar light, and the phenomenon of *a lunar eclipse* is the consequence. It is to be remarked that in all lunar eclipses, the solar rays are supposed to be *wholly* intercepted from the obscured surface, those cases in which a partial deprivation of light only occurs, being left out of consideration, on account of the difficulty of determining the exact instant when the obscuration begins or ends. Hence a lunar eclipse corresponds to a total eclipse of the sun in regard to its specific character, the surface obscured being, in each case, actually immersed in the shadow of the eclipsing body.

If the moon revolved in the plane of the ecliptic, she would necessarily pass between the sun and the earth at every conjunction, while again at every opposition, the earth would be interposed between the sun and moon. Hence a solar eclipse would take place at every new moon, and a lunar eclipse at every full moon. The lunar orbit being, however, inclined at an angle of about 5° to the plane of the ecliptic, an eclipse, whether solar or lunar, can only occur, when the moon during conjunction or opposition is near either of the nodes of her orbit. Astronomers have found by calculation that a solar eclipse cannot take place unless the moon during conjunction be within 17° 21′ 27″ of her node; nor a lunar eclipse unless during opposition her distance from the node be less than 11° 25′ 40″. The question with respect to the specific nature of the eclipse which may happen in either case, whether it be total or partial, will depend, *cæteris paribus*, upon the distance of the moon within the assigned limit.

It follows from the foregoing remarks that, whenever the lunar node returns to the same position with respect to the sun and moon, an eclipse of the same nature will always recur. It is manifest that the frequent occurrence of eclipses in general, will depend on the condition of the lunar nodes with respect to their being fixed or moveable. Now, it appears from observation, that they regress with a rapid motion on the ecliptic, making a complete tour of the heavens in about 18 years. A remarkable relation subsists between the synodic revolution of the moon, and the motion of her nodes, which causes the phenomena of eclipses to return within a definite period nearly in the same order. It appears, in fact, that while 223 lunations include 6585.321 days, the nodes return to the same position with respect to the sun in 6585.772 days. The difference amounts to .451 of a day, or barely 12 hours, during which interval of time the sun describes an arc of 28′ 6″ relative to the lunar node. Hence at the end of 6585.321 days the moon will have returned to the same position with respect to the sun, and will only be at a distance of 28′ 6″ from the same position with respect to the node. This period of 223 lunations or $18^{y}\,10^{d}\,7^{h}\,43^{m}$[*], which occasions a recurrence of eclipses in the same order, was known to the Chaldeans, who arrived at its discovery by comparing together the records of eclipses extending throughout a long succession of ages[†]. It appears from theory that seven eclipses *may* and that two *must* take place in the course of every year. When the number of eclipses is the greatest possible, both eclipses are solar. Out of seventy eclipses which usually take place within a period of 18 years, the average number of solar eclipses is forty-one, and of lunar twenty-nine.

Although solar eclipses, generally speaking, occur very frequently, a total eclipse of the sun is an event which happens only on very rare occasions. This arises from the circumstance of the moon being so small a body compared with the sun, that her shadow frequently does not extend so far as the earth, and even when it does, its dimensions are so inconsiderable that only under conditions of the most favourable nature, can any portion of the earth pass through it. In fact, although an eclipse recurs nearly about the same time within a period of 223 lunations, it will not be exactly of the same magnitude, and the alteration, although small, may suffice to transform it from a total to a partial eclipse.

If a total eclipse of the sun is an event of rare occurrence even anywhere on the surface of the earth, still more especially is this true when the question relates to some particular place. Halley, in a paper on the total eclipse of the sun which happened at London on the 3rd of May, 1715, has remarked that there had not previously occurred a total eclipse of the same body, which was visible in that city, since the 20th of March, 1140 A.D. In connexion with this circumstance it is to be borne in mind that the cycle of 223 lunations has respect to the centre of the earth, and not to any point on its surface. Now, in virtue of the diurnal motion, the position of any point on the earth's surface relative to the sun and moon, is generally different from the position of the centre of the earth relative to the same bodies. Moreover, since the earth is a body of considerable magnitude, while on the other hand the region obscured by the moon's shadow, generally does not exceed 180 miles in diameter, it follows that only a comparatively narrow zone of the earth's surface can be eclipsed during any particular conjunction of the sun and moon. It is clear, therefore, that a total eclipse of the sun may recur regularly at the prescribed time within the cycle, and yet may not be visible at the same place of the earth's surface.

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[*] It will be $18^{y}\,10^{d}\,7^{h}\,43^{m}$, or $18^{y}\,11^{d}\,7^{h}\,43^{m}$, according as four or five leap years happen within the cycle.

[†] In these calculations it is only the *mean* synodic revolution of the moon, and the *mean* motion of her node, that are considered. It is clear, therefore, that even although the two periods alluded to in the text were precisely equal, the inequalities both of the moon's longitude and of her node would cause the corresponding eclipses in each cycle to be of somewhat different magnitudes, and also to recur with more or less irregularity. The cycle is sufficiently true, however, to enable a person by means of it to predict eclipses in a rough way, and this is all that the Chaldean astronomers aspired to accomplish.

Although a solar eclipse may last several hours, counting from the commencement to the end of the obscuration, the interval of time during which the sun is completely hidden behind the dark body of the moon is very limited. The duration is greatest when the moon is in perigee and the sun in apogee; for the apparent diameter of the moon being then the greatest possible, while that of the sun is the least possible, the excess of the former over the latter, upon which the totality of the eclipse depends, has then attained its maximum. Now the perigean diameter of the moon is equal to 33′ 31″, and the apogean diameter of the sun is equal to 31′ 30″. The difference, 2′ 1″, is the arc which the moon in this case describes during the totality of the eclipse. It is manifest, therefore, when the rapidity of the moon's motion, especially in perigee, is taken into account, that even under the most favourable circumstances the sun will not continue totally eclipsed for more than a few minutes.

The duration of a total eclipse of the sun varies, when all other circumstances are the same, with the latitude of the place obscured, being greatest at any place under the equator. Du Séjour found from theory that the utmost possible duration of a total eclipse of the sun is $7^m\ 58^s$ under the equator, and $6^m\ 10^s$ in the latitude of Paris*.

The duration of an annular eclipse is greatest when the moon is in apogee, and the sun is in perigee, for the apparent diameter of the sun is then the greatest, while that of the moon is the least possible, and consequently the excess of the former over the latter, upon which the annular appearance depends, is then a maximum. The greatest apparent diameter of the sun is 32′ 35″, and the least apparent diameter of the moon is 29′ 22″. Hence the difference, 3′ 13″, is the arc described by the moon while the eclipse continues annular. The maximum duration of an annular eclipse exceeds that of a total eclipse for two reasons: first, because the excess of the perigean diameter of the sun over the apogean diameter of the moon (3′ 13″) is greater than the excess of the perigean diameter of the moon over the apogean diameter of the sun (2′ 1″); secondly, because when the moon is in apogee her motion over the sun's disk is much slower than when she is in perigee. Du Séjour found by actual calculation that the utmost possible duration of an annular eclipse under the equator, is $12^m\ 24^s$[*], and under the latitude of Paris is $9^m\ 56^s$[†].

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* Mém. Acad. des Sciences, 1777, p. 318.

The writings of ancient authors contain some interesting allusions to eclipses, both of the sun and moon. The most remarkable is a total eclipse of the sun, which Herodotus relates to have happened while the Medes and Lydians were actually engaged in battle. According to the statement of the Greek historian, "the war between the two nations had continued during five years with alternate advantages to either party. In the sixth there was a nocturnal combat; for after an equal fortune on both sides, and while the two armies were engaged, the day suddenly became night."[‡] He mentions that Thales the Milesian had predicted this phenomenon to the Ionians, and had ascertained the time of the year in which it would happen. He then adds that the Lydians and the Medes, seeing that the night had thus taken the place of day, desisted from the combat, and became desirous, on both sides, of making peace.

This remarkable eclipse is alluded to by several ancient writers who flourished subsequently to Herodotus. As the last-mentioned author has not assigned the time of its occurrence, the ascertainment of this point has given rise to a good deal of discussion. Cicero and Pliny both concur in asserting that it happened in the fourth year of the forty-eighth Olympiad. This would place it in the year 585 A.C. This date has been adopted by Riccioli, Newton, and various other authorities of modern times. On the other hand, Scaliger found by actual calculation that it happened on the 1st of October, 583 A.C.; Usher placed it in the year 601 A.C.; Bayer, Costard, and several others, have maintained that a total eclipse of the sun, which appears from theory to have happened on the 18th of May, 603 A.C., was the one alluded to by Herodotus.

The question with respect to the date of the eclipse, continued in this state of uncertainty until the year 1811, when Baily finally communicated a paper to the Royal Society containing its true solution. By a skilful criticism of the passage in which the Greek historian alludes to the event, he has proved beyond all doubt that it could not have happened earlier than 629 A.C., or later than 595 A.C. Out of seventy eclipses which happened within that period, he found only one which was total in the peninsula of Asia Minor. That eclipse happened on the 30th of September, 610 A.C. It was central and total to part of Asia Minor, Armenia, and Media, and the moon's shadow passed over the very locality where the two armies most probably were engaged[§]. It seems impossible to withhold the conclusion that this is the eclipse mentioned by the Greek historian as having produced so memorable an impression on those who were spectators of it.

Herodotus also mentions a total eclipse of the sun, which happened when Xerxes was advancing with his army from Sardis to Abydos. He states that the sun became invisible, although the heavens everywhere presented a serene and cloudless aspect; and, that in consequence, night took the place of day[‖]. The date of this eclipse has been referred to the year 480 A.C. The question of its actual occurrence is liable, however, to some doubt, arising from a difficulty experienced in reconciling the statement of the historian with calculations founded on the solar and lunar tables.

Thucydides the historian alludes to an eclipse of the sun which happened in the first year of the Peloponnesian war. He mentions that it took place about noon, and that several stars were visible*. The language of the writer is somewhat equivocal with respect to the question whether it was a total or a partial eclipse. Kepler considers it to have been total†. It occurred on the 3rd of August, 431 A.C.

Diodorus Siculus relates that a total eclipse of the sun happened when Agathocles, king of Syracuse, was proceeding with his fleet to Africa. He affirms that the darkness was so great as to bring on the appearance of night, and that stars were visible in all directions. This eclipse is computed to have occurred on the 15th of August, 310 A.C.

Philostratus, in his "Life of Apollonius," mentions that the death of Domitian, the Roman emperor, was previously announced by a phenomenon which appears to have been no other than a total eclipse of the sun. "In the heavens," says he, "there appeared a prodigy of this nature: a certain *corona*, resembling the Iris, surrounded the orb of the sun and obscured his light." It follows from a remark which he afterwards makes in reference to this event, that the darkness was so great as to cause the appearance of night. The *corona* to which Apollonius alludes is a phenomenon that is generally seen around the dark body of the moon during a total eclipse of the sun. A more detailed account respecting it will be given presently. This eclipse has been referred to the year 95 A.D.

Plutarch, in his "Dissertation on the Lunar Spots," alludes to a total eclipse of the sun which had *recently* happened about mid-day. The darkness was so great as to cause the day to resemble night. Stars were everywhere visible. This eclipse must have occurred towards the close of the first century, or early in the beginning of the second. Kepler states that he calculated a great many eclipses which occurred about the year 100 A.D., and that he found no one which agreed better with the words of Plutarch than a total eclipse of the sun which happened in the year 113 A.D. It is not improbable, however, that it is identical with the eclipse mentioned by Philostratus, the date of which is necessarily antecedent to the month of September, 96 A.D.

Total eclipses of the sun are also recorded to have happened in the years 237, 360, 418, 484, 787, 842, 878, 957, 1113.

It has been mentioned that Halley, in a paper communicated to the Royal Society, alludes to a total eclipse of the sun which happened at London on the 20th of March, 1140 A.D. The illustrious astronomer does not state in very explicit terms whether his knowledge of this eclipse is founded upon records of its actual occurrence, or upon a strict calculation of its date by means of the solar and lunar tables. Neither Kepler nor Riccioli makes any allusion to a total eclipse of the sun having occurred in this year. The following passages extracted from the works of contemporary writers will shew, however, that it was not allowed to pass unrecorded.

In the section of the *Saxon Chronicle* which relates to the events of the year 1140, there appears this statement: "In the Lent the sun and the day darkened about the noontide of the day, when men were eating; and they lighted candles to eat by. That was the thirteenth day before the calends of April. Men were very much struck with wonder."‡ The annalist then proceeds to relate the dire consequences which followed this event.

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* *Thucyd.*, lib. ii.

† *Ad Vitellionem Paralipomena*, p. 292

In reference to the same eclipse, William of Malmesbury, states, that "while persons were sitting at their meals, the darkness became so great that they feared the ancient chaos was about to return, and upon going out immediately, they perceived several stars about the sun."[*]

Total eclipses of the sun are recorded to have happened in the years 1187, 1241, and 1415. A remarkable total eclipse of the sun occurred on the 17th of June, 1433. This eclipse was visible in Scotland, and the time of its occurrence was long remembered by the people of that country as the *Black Hour*. Maclaurin states, that there is an account of it in a manuscript, preserved in the library of the University of Edinburgh. It is therein mentioned that the eclipse took place about three o'clock in the afternoon, and that the darkness was so profound that nothing was visible. The latter remark is manifestly an exaggeration. It appears, however, that it was an eclipse of a very unusual kind, for Maclaurin found that at the time of its occurrence the sun was only 2° from his apogee, and the moon not more than 13° from her perigee[†]. This eclipse is not in Riccioli's Catalogue, but he refers to it in another part of his work[‡].

History makes mention of total eclipses of the sun which occurred in the years 1485, 1506, 1530, 1544, 1560, 1567, 1598[§], 1605, 1652[||], 1699[¶].

Down to the beginning of the eighteenth century, the accounts respecting total eclipses of the sun, contain very few remarks which are of advantage in forming the basis of any physical enquiry. The descriptions of similar phenomena which have been observed in more recent times derive considerable value from the interesting details by which they are accompanied.

The first total eclipse of the sun, respecting which the accounts have any pretension to fulness or precision, was one which happened on the 12th of May, 1706. It was observed at Montpellier by MM. Plantade and

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[*] *Historiæ Novellæ*, lib. ii.; *Rerum Anglicarum post Bedem Præcipui Scriptores*, p. 105.

[†] Phil. Trans., 1737, p. 194.

[‡] Almag. Nov., lib. v., cap. ii., Schol.

[§] This eclipse was total in the British Isles. The moon's shadow seems to have passed over the border counties of England and Scotland. The day of the eclipse was long remembered in both countries as *Black Saturday*.

[||] This eclipse was also total in the British Isles. It was observed by Dr. Wyberd at Carrickfergus in the north of Ireland (see Wing's *Astronomia Britannica*, p. 355). The day of its occurrence gave rise to the expression *Mirk Monday* among the people of Scotland, which is even still used in some parts of that country, although the eclipse itself has long ago fallen into oblivion.

[¶] This eclipse happened on the 24th of September, and was observed at various places in the north of Europe. It is mentioned by several of the Swedish observers of the total eclipse of 1733.—(*Acta Lit. et Scien. Sueciæ*, tom. iv., Upsal., 1742.) At Leipsic, where it was very nearly total, a correspondent of the Royal Society states, that "when ten digits were obscured, the sky, being otherwise very clear, began to appear of a more livid or wan complexion, and more sad than it usually looks, with a clear sky when the sun is set, or below the horizon. The cocks also which had hitherto crowed very frequently, as if silenced, going to roost, left off crowing and did not renew it, till by the recovery of the sun's light they had recovered their former gaiety and mirth."—(Phil. Trans., 1700, p. 624.) Louville, in his account of the total eclipse of 1715, to be presently mentioned, has noticed a similar fact. He says, that a little before the sun was totally eclipsed, the cocks of London began to crow as at daybreak; that they were silent during the total obscuration; and that the sun had no sooner reappeared than they commenced again to crow with greater vivacity than before.—(Mém. Acad. des Sciences, 1715, p. 98.)

Capiés[*], at Geneva by Duillier[†], at Nuremburg, by Wurtzelban[‡], and at a variety of other places. At Montpellier the total obscuration lasted $4^m\ 10^s$. During the time that the sun was totally hidden, there appeared a *corona* of light around the dark body of the moon. The planets Venus, Mercury, and Saturn, as well as Aldebaran, and several other fixed stars, were visible to the naked eye. The effects produced upon the animated creation by the sudden transition from day to night were remarkable. "The bats flew about as at dusk. The fowls and pigeons betook themselves in great haste to their resting-places. The little birds which sung in cages were silent and put their heads under their wings. The animals which were at labour stood still." At Geneva the total obscuration lasted 3^m. Duillier states, that the Council which had been engaged in deliberation when the eclipse came on, arose from their seats, because they were unable to read or write. Bats were seen flying about, and swallows looking in amazement for a place of refuge. In several parts of the city there were seen persons prostrate on the ground and offering up prayers, under the impression that the last day was come. On the tops of some of the mountains of Switzerland, where the view of the heavens was not obstructed by the gross vapours that accumulate in the lower regions of the horizon, the stars appeared as thickly strewed as in the time of full moon. The aspect of the heavens could neither be compared to the darkness of night, or the chastened hue of twilight[§]. Wurtzelban states, that it was impossible for any person not to feel appalled at the spectacle[‖].

Halley has given an interesting account of the total eclipse of the sun which happened at London on the 3rd of May, 1715[¶]. The total obscuration lasted $3^m\ 22^s$. The planets Jupiter, Mercury, and Venus, as well as Capella and Aldebaran, were visible to the naked eye. There appeared a luminous ring around the moon as on the occasion of the eclipse of 1706. "I forbear," says Halley, "in his communication to the Royal Society, "to mention the chill and damp which attended the darkness of this eclipse, of which most spectators were sensible and equally judges. Nor shall I trouble you with the concern that appeared in all sorts of animals, birds, beasts, and fishes, upon the extinction of the sun, since ourselves could not behold it without some sense of horror."[**]

Louville, who repaired from Paris to London, for the express purpose of witnessing this eclipse, has also given an account of it which appears in the "Memoirs of the Academy of Sciences," for 1715[††]. In various places of England which were more favourable for observing the phenomenon than London, as many as twenty stars were seen with the naked eye during the totality of the eclipse[‡‡]. The direction of the shadow was towards the north-east. At Upsal, in Sweden, the total obscuration lasted $4^m\ 9^s$[§§].

On the 22nd of May, 1724, a total eclipse of the sun happened, which was visible in France and Germany. It was observed by Maraldi and J.

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[*] Mém. Acad. des Sciences, 1706, p. 113 (Hist.).

[†] Phil. Trans., 1706, p. 2241, et seq.

[‡] Miscellanea Beroliniensis, tom. i., p. 219, et seq.

[§] Mém. Acad. des Sciences, 1706, p. 118 (Hist.).

[‖] Miscellanea Beroliniensis, tom. i., p. 223.

[¶] Phil. Trans., 1715, p. 245, et seq.

[**] Phil. Trans., 1715, p. 261.

[††] Mém. Acad. des Sciences, 1715, p. 89, et seq.

[‡‡] Phil. Trans., 1715, p. 250.

[§§] Ibid., 1715, p. 256.

Cassini, at Trianon[*], and by Delisle, at Paris[†]. At Trianon, the total obscuration lasted $2^m\ 16^s$. Venus, Mercury, and a few of the fixed stars, were visible to the naked eye. The obscurity does not seem to have been so great as on some former occasions. Wagner states, that when it was at its maximum, there was still light enough to enable a person to decipher written characters and to discern objects at a moderate distance off[‡]. On this occasion also a *corona* of light was seen to encompass the dark body of the moon, during the totality of the eclipse.

On the 2nd of May, 1733, there happened a total eclipse of the sun, which was visible in the north of Europe. At Forshem, in Sweden, the total obscuration lasted $3^m\ 8^s$[§]. *Jupiter*, the stars of *Ursa Major*, *Capella*, and several other stars, were visible to the naked eye. It was generally remarked, however, that the darkness was not so great as during the total eclipses of 1699 and 1715[‖]. The luminous ring formed a conspicuous phenomenon during the total obscuration. It was sufficiently bright to be seen distinctly with the naked eye[¶]. Three or four spots of a reddish colour were also perceived near the limb of the moon, but not in immediate contact with it[**]. These interesting phenomena will be alluded to presently in more detail.

On the 9th of February, 1766, a total eclipse of the sun occurred, which was observed in the Southern Ocean by the persons on board the French ship of war the *Comte d'Artois*. The total obscuration lasted only 53^s. There was seen a luminous ring about the moon, which had four remarkable expansions situate at a distance of 90° from each other[††].

On the 24th of June, 1778, there happened a total eclipse of the sun, which was observed at sea by the Spanish Admiral Don Antonio Ulloa, while passing from the Azores to Cape St. Vincent[‡‡]. The total obscuration lasted four minutes. The luminous ring presented a very beautiful appearance. Before it became very conspicuous, the stars of the first and second magnitude were distinctly visible; but when it attained its greatest brilliancy, those of the first magnitude alone could be perceived. The darkness was such, that persons who had been asleep in the afternoon, having awoke, imagined to their great astonishment that the night was already far advanced[§§]. The fowls, birds, and other animals on board, took their usual position for sleeping, as if it had been night.

On the 16th of June, 1806, a total eclipse of the sun occurred, which was visible in North America. At Kinderhook, in the State of New York, it was observed by the Spanish astronomer, Don Joachim Ferrers[‖‖]. The total obscuration lasted $4^m\ 37^s$. One or two of the planets and a few stars of the first magnitude were visible. The moon appeared to be surrounded by a luminous ring. There was a slight fall of dew, while the sun was totally hidden.

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[*] Mém. Acad. des Sciences, 1724, p. 176, 178.

[†] Ibid., 1724, p. 316, et seq.

[‡] Miscellanea Berolinienses, tom. iii., p. 287.

[§] Acta Lit. et Scien., Sueciæ, tom. iv., p. 61, Upsaliæ, 1735.

[‖] Acta, Upsal., tom. iv., pp. 60, 63, 64.

[¶] Acta, Upsal., tom. iv., p. 64.

[**] Phil. Trans., 1733, p. 135; Acta, Upsal., vol. iv., p. 65.

[††] The officers of the *Comte d'Artois* gave a detailed description of this eclipse to Le Gentil, who has briefly alluded to it in the beginning of the second volume of his work. *Voyage dans les Mers de l'Inde.* Paris, 1781.

[‡‡] Phil. Trans., 1779, p. 105, et seq.

[§§] The total obscuration commenced at 44 minutes past three o'clock in the afternoon. (Phil. Trans., 1779, p. 107.)

[‖‖] Amer. Phil. Trans., vol. vi., p. 264, et seq.

On the 30th of November, 1834, there happened a total eclipse of the sun, which was visible in Georgia and South Carolina, U.S. At Milledgeville, Georgia, the phenomenon was observed by the French astronomer, Nicollet*. The total obscuration lasted $1^m\ 15^s$. At Beaufort, South Carolina, two planets and four stars of the first magnitude were visible to the naked eye†.

A total eclipse of the sun depends upon the concurrence of so many circumstances, that an opportunity of observing a phenomenon of this nature seldom occurs to the astronomer, even if the place where it is visible were invariably favourable for that purpose. When it is considered, however, that the track of the lunar shadow is not infrequently confined to a region remote from the great centres of European science, it may easily be conceived that an astronomer may pass his whole lifetime without enjoying the gratification of witnessing so impressive a spectacle. An intense interest was therefore naturally excited by the approaching occurrence of the total eclipse of July 8, 1842, which was announced to be visible in the north of Italy, and in the southern provinces of France, Germany, and Russia. Many of the most eminent astronomers of Europe repaired to different stations upon the track of the lunar shadow, with the intention of observing the phenomenon. M. Arago awaited its occurrence at Perpignan; M. Valz, at Marseilles; M. Petit, at Montpellier; the late Mr. Baily, at Pavia; Mr. Airy, at the Superga, near Turin; M. Carlini, at Milan; MM. Santini, and Conti, at Padua; MM. Schumacher and Littrow, at Vienna; and MM. Otto Struve, and Schidlowsky, at Lipesk. It was witnessed under favourable circumstances at all these stations, as well as at several other places; and detailed statements respecting it were drawn up by the various observers, which have formed the groundwork of much interesting speculation‡. The following account of the occurrence of the eclipse, by M. Arago, cannot fail to repay perusal by the reader.

"At Perpignan, persons who were seriously unwell, alone remained within doors. As soon as day began to break, the population covered the terraces and battlements of the town, as well as all the little eminences in the neighbourhood, in hopes of obtaining a view of the sun as he ascended above the horizon. At the citadel we had under our eyes, besides numerous groups of citizens established on the slopes, a body of soldiers about to be reviewed.

"The hour of the commencement of the eclipse drew nigh. More than twenty thousand persons, with smoked glasses in their hands, were examining the radiant globe projected upon an azure sky. Although armed with our powerful telescopes, we had hardly begun to discern the small notch on the western limb of the sun, when an immense exclamation, formed by the blending together of twenty thousand different voices, announced to us that we had anticipated by only a few seconds, the observation made with the unaided eye by twenty thousand astronomers *equipped for the occasion*, whose first essay this was. A lively curiosity, a spirit of emulation, the desire of not being outdone, had the privilege of giving to the natural vision an unusual power of penetration. During the interval that elapsed, between this moment, and the almost total disappearance of the sun, we remarked nothing worthy of relation, in the countenances of so many spectators. But when the sun, reduced to a very narrow filament, began to throw upon the horizon only a very feeble light, a sort of uneasiness seized upon all; every person felt a desire to communicate his impressions to those around him. Hence arose a deep murmur, resembling that sent forth by the distant ocean after a tempest. The hum of voices increased in intensity as the solar crescent grew more slender; at length the crescent disappeared, darkness suddenly succeeded light, and an absolute silence marked this phase of the eclipse, with as great precision as did the pendulum of our astronomical clock. The phenomenon in its magnificence, had triumphed over the petulance of youth, over the levity which certain persons assume as a sign of superiority, over the noisy indifference of which soldiers usually make profession. A profound stillness also reigned in the air; the birds had ceased to sing.

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* Silliman's Amer. Journal, vol. xxviii., p. 193.

† Silliman's Journal, vol. xlii., p. 175.

‡ Messrs. Baily and Airy have given descriptions of the eclipse, in vol. xiv. of the *Memoirs of the Astronomical Society*. The observations of the French Astronomers are all given by M. Arago in the *Annuaire* for 1845. In vol. iv. of the *Giornale dell' Istituto Lombardo*, there are interesting communications respecting the phenomenon, by MM. Piola, Carlini, Belli, Santini, and Configliachi. M. Schumacher has given an account of the eclipse in No. 457 of the *Astronomische Nachrichten*. An account of the observations of M. Otto Struve and his colleague is inserted, in No. 470 of the last-mentioned Journal. In the *Annuaire* for 1845, M. Arago has collected and arranged all the observations of the eclipse, and has discussed them in the same volume, with the ability which characterizes all the labours of that illustrious philosopher.

"After an interval of solemn expectation, which lasted about two minutes, transports of joy, shouts of enthusiastic applause, saluted with the same accord, the same spontaneous feeling, the first reappearance of the rays of the sun. To a condition of melancholy produced by sentiments of an indefinable nature, there succeeded a lively and intelligible feeling of satisfaction which no one sought to escape from, or moderate the impulses of[*]. To the majority of the public, the phenomenon had arrived at its term. The other phases of the eclipse had few attentive spectators beyond the persons devoted especially to astronomical pursuits."[†]

The interval of time during which the sun was totally hidden behind the dark body of the moon was comparatively brief, even at those places which were most favourably situated for observing the phenomenon. At Perpignan, which was close to the centre of the lunar shadow, the total obscuration lasted only $2^m\ 11^s$. At Pavia, the duration of the totality was $2^m\ 24^s$. At Venice, which was situate near the southern limit of the shadow, the duration was only 43^s. In the regions of eastern Europe, the total obscurity lasted somewhat longer. At Lipesk the duration extended to $3^m\ 5^s$.

The luminous ring usually seen around the moon during a total eclipse of the sun, appeared on this occasion with great splendour. There were also perceived, three rose-coloured protuberances of sensible magnitude in apparent contact with the moon's limb. The darkness which prevailed during the totality of the eclipse was considerable. Sig. Piola states that at Lodi, the planet Mars, two stars of the constellation *Gemini*, Aldebaran,

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[*] In connexion with this remark, the following anecdote, which, according to M. Arago, appeared in the *Journal of the Lower Alps*, of July 9, 1842, cannot fail to prove interesting to the reader. A poor child of the commune of Sièyes was watching her flock when the eclipse commenced. Entirely ignorant of the event which was approaching, she saw with anxiety the sun darken by degrees, for there was no cloud or vapour visible which might account for the phenomenon. When the light disappeared all at once, the poor child, in the height of her terror, began to weep, and call out for help. Her tears were still flowing when the sun sent forth his first ray. Reassured by the aspect, the child crossed her hands, exclaiming, in the *patois* of the province, " *O beou Souleou !*" (O beautiful Sun !)

[†] Annuaire, 1846, p. 303.

Capella, as well as several other stars of the first magnitude, were visible. The citizens of Venice remarked, with respect to a steamboat which was passing on the Lagunes, during the totality of the eclipse, that the column of smoke which usually issues from the funnel was no longer visible. The sparks of flame which the column draws along with it, appeared in consequence to be isolated, and produced a very beautiful effect*.

The appearance presented by surrounding objects during the eclipse was very remarkable. M. Lenthéric has stated that, at Montpellier, a little before the commencement of the totality of the eclipse, the light had acquired a livid and ashy tint, imparting to the human countenance an aspect which was painful to contemplate. MM. Pinaud and Boisgirard remarked that at Narbonne, as the eclipse advanced, the obscurity assumed a character quite peculiar. It had a wan and livid hue, a shade of greyish olive, which seemed to throw over nature a veil of mourning. At Lodi the aspect of the heavens, according to Sig. Piola, was very striking. There were seen two reddish zones extending along the horizon, one in the southern and the other in the northern region of the heavens. They were of a dull copper colour, totally different from the ruddy hue of the aurora or the twilight. The rest of the heavens passed without any degradation to a dark azure inclining to violet. Its light reflected by the waters of the Po and the Lake of Lecca imparted to them an aspect which inspired terror. At Lipesk the heavens appeared of a greyish violet. The stars *Aldebaran* and *α Orionis*, which are usually red, *appeared quite white*. It was remarked by all observers, that the spectacle presented during the totality of the eclipse was of a very appalling nature.

A great number of interesting facts were noticed respecting the effects produced upon animals, by the sudden transition from day to night. M. Arago states, that in many instances, horses and other animals employed in labour, halted all at once when the eclipse became total, lay down, and obstinately refused to move in spite of whip or spur. At Montpellier, according to M. Lenthéric, the bats, thinking that night was come, left their retreats. An owl was seen to leave the tower of St. Peter and fly over a part of the town; the swallows disappeared; the fowls went to roost; a herd of cattle feeding in a field, formed themselves into a circle, their heads directed outwards, as if to resist an attack. At Venice swallows were caught in the streets, the terror with which they were seized, having taken from them the power of flight. On the other hand, M. Arago states it to be a well-ascertained fact, that the horses employed in the diligences continued to pursue their courses, without seeming to be in the slightest degree affected by the phenomenon.

The effect of the obscurity upon those plants which usually close up their leaves during night, was very apparent. At Prades in France, at Milan, and at Vienna, it was found that several plants had closed up during the totality of the eclipse.

With respect to the variation of the thermometer during the eclipse, M. Arago remarks that it was not so considerable as might have been supposed from the impression of cold upon the hands and face. At Perpignan a thermometer, placed in the shade, descended 3° on the centigrade scale. With respect to the thermometers exposed to the direct action of the solar rays, the variation, of course, was much greater. A thermometer with a black bulb, contained in a globe of glass, from which the air was exhausted, being exposed to the sun, was found to have descended 8°.7 between $5^h\ 6^m$ and $5^h\ 48^m$[*]. A thermometer with a bulb of ordinary glass descended 5°.5 between $5^h\ 10^m$ and $5^h\ 48^m$. At the various places of Italy, where observations of this nature were made, the mean descent of the thermometer was 2°.5 Reaumur. At Lipesk, M. Otto Struve found that a thermometer placed in the shade fell 3° Reaumur[†]. At Perpignan, Turin, Vienna, and several other places, a heavy dew fell during the obscuration.

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* Giornale dell' Istituto, del Lombardo, tom. iv., p. 310.

The total eclipse of July 8, 1842, afforded an interesting illustration of the high state of perfection which astronomical science has attained, in the fidelity with which it responded to the calculations that had been previously made respecting the time of its occurrence. The coincidence which subsisted in this respect, combined with the remarkable phenomena which presented themselves during the obscuration, were also well calculated to elevate the mind to a contemplation of the Eternal Being who directs the movements of the celestial bodies with such unerring regularity, and exercises such an all-pervading influence over the innumerable arrangements of creation. Although in European countries a total eclipse of the sun is no longer regarded with feelings of superstitious terror, it would be forming an opinion of human nature equally erroneous and ignoble, to suppose that in the present advanced state of civilisation, a spectacle of such sublimity could be viewed with sceptical indifference. "All the accounts respecting this eclipse," says Sig. Piola, "contain reflections on the perfection of that great machine of the universe, whose movements are so regular that the astronomer is enabled, long beforehand, to predict their effects with unfailing precision; and from contemplating the machine, it was natural to ascend to the Supreme Artificer. While this idea swells in the mind, there is another which, at the same time, shrinks into insignificance—that suggested by contemplating the position of man in the midst of creation. The magnificence of the scale upon which the phenomena of the eclipse, whether atmospheric or celestial, took place, was patent to every spectator. The extensive coloration of an unusual hue, that was visible, the rapid changes which occurred, above all the obscurity which settled over nature like the funereal pall thrown over a dead body, and whose subsequent withdrawal in an instant, operated like a resurrection—all this produced on the mind a mixture of profound and indefinable impressions which it will be pleasing to hold long in remembrance."[‡]

Among the various eclipses of the sun recorded as having happened in ancient times, some were, in all probability, *annular;* but in no instance is the description of the writer sufficiently clear to establish, beyond all doubt, the actual occurrence of an eclipse of this nature. The earliest eclipse, which is unequivocally asserted to have been *annular*, was one which occurred in the year 1567. Clavius, who observed it at Rome, has stated that, when the obscuration was greatest, there still remained around the moon's limb a very narrow ring of the solar light[§]. Kepler, however, found by calculation, that the sun must have been totally covered by the moon on that occasion, and upon this ground he maintained that the luminous ring, observed by Clavius, was no other than the *corona* of light which usually appears around the moon during a total eclipse of the sun*.

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[*] The total obscuration commenced at $5^h\ 46^m\ 51^s$ A.M.

[†] Bibliothèque Universelle de Genève, vol. xliv., p. 369.

[‡] Giornale dell' Istituto del Lombardo, tom. iv., p.

Tycho Brahé, misled by an erroneous determination of the apparent diameters of the sun and moon, came to the conclusion that a total eclipse of the sun was impossible, and that, in fact, all central eclipses of that body were necessarily annular. In accordance with this view of the subject, he contended that the eclipse of 1560, observed at Coimbra by Clavius, as well as the eclipse of 1598, observed at various places in the North of Europe, both of which were reputed to have been total, were in reality annular. It has been ascertained, however, that in each of these instances, the apparent diameter of the moon was less than that of the sun; and, therefore, it follows that during a short interval of time the sun must have been totally concealed from observation.

In 1601 there happened an eclipse of the sun, which appears, beyond all doubt, to have been annular in Norway. Longomontanus asserts, upon the authority of Fossius, Bishop of Bergen, that the fishermen on the neighbouring coast perceived with great admiration the whole body of the moon projected upon the sun, leaving uncovered a uniform ring of the solar disk, about a digit and a half in breadth†. An annular eclipse was also observed by Bouillaud, in the year 1639‡.

The first annular eclipse respecting which we possess a detailed account, was one which happened on February 18, 1737. It was observed in Scotland by the celebrated mathematician, Maclaurin, who communicated to the Royal Society an interesting paper respecting it, which appears in the volume of the *Transactions* of that body for the same year§. The annular eclipses which have since been observed are those of 1748, 1764, 1791, 1820, 1831, 1836, 1838, and 1847.

We now proceed to notice, in detail, the phenomena which generally characterise solar eclipses, and to give a brief account of the various speculations that have been propounded respecting their physical origin.

It has been universally remarked that during the progress of eclipses *the colour of the sky* undergoes a change. Halley, in his account of the total eclipse of 1715, speaks very explicitly upon this point. "When the eclipse," says he, "was about ten digits (that is, when about five-sixths of the solar diameter were immersed), the face and colour of the sky began to change from perfect serene azure blue to a more dusky livid colour, intermixed with a tinge of purple, and grew darker and darker till the total immersion of the sun."‖ The observers of the total eclipse of July 8, 1842, all concur in asserting that the colour of the sky underwent a remarkable change during the progress of the obscuration; but they differ materially in the details which they furnish respecting the phenomenon. M. Arago remarks that these discordances are mainly attributable to physiological causes connected with the organ of vision; and as it is impossible to arrive at an adequate appreciation of the influence of these causes, he considers that any enquiry founded upon the statements of the various observers would be altogether useless, more especially as they are for the most part destitute of precision. He, therefore, undertakes simply to investigate the effect which the progress of the eclipse tends to produce upon the colour of the atmospheric light, in any particular region, without taking into account the modifying influence of extraneous causes, selecting for this purpose the region which is vertical relative to the spectator. He remarks that every particle of the atmosphere, although illuminated mainly by the direct rays of the sun, is also affected in some degree by the light reflected in every direction from the other particles. During the progress of the eclipse, the region of the atmosphere which lies vertically above the spectator, ceases gradually to be illuminated by the direct rays of the sun, while, on the other hand, it is constantly exposed to the same intensity of reflected light from the region which lies in the horizon of particles situate at a great altitude above the place of observation, for the sun still continues to shine upon that region with all his force. The proportion of direct to reflected light continues to diminish as the eclipse increases in magnitude, until at length the reflected light produces a more intense effect than the direct, and thereby determines the visible colour of the sky. Now it is a fact which cannot fail to have come under the observation of every person, that the rays of light proceeding from those regions of the atmosphere that lie near the horizon, invariably differ in hue from those which are transmitted from the more elevated regions. It follows, therefore, as a necessary consequence, that the colour of the sky in the region which is vertical relative to the spectator will undergo a perceptible change*.

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* Ad Vitellionem Paralipomena, p. 299.

† Astronomia Danica, lib. i., cap. 9.

‡ Astronomia Philolaica, lib. ii., p. 210.

§ Phil. Trans., 1737, p. 177, et seq.

‖ Ibid., 1715, p. 247.

This explanation is very ingenious, but it does not give a satisfactory account of the unnatural aspect which the sky exhibits during an eclipse, and which has been universally remarked to be totally different from the appearance of the dawn or twilight. It is to be borne in mind, however, that the circumstances which determine the faint illumination of the atmosphere during an eclipse, although analogous to those upon which the phenomenon of the dawn or twilight depends, are not absolutely identical with them; and hence it is not improbable that the different condition of the light, arising from this cause, may produce the pallid hue which is visible during an eclipse.

It has been found that while the colour of the sky changes very sensibly during eclipses of the sun, a similar effect is also produced upon terrestrial objects. Even as early as the year 840, A.D., it was remarked that during the total eclipse of the sun which happened in that year, the colours of objects on the earth were changed†. Kepler mentions that during the solar eclipse which happened in the autumn of 1590, the reapers in Styria noticed that everything had a yellow tinge‡. It was remarked by MM. Plantade and Clapiés, on the occasion of the total eclipse of 1706, that as the obscuration increased or diminished, objects changed their colour. When two-thirds of the solar diameter were eclipsed, they assumed the colour of *orange yellow;* when there was only about the twenty-fifth part of the diameter visible they assumed *a reddish tinge*, resembling water that has been diluted with wine.

Sir John Clarke, in an account of the annular eclipse of 1737, states, that there was no considerable darkness, but that the ground was covered with a kind of *dark greenish colour*§.

In Le Gentil's brief description of the total eclipse of 1766, it is stated that during the greatest obscuration objects assumed *a tinge of livid yellow*, which produced a very remarkable effect‖.

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* Annuaire, 1846, p. 296.

† Ad Vitellionem Paralipomena, p. 294.

‡ Ad Vitellionem Paralipomena, p. 303.

§ Phil. Trans., 1737, p. 197

‖ Voyage dans les Mers de l'Inde

During the total eclipse of 1842, it was universally remarked that the colours of terrestrial objects were changed.

M. D'Hombres-Firmas perceived that when three-fourths of the sun were eclipsed, objects of a reddish colour, and the human countenance especially, appeared paler, and acquired an olive hue. According to M. Lordat, a few minutes before the commencement of the totality of the eclipse, objects appeared to have a slight tinge of yellow; the light anon became wan and livid; in certain positions the human countenance had a cadaverous aspect*.

Sig. Piola states that the observers of the eclipse in Italy generally remarked that towards the total obscuration objects assumed a greenish tinge, which passed gradually to a saffron hue, or to violet, as some of the observers asserted†. According to Mrs. Airy, the effect produced was like looking at objects through a very dark greenish glass‡.

It is manifest that the aspect of terrestrial objects cannot fail to be affected by the change of colour which the atmospheric light undergoes during the progress of an eclipse. It cannot be doubted, however, that the phenomena above mentioned are, to a considerable extent, attributable to the influence of contrast. Moreover, it is to be remarked that the physiological constitution of the eye tends to produce a modifying effect upon the specific hue of objects. Under these circumstances, it would obviously be premature to make such observations as those above cited, the groundwork for deducing any sound conclusions of a general nature.

The darkness which prevails during a total eclipse of the sun is not so profound as might be expected, nor as it is generally supposed to be. Ferrer, in his account of the total eclipse of 1806, states, that even when the effect produced by the interception of the solar rays was at its maximum, the light which still remained, was equal to that of the full moon§. In general, it has been found that the darkness is sufficiently intense to prevent a person from reading, although there have not been wanting several instances of a contrary nature. Mr. Airy has remarked that the illumination during the total eclipse of 1842 was so small, that he could with difficulty read the divisions on the watch-plate, which was within eight inches of his eye. The faint visibility which continues to subsist even when the sun is totally concealed behind the dark body of the moon, arises mainly from the light reflected by those regions of the atmosphere which are still illuminated by the direct rays of the sun. It is evident, however, that the *corona* around the moon will also contribute in some degree towards producing the effect. The observation of Don Ulloa, relative to the darkness which prevailed during the total eclipse of 1778, is very decisive upon this point. It has been mentioned that at the commencement of the total obscuration he was enabled to perceive the stars of the second magnitude, but that, after the appearance of the luminous ring, only those of the first magnitude were visible.

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* Annuaire, 1846, p. 291.

† Giornale dell' Istituto del Lomb., tome iv., p. 304.

‡ Mem. Ast. Soc., vol. xv., p. 17. In thus using the liberty of divesting the observation cited by Mr. Airy, of the anonymous character in which he has presented it in his paper on the eclipse, the author pleads the example of M. Arago, upon whose authority alone he has been induced to adopt a course which is at all times desirable, but more especially so when the subject is of an historical nature, as it happens to be in the present instance.

§ Trans. Amer. Phil. Soc., vol. vi., p. 266.

The darkness which prevails during the total obscuration of the sun, does not appear to be equally profound, when the observations of different eclipses are compared together; nor even at different places where the same eclipse has been observed. This arises partly from the variable condition of the atmosphere, and partly from the circumstance that during an eclipse all places are not equally immersed in the moon's shadow. It is manifest that all those places which are situate close to the boundary of the lunar shadow, are exposed in a greater degree to the light reflected from the regions of the atmosphere upon which the sun is still shining, than are the places contiguous to the centre of the shadow. It was upon this principle that Halley explained the fact that at London the obscurity during the total eclipse of 1715, was less intense than that which prevailed in various other parts of England that were more deeply immersed in the lunar shadow*.

A fact was noticed during the total eclipse of 1842, which deserves to be mentioned. Sig. Piola states, that at Lodi, the darkness during the totality of the eclipse was equal to that by which the stars of the second magnitude are usually discerned, and yet only those of the first magnitude were visible†. A similar remark was made by M. Otto Struve, with respect to the same eclipse. He states that at Lipesk the darkness surpassed in a small degree that which reigns at St. Petersburg during the summer solstice; but that, while in the latter case the stars of the third magnitude are usually discernible without any difficulty, in the former case those of the first alone could be perceived‡. Sig. Belli explains this curious fact by reference to a physiological principle. He remarks that during the short interval of total obscuration, the eye has not sufficient time to recover from the dazzling effect of the sun's rays, and consequently is unable to take due advantage of the obscurity which actually prevails§. This is, doubtless, the true explanation of the anomaly observed on such occasions.

The suddenness with which day succeeds night, upon the reappearance of the sun after undergoing a total eclipse, has been remarked by all persons who have witnessed a phenomenon of this nature. The first ray of the sun darts forth from behind the moon's limb with a velocity that has been compared to the swiftness of an arrow, a flash of lightning, or some such emblem of extraordinary speed‖. According to Sig. Piola, so rapid was the effect produced by the reappearance of the sun on the occasion of the total eclipse of 1842, that it might be said to have been night, and in an instant it was day¶. The same observer states, that it was universally remarked by the spectators of the eclipse at Lodi, that the light emitted by the sun previous to the total obscuration, produced a much less dazzling effect than that which distinguished his emersion from behind the dark body of the moon. In the former case, the spectator was enabled to look upon the sun for several minutes previous to the total obscuration, without experiencing any inconvenience; in the latter case, it was impossible for the naked eye to withstand the violent impact of the first rays of the sun. This fact had been already noticed by Halley, on the occasion of the total eclipse of 1715. The English astronomer had suggested two distinct causes, to whose combined operation it might be ascribed. One of these was of a physiological nature; the other implied the existence of a lunar atmosphere. He remarked, in the first place, that previous to the total obscuration, the pupil of the eye might be very much contracted by viewing the sun, and, consequently, the organ of vision would be less liable to suffer from the effulgence of the light than at the instant of emersion, when the pupil had again expanded. Secondly, he suggested that, as the eastern margin of the moon, at which the sun disappeared, had been exposed for a fortnight to the direct action of the solar rays, the heat generated during this period might cause vapours to ascend in the lunar atmosphere, which, by their interposition between the sun and the earth, would have the effect of tempering the effulgence of the solar rays passing through them. On the other hand, the western margin of the moon, at which the sun reappeared, had just experienced a night of equal length, during which the vapours suspended in the lunar atmosphere, had been undergoing a course of precipitation upon the moon's surface under a process of cooling. In this case, therefore, the solar rays would meet with less obstruction in passing through the lunar atmosphere, and, consequently, it was reasonable to suppose that they would produce a more intense effect*.

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* Phil. Trans., 1715, p. 250. Sig. Piola has stated that persons stationed upon the hills around Brescia, during the total eclipse of 1842, enjoyed the beautiful prospect of the peaks of Rosa and Cimone brilliantly illuminated by the sun's rays, while they themselves were involved in the obscurity of the moon's shadow. (*Gior. dell' Ist. Lomb.*, p. 310.)

† Giorn. dell' Ist. Lomb., tome iv., p. 341.

‡ Bibliothèque Universelle de Genève, tome xliv., p. 368.

§ Giorn. dell' Ist. Lomb., tome iv., p. 341.

‖ "Instar fulgoris"—"instar sagittarum radii solis repente prorumpentes"—"radius solis instar sagittæ prevolavit"—"radius admodum illustris prorupit," &c., &c. Such are the terms employed by the Lutheran pastors of Sweden, in describing the reappearance of the first rays of the sun on the occasion of the total eclipse of 1733.

¶ "Talchè si potè dire che in momento era notte e fu giorno" (*Giornale dell J. R. Istituto del Lombardo,* tome iv., p. 314). Attempts have been made to detect the motion of the moon's shadow in the course of its passage over the surface of the earth. To effect such an object, however, is manifestly a very difficult matter

It is clear that both causes above mentioned might conspire together in producing the observed effect. As, however, there are no reasons for supposing that the moon possesses an atmosphere capable of exercising any appreciable influence, it is probable that the true explanation is to be sought in the different dimensions of the pupil of the eye, at the commencement and the end of the total obscuration.

The *luminous ring* that appears around the moon is one of the most interesting features of a total eclipse of the sun. The earliest allusion to it is probably to be found in the passage of the *Life of Apollonius*, already cited, wherein the author mentions that the death of the emperor Domitian had been previously announced by a total eclipse of the sun. "In the heavens," says Philostratus, "there appeared a prodigy of this

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immense velocity with which the shadow sweeps over any particular place of observation. Halley calculated that the shadow of the eclipse of 1715 passed over England at the rate of fifty-nine geographical miles in a minute (*Phil. Trans.*, 1715, p. 260). Mr. Airy has mentioned in his account of the total eclipse of 1842, that he endeavoured to detect the progress of the lunar shadow as it passed over the immense plain of Lombardy, but that his efforts were unsuccessful. He adds, however, that Messrs. Plana and Forbes felt assured that they saw the darkness travel over the country. The only remark of a similar nature which the author has met with in the accounts of former eclipses is contained in the following extract from Duillier's paper on the total eclipse of 1706:—"A little before the total obscuration, the country on the west side did already seem overcast with darkness; and after the total obscuration, *the darkness was seen to leave us more and more, and to fly eastward*" (*Phil. Trans.*, 1706, p. 2243).

* Phil. Trans., 1715, p. 248.

nature. A certain *corona*, resembling the *Iris*, surrounded the orb of the sun, and obscured his light."

Plutarch, also, about the same time, alludes in one of his works to the phenomenon of the luminous ring. Speaking of a total eclipse of the sun which had *recently* happened, he endeavours to show why the darkness arising from occurrences of this nature is not so profound as that which usually prevails when the sun is below the horizon. He begins by assuming, as the basis of his reasoning, that the earth greatly exceeds the moon in dimensions. After citing various authorities in support of this assertion, he then proceeds thus:—"Whence it happens that the earth, on account of its magnitude, entirely conceals the sun from our sight. . . . . . . But even although the moon should at any time *hide the whole of the sun*, still the eclipse is deficient in duration as well as amplitude, for a peculiar effulgence is seen around the circumference, which does not allow a deep and very intense shadow."* It cannot admit of any doubt that the phenomenon alluded to in the above passage is the luminous ring that has been invariably observed around the moon in modern times on the occasion of a total eclipse of the sun†.

It would seem that Clavius observed the luminous ring at Rome during the eclipse which happened on April 9, 1567, although he was not conscious of its real nature. It has been already mentioned, that when the obscuration was greatest, he perceived a narrow ring of light around the moon, which he supposed to be the margin of the solar disk. Kepler, however, maintained that the luminous circle seen by Clavius, could not really be a portion of the sun. He found, in fact, by calculation, that during the eclipse, the moon was at her mean distance from the earth, in which position, he remarked, her apparent diameter exceeds that of the sun even when he is in perigee; while at the same time the sun was approaching towards apogee, where his apparent diameter is the least possible. He therefore came to the conclusion that the sun must have been totally covered by the moon during the eclipse, and, consequently, that the appearance observed by Clavius could not have been produced by the direct transmission of the solar rays. The explanation which that illustrious astronomer has given of the physical cause of the phenomenon will be noticed presently.

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* It will be seen that the passage in the text differs materially from that cited by M. Arago, in the *Comptes Rendus*, tome xiv., p. 848, which is to the following effect:—"La Lune laisse déborder autour d'elle, dans les éclipses, une partie du Soleil, *ce qui diminue l'obscurité*." It is merely on the strength of the last part of this sentence, and in direct contradiction to the first part, that M. Arago ventures to suggest that the phenomenon of the luminous ring is probably that to which Plutarch alludes. It is manifest, however, that the sentence may be supposed, with greater plausibility, to apply to *annular* eclipses. With respect to the passage cited in the text, there cannot exist a shadow of a doubt that it refers to the luminous ring that is visible around the moon during a *total* eclipse of the sun. The following are the express terms in which Plutarch alludes to the phenomenon:—ἡ δὲ σελήνη κἂν ὅλον ποτὲ κρύψῃ τὸν ἥλιον, οὐκ ἔχει χρόνον, οὐδὲ πλάτος, ἡ ἔκλειψις, ἀλλὰ περιφαίνεταί τις αὐγὴ περὶ τὴν ἴτυν, οὐκ ἐῶσα βαθεῖαν γίνεσθαι τὴν σκιὰν καὶ ἄκρατον. (Plut., *Opera Mor. et Phil.*, vol. ix., p. 682, *Edit. Lips.*, 1778). M. Arago does not mention the part of Plutarch's works in which the original of the translation given by him is to be found. It is not improbable, therefore, that he may refer to some less unequivocal passage of the Greek author than that above cited.

† This eclipse is sometimes referred to the year 98, A.D.; Kepler, however, is disposed to believe that it happened in the year 113, A.D. (*Tabul. Rudolp. Præcept.*, p. 104).

The luminous ring was visible around the moon during the total eclipse of 1598. Jessenius, who observed the eclipse at Torgau in Germany, remarked that during the greatest obscuration there appeared a bright light shining around the moon*. On this occasion, also, the phenomenon was generally supposed to arise from a defect in the totality of the eclipse, although Kepler strenuously contended that such an explanation was at variance with the relation between the values of the apparent diameters of the sun and moon, as computed for the time of the eclipse by the aid of the solar and lunar tables. He considered the phenomenon to be identical in its nature with that observed during the eclipse of 1567, and to be a usual accompaniment of total eclipses of the sun.

The views of Kepler respecting the eclipses of 1567 and 1598 first appeared in his "Supplement to Vitellion," which was published in the year 1604. They received a striking confirmation from the observations of the total eclipse of the sun which happened in the following year. On this occasion the eclipse was observed at Naples, under circumstances which did not admit of any doubt respecting the existence of the ring. "The whole body of the sun was effectually covered for a short time. The surface of the moon appeared quite black; but around it there shone a brilliant light of a reddish hue, and uniform breadth, which occupied a considerable part of the heavens."†

During the total eclipse of the sun which happened on the 29th of March, 1652, and which was visible in the British Isles, the moon was seen surrounded by a ring of light. Dr. Wyberd, who observed this eclipse at Carrickfergus in the north of Ireland, has stated that when the sun was reduced to a very slender crescent of light, *the moon all at once threw herself within the margin of the solar disk with such agility, that she seemed to revolve like an upper millstone, affording a pleasant spectacle of rotatory motion!* He remarks, however, that in reality the sun was totally eclipsed, and that the appearance was due to a *corona* of light around the moon, arising from some unknown cause. He adds, that it had a uniform breadth of half a digit, or a third of a digit at least, that it emitted a bright and *radiating* light, and that it appeared concentric with the sun and moon when the two bodies were in conjunction ‡.

The luminous ring formed a conspicuous accompaniment of the total eclipse of the sun which happened on the 12th of May, 1706. The description of it given by MM. Plantade and Capiés, who observed the eclipse at Montpellier, is clearer and more precise than any other that had been hitherto recorded. As soon as the sun was totally eclipsed, there appeared around the moon a very white light forming a kind of *corona*, the breadth of which was equal to about 3′. Within these limits the light was everywhere equally vivid, but beyond the exterior contour, it was less intense, and was seen to fade off gradually into the surrounding darkness, forming an annulus around the moon of about 8° in diameter §.

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* Ad Vitellionem Paralipomena, p. 299.

† Kepler, *De Stella Nova*, p. 116, 4to., Prag., 1606.

‡ Wing, *Astronomia Britannica*, p. 356.

§ Mém. Acad. des Sciences, 1706, p. 251.

Halley's account of the total eclipse of the sun which happened at London in 1715, corroborates the testimony of preceding observers with respect to the existence of the luminous ring, and also contains some interesting particulars relative to its physical aspect. "A few seconds," says he, "before the sun was all hid, there discovered itself round the moon a luminous ring about a digit, or perhaps a tenth part, of the moon's diameter in breadth. It was of a pale whiteness or rather pearl colour, seeming to me a little tinged with the colours of the *Iris*, and to be concentric with the moon." He remarked, moreover, that the ring appeared much whiter and more brilliant near the body of the moon than at a distance from it, and that the exterior boundary was very ill defined, seeming to be determined only by the extreme rarity of the luminous matter[*].

The account of the appearance of the ring on this occasion, given by the French astronomer Louville, tends to confirm the truth of Halley's remarks. He has stated, however, that there were interruptions in its brightness, causing it to resemble the radial *glory* with which painters encircle the heads of the saints. He asserted with confidence that it appeared to be concentric with the moon[†].

The luminous ring formed a conspicuous feature of the total eclipse of the sun which occurred on the 22nd of May, 1724. Maraldi has stated, that at the commencement of the total immersion, the ring appeared broader on the east than on the west side; but, on the other hand, that at the end of the total immersion, it appeared broader on the west side. He remarked also that the breadth of the ring where it bordered upon the northern limb of the moon, was greater than the breadth at the part bordering upon the southern limb[‡].

The luminous ring appeared with great splendour on the occasion of the total eclipse of 1733. A great many interesting observations relative to it, due chiefly to Lutheran pastors, are to be found in the fourth volume of the *Transactions* of the Royal Society of Sweden[§]. The following are a few details respecting it, which have been extracted from the paper by Celsius embodying them.

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[*] Phil. Trans., 1715, p. 249.

[†] Mém. Acad. des Sciences, 1715, p. 90.

[‡] Ibid., 1724, p. 178.

[§] Nothing can be more praiseworthy than the zeal with which the clergymen in the rural districts of Sweden responded to the invitation of the Royal Society of that country to note the various phenomena connected with the occurrence of the total eclipse of 1733. The observations transmitted by them to the Society on this occasion, were all arranged methodically by Celsius, previous to their insertion in the volume referred to in the text. Although they cannot pretend to much precision, still it may be asserted without hesitation, that they form the most complete description of a total eclipse of the sun which is to be found in the records of astronomical observation previous to the total eclipse of 1842. This is more especially remarkable, as only a few years previously, other two total eclipses of the sun had happened (in fact, during the intermediate period the moon's node in the one case had made only half a revolution, and in the other not more than a whole revolution), by means of which the inhabitants of the two countries of western Europe, which boast of their superior civilization, had an opportunity of making similar observations. With respect to the total eclipse of 1715, the account given by Halley is the only one due to an English observer which contains a single remark of a physical nature. The other descriptions, relating to the duration and magnitude of the eclipse, merely served the purpose of enabling Halley to determine the precise track of the lunar shadow as it passed over England. Something might have been expected from Cotes, who observed the eclipse at Cambridge; but, according to Halley, he had the misfortune *to be oppressed with too much company*. The accounts given by the French observers of the total eclipse of 1724, are still more meagre and unsatisfactory; since in this case there is not even one description which might compensate, in some degree by its merits, for the barrenness of the others.

The pastor of Stona Malm states, that at Catherinesholm, during the total obscuration, there was seen a ring around the sun about a digit in breadth, *from which there issued rays of light* *. According to the pastor of Forshem, the ring appeared of a reddish colour, similar to that which is perceived in the *Iris.* At the commencement of the total obscuration it appeared broadest towards the west; in the middle it presented a uniform aspect; and at the close it was broadest towards the north †. Vallerius, another pastor, states, that the ring was more ruddy and compact close to the sun, and that at a distance from that body it appeared of a greenish colour. The pastor of Smoland affirms, that during the total obscuration the limb of the moon resembled gilded brass, and that the faint ring around it emitted rays in an upward as well as in a downward direction, *similar to those seen beneath the sun when a shower of rain is impending* ‡. M. Edstrom, Mathematical Lecturer in the Academy of Charlestadt, asserts, that the ring appeared everywhere of equal breadth, that it emitted rays from above as well as from below, that these rays were equal in brilliancy, but of unequal length, and that *they plainly maintained the same position until they vanished along with the ring upon the reappearance of the sun's limb* §. At Lincopia the ring appeared of a bright white colour, but *it did not exhibit a radial* aspect ‖. From the descriptions given by several observers, it would seem that at the commencement of the total obscuration, the ring appeared brighter and broader at the part of the moon's limb where the sun had disappeared, but that towards the close of the obscuration it was more conspicuous in both these respects at the part where the sun was about to emerge.

It has been already mentioned, that a luminous ring was seen around the moon during the total eclipse of the sun which happened on the 9th of February, 1766. The most remarkable feature exhibited by it consisted of four luminous expansions, separated from each other by equal intervals of 90°. Two of them were situate in the plane of the ecliptic; the other two were at opposite extremities of a diameter of the ring perpendicular to that plane. A copy of a drawing of the ring by the officers of the French ship of war, the *Comte d'Artois*, appears at the beginning of the second volume of Le Gentil's *Voyage dans les Mers de l'Inde.*

A very interesting account of the luminous ring as it appeared during the total eclipse of 1778, is given by the Spanish Admiral Don Antonio Ulloa. He states, that five or six seconds *after* the commencement of the total obscuration, a brilliant luminous circle was seen surrounding the moon, which became more vivid as the centre of that body continued to approach the centre of the sun. About the middle of the eclipse, its breadth was equal to one-sixth of the moon's diameter. There appeared issuing from it, a great number of rays of unequal length, which could be discerned to a distance equal to the lunar diameter. It seemed to be endued with a rapid rotatory motion, which caused it to resemble a firework turning round its centre. The colour of the light was not uniform throughout the whole breadth of the ring. Towards the margin of the lunar disk, it appeared of a reddish hue; then it changed to a pale yellow, and from the middle to the outer border, the yellow gradually became fainter, until at length it seemed almost quite white *.

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* Acta Lit. et Scien. Succ. Upsal., tom. iv., p. 56.

† Ibid., p. 61.

‡ Ibid., p. 62.

§ Ibid., p. 57.

‖ Ibid., p. 59.

Ferrer, in his account of the total eclipse of 1806, has given a brief description of the luminous ring. He states, that the colour of the light emitted by it resembled pearl colour, and that it had a breadth of about 6′. From the exterior margin there were seen luminous rays extending outwards to a distance of more than 3°. The light was brightest at the edge of the moon, and terminated very confusedly at the outer border. With respect to its relative position, the ring seemed to be concentric with the sun †. De Witt, who observed the same eclipse at Albany in the State of New York, states, that "the luminous circle on the edge of the moon, as well as the rays which were darted from her, were remarkably pale, and had that bluish tint which distinguishes the colour of quicksilver from a dead white." ‡

The luminous ring appeared with great splendour during the total eclipse of July 8, 1842. It was of uniform brightness in those parts that bordered on the moon's limb, whence it faded imperceptibly outwards, terminating so confusedly, that it was impossible to trace its exterior limit. The difference between the inner and outer parts of the ring appeared to M. Arago to be sufficiently marked to sanction the subdivision of the ring into two concentric zones, the inner zone being everywhere of equal density and well defined at the outer border, while on the other hand the exterior zone, although the broader of the two, was fainter even at the inner border, and gradually diminished in brightness until it was lost in the surrounding darkness. The interior zone was a conspicuous object at all the stations where the eclipse was observed, and everywhere presented the same aspect. The exterior zone, being much fainter, did not exhibit the same degree of magnitude at the different places of observation. Indeed it was only under favourable conditions of the atmosphere that it was visible at all.

With respect to the dimensions of the *corona*, it could not be expected that the observations would present a very close agreement, owing to the indefinite nature of its structure. At Perpignan, M. Silva found by means of a repeating circle, that the interior zone had an invariable breadth of 3′ during the whole time of complete obscuration. Mr. Airy estimated its breadth at an eighth part of the lunar diameter, or about 4′. From the statements of the various observers, it would appear that this part of the luminous ring, or in other words the part exhibiting a uniform condensation, extended from the moon's limb to a distance of between 3′ and 4′.

The exterior zone of the *corona* being more or less perceptible according to the condition of the atmosphere, there naturally arose considerable discordances in the observations relative to its breadth at the different stations. At Montpellier, M. Petit obtained 8′ 45″ for the distance to which the corona was visible. This would indicate the breadth of the exterior part to be about 5′. Mr. Baily estimated the breadth of the whole *corona* at half the moon's diameter, or about 16′. At Lipesk, where it appeared with great splendour, M. Otto Struve found that it was visible to a distance of 25′ from the moon's limb.

At several stations the regularity of the contour of the ring appeared to be interrupted by two or more expansions of light. In France there were generally two such expansions visible. They were situate at opposite extremities of a diameter, passing through the point at which the sun went in behind the moon's limb, and the opposite point at which he reappeared. At Milan, Sig. Picozzi observed two jets of light occupying a similar position. It was, doubtless, from the same cause that the ring appeared slightly eccentric at Novara, for it was remarked that its greater axis coincided with the direction of the moon's motion[*]. A similar elongation of the ring was also observed at Padua, by Sig. Biela, and several other persons[†]. Sig. Pietropoli, who observed the eclipse at that city, remarked that the ring sensibly bulged out at the opposite extremities of a diameter inclined to the horizon at an angle of about 10°[‡]. M. Otto Struve perceived several luminous jets, which in some instances extended as far as 4° from the moon's limb[§].

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* Phil. Trans., 1779, p. 108.

† Trans. Amer. Phil. Soc., vol. vi., p. 266.

‡ Ibid., p. 301.

From the ring there were generally seen to issue diverging rays of unequal length. At Perpignan, M. Mauvais found that some of these rays extended as far as 33′ from the moon's limb. Mr. Baily, in his account of the eclipse, remarks, that at Pavia the diverging rays had the effect of depriving the *corona* of the appearance of a ring[‖]. On the other, Mr. Airy states, with respect to the aspect of the *corona*, when viewed from the Superga, that, although a slight radiation might have been perceptible, it was not sufficiently intense to affect in a sensible degree the annular structure by which the luminous appearance was plainly distinguished[¶]. These discordances are doubtless mainly attributable to the different conditions of the atmosphere at the various places of observation.

Phenomena of a still more anomalous nature were remarked by several observers in France. At Perpignan, M. Arago, distinctly perceived with the naked eye, a little to the left of the diameter, passing through the highest point of the moon's limb, a luminous spot composed of jets, entwined in each other. He states, that "in appearance, they resembled a hank of thread in disorder." A similar phenomenon was observed at Montpellier. It was remarked, also, at some stations, that the direction of the diverging rays was not in all instances perpendicular to the moon's limb. Some of these rays when prolonged towards the moon, instead of passing through the centre of that body, cut off only a small segment of her disk.

The prevailing colour of the ring at the different stations was white. At Perpignan it assumed a yellowish hue in the telescope, but it appeared white to the naked eye. According to Mr. Airy, it had a close resemblance to peach colour. Mr. Baily and M. Otto Struve both found it quite white.

The different degrees of brilliancy which the ring exhibited at the various places of observation is worthy of remark. At Perpignan, its lustre resembled that of the moon. The aspect which it presented to Mr. Airy, at the Superga, was somewhat similar. On the other hand, at Pavia it appeared with a splendour which excited the admiration of every spectator. "I had imagined," says Mr. Baily, "that the *corona*, as to its brilliant or luminous appearance, would not be greater than that faint crepuscular light which sometimes takes place on a summer's evening, and that it would encircle the moon like *a ring*. I was, therefore, somewhat surprised and astonished at the splendid scene which now so suddenly burst upon my view."* At Lipesk, the brilliancy of the ring was still more vivid. According to M. Otto Struve, it was so intense as to be barely supportable to the naked eye. So strong was the impression which it produced upon the spectators of the eclipse, that many of them could with difficulty be persuaded that the whole of the solar disk was actually concealed behind the body of the moon †.

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[*] Giorn. dell' Ist. del Lomb., tom iv., p. 307. Sig. Configliachi also affirms, that at Monguzzo two expansions of light were visible at opposite extremities of a diameter of the ring. (*See the volume above cited*, p. 368.)

[†] Giorn. dell' Ist. del Lomb., tom. iv., p. 381.

[‡] Ibid., p. 383.

[§] Annuaire, 1846, p. 329.

[‖] Mem. Ast. Soc., vol. xv., p. 5.

[¶] Ibid., p. 15.

These extraordinary variations in the brightness of the ring are doubtless attributable to the more or less favourable condition of the atmosphere at the different places of observation. In connexion with this explanation, M. Arago has suggested, with great probability, that the intense brilliancy which the ring exhibited at Lipesk, may have arisen from the comparatively high altitude of the sun during the totality of the eclipse. As an illustration of this remark it may be stated that at Perpignan, where the ring resembled the moon in lustre, the altitude of the sun at the time of his total immersion was 11° 59′; whereas at Lipesk the altitude of the same body was 41° 19′, when the totality of the obscuration took place. Now it is very manifest that the atmospheric medium of air through which the eclipse was observed, was much less favourable for discerning the brightness of the ring in the former case, than it was in the latter ‡.

At Montpellier, there were many persons who asserted that the ring turned continually round its centre. We have seen that a similar remark had been already made with respect to the *corona* which appeared during the total eclipses of 1652 and 1778. At Lipesk, the light of the ring seemed to M. Otto Struve to be in a state of violent agitation. Mr. Baily states, that the rays had a flickering appearance, somewhat like that which a gas illumination might be supposed to assume, if formed into a similar shape.

The ring generally became visible a few seconds previous to the total immersion of the sun, and it continued to be perceived during an equal interval of time subsequent to his reappearance. This circumstance suggested to M. Arago an interesting method of determining the intensity of the light of the ring relative to the light diffused throughout the atmosphere by the full sun. It is manifest, that at the instant when the ring first becomes visible, its light must exceed, in intensity, the atmospheric light which appears around it. In order that an object may become barely visible, it is necessary that its brightness should exceed that of the ground upon which it appears projected by a certain determinate quantity §. In this manner, then, the light of the ring becomes directly comparable in intensity with the light diffused around it. Now, the atmospheric light around the ring varies in the direct ratio of the solar segment, which remains uncovered by the moon. Hence, knowing the magnitude of the visible segment of the sun, it is easy to compare the light diffused by it through the atmosphere with the light diffused by the full sun, and when this point has been ascertained, the relative intensities of the light of the ring and the light diffused by the full sun become comparable also. Now, the magnitude of the solar segment may obviously be deduced from

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* Mém. Ast. Soc., vol. xv., p. 4.

† Annuaire, 1846, p. 336.

‡ Ibid., p. 336.

§ At p. 381 of the *Annuaire* for 1846, M. Arago asserts, as the result of experiment, that in such a case the brightness of the object must exceed that of the ground upon which it is projected by a sixtieth part.

the interval of time which elapses between the instant of the ring becoming visible, and that of total obscuration, for the time which the moon takes to cover the segment will at once afford an indication of its breadth.

It appears from the foregoing remarks, that the comparison of the light of the ring with the light diffused in the atmosphere by the full sun, depends upon the determination of the precise time which elapses between the commencement of the visibility of the ring, and the instant of total obscuration. On this point, however, the observations do not agree sufficiently well with each other, to be of much utility to enquirers in resolving so delicate a question. At Montpellier, M. Petit perceived the ring, five or six seconds before the sun had totally disappeared behind the dark body of the moon. At Salon it was seen by M. Largetau, four or five seconds previous to the total immersion. Mr. Baily has remarked, that only three or four seconds were wanting to complete the total immersion of the sun when he first saw the ring.

The luminous ring was seen at some places that were situate actually beyond the limits of the lunar shadow, and where consequently a segment of the solar disk continued visible, even during the time of greatest obscuration. In such cases it is manifest that the magnitude of the solar segment visible, upon which the relative intensity of the light of the ring, as determinable by the foregoing method, depends, may be obtained by calculating the maximum phase of the eclipse at the place under consideration. M. Arago cites an interesting observation of this nature, made by M. D'Hombre Firmas, at Alais, which was contiguous to the lunar shadow, but not actually involved in it. " Every one," says the last-mentioned individual, " remarked the circle of pale light which encompassed the moon when she almost entirely covered the sun*. It would appear that this is not the only instance in which the ring has been seen during a partial eclipse of the sun †.

The ring at first was only partially visible, being incomplete on the side of the solar disk which was still uncovered by the moon. As soon, however, as the total obscuration was effected, the ring appeared entire. " A bright line," says Mrs. Airy, " seemed to form round the right side of the moon before the disappearance, but not quite round, so that the ring was not complete; but at the moment of the total disappearance, the ends seemed suddenly to join and form the complete ring."‡ At Milan, Sig. Majocchi, and his companions, perceived a fragment of the ring a few seconds before the total obscuration, situate in the region where the first contact of the two bodies took place; and they continued to discern a faint trace of it in the opposite region, a short time after the reappearance of the sun §. This tallies exactly with the observation just cited.

It was remarked that the ring first appeared brightest on the side of the solar disk, which was just covered by the moon, but that previous to the close of the total obscuration, it was brightest at the part where the sun was about to reappear. This interesting fact is alluded to by several of the observers of the eclipse. There was no defined edge to the ring; it changed sensibly, being brightest first on the left side where the sun had gone in, then below, and then on the right side; the light coming out at each place successively, like little beams from the moon's edge. Such are the terms in which the variation of the appearance of the ring as viewed from the Superga, is described[*]. Sig. Piola states, that at St. Angelo, near Lodi, the same remark was made by every person who witnessed the eclipse[†]. It has been already mentioned that a similar fact was noticed, with respect to the appearance of the ring, during the total eclipses of 1724 and 1733.

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* Annuaire, 1846, p. 339.

† Thus in an account of the solar eclipse of November 27, 1722, communicated by an observer in America to the Royal Society, it is stated that at Barnstable, on Cape Cod, there was but a little left of the sun, and that nearer the head of the Cape, *there was a ring of light quite round the moon* (Phil. Trans. 1724, p. 69).

The question relative to *the physical cause* of the luminous ring has given rise to much speculation. Some persons have supposed it to derive its origin from the moon, while others again have maintained that it is purely a solar phenomenon. A brief historical statement of the different hypotheses that have been advanced in connexion with this subject may not, perhaps, prove uninteresting to the reader.

Various explanations of the luminous ring have been advanced in accordance with the supposition of its being an appendage of the moon. The earliest is that which ascribes it to the influence of a lunar atmosphere. It was first suggested by Kepler, as a probable mode of accounting for the phenomenon. He conjectured [that the rays of light proceeding from the sun to the earth, might be refracted in passing through the moon's atmosphere, and might thereby occasion an appearance resembling the luminous ring[‡]. This view of the origin of the phenomenon was regarded with favour by Halley, although at the same time he admitted that it could not be considered as fully established. If the ring was due to the existence of a lunar atmosphere, it ought invariably to appear concentric with the moon. On the other hand, if it arose from the presence of an atmosphere about the sun, it could only surround the moon equably at the instant when the centres of the two bodies were in conjunction. In the latter case, it is manifest that the ring ought to appear broadest first at the point where the sun had just disappeared behind the moon's limb, and afterwards at the opposite point where he was about to emerge. It is easy to see, therefore, that observations on the position of the ring during an eclipse, if executed with precision, might afford a valuable criterion for deciding the question, whether the ring was an appendage of the sun or moon. All such observations have, however, been hitherto so vague and contradictory, that no reliable conclusion can be deduced from them. The ring fades off so imperceptibly from the moon's limb, that the establishment, beyond all doubt, of a variation in its breadth during the passage of the moon across the solar disk, if even such a variation existed, would seem to be impracticable. In some instances it has been affirmed that the ring appeared concentric with the moon, while in others it has been maintained with equal confidence, that it was concentric with the sun. The observations in general are decidedly more favourable to the supposition of the ring being concentric with the sun, than to its holding a similar relation with respect to the moon[§]. But indeed it is evident, from a multitude of other considerations, that the moon does not possess an atmosphere so extensive as that which the ring would assign to her. Assuming the ring which was visible during the total eclipse of 1806 to be really due to such a cause, Ferrer calculated, from the apparent breadth of the ring, that the atmosphere must have attained an elevation of 348 miles above the moon's surface, whence it would seem to be fifty times more extensive than the earth's atmosphere. He remarked, however, that the refraction of the lunar atmosphere, as indicated by observations of occultations and eclipses, is so small, as to prove, beyond all doubt, that its density is 1980 times less than the density of the terrestrial atmosphere*. The observations of the total eclipse of 1842, tend also to shew that if a lunar atmosphere really exist, its effects are totally inappreciable†. It is impossible, therefore, to avoid the conclusion that the luminous ring cannot proceed from such a cause.

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[*] Mem. Ast. Soc., vol. xv., p. 16.

[†] Gior. dell' Ist. del Lomb., tom. iv., p. 306.

[‡] Ad Vitellionem Paralipomena, p. 302; Epitome Astronomiæ, p. 893.

[§] Halley, while inclined to suppose that the luminous ring which appeared during the total eclipse of 1715, arose from the presence of an atmosphere about the moon, did not deny at the same time, that some persons found the ring to increase in breadth as the emersion approached. "This circumstance," says he, "*together with the contrary sentiments of those whose judgment I shall always* revere, makes me less confident" (*Phil. Trans.* 1715, p. 249). It is not improbable that Halley here alludes to the illustrious Newton, with whom he lived on terms of intimate friendship.

Finding that the hypothesis of a lunar atmosphere was incapable of accounting for the phenomenon of the luminous ring, La Hire suggested, that it might be produced by the reflexion of the solar rays from the inequalities of the moon's surface contiguous to the edge of her disk, combined with their subsequent passage through the terrestrial atmosphere. In order to obtain an experimental illustration of this view of the origin of the ring, he took a round unpolished stone of a yellowish colour, which was about two inches in diameter; and having suspended it in the air, at the window of his chamber, so as to appear in the direction of the sun, he retired within the chamber, keeping his eye in the centre of the shadow, until the stone covered the sun and extended a little beyond it. Observing the stone at this distance, he perceived around it a bright margin of light which he ascribed to the reflexion of the solar rays from the asperities of its edge, and upon the same principle he asserted that the luminous ring which appeared around the moon during a total eclipse of the sun, might be accounted for‡. Delisle, however remarked, as a proof of the fallacy of this explanation, that the same appearance would be produced if the stone was *perfectly smooth*, nay, he even asserted, that if instead of the stone, a piece of black pasteboard, cut out into any shape approaching to a circle was employed, there would appear a similar line of light around its edges. It was clear, therefore, that the hypothesis of La Hire was untenable.

Delisle was the first who conjectured that the luminous ring might be occasioned by the diffraction of the solar rays which passed near the moon's edge. He exhibited an illustration of the effect so produced by admitting the sun's light through a very small aperture into a dark room, and then receiving the cone of light thus formed upon a metal disk of somewhat larger dimensions than the circular section of the cone. Upon then viewing the metal disk from behind, either with the naked eye or with a small telescope, he perceived around it a bright margin of light, exactly resembling that which was visible in the experiment of La Hire [*].

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* Trans. Amer. Phil. Soc., vol. vi., p. 274.

† Louville has stated, that at London, during the progress of the total eclipse of 1715, the part of the solar disk contiguous to the eastern limb of the moon appeared to grow continually paler, so as to announce beforehand that it was about to be eclipsed (*Mém. Acad. des Sciences*, 1715, p. 94). This circumstance was considered by him to afford a very convincing proof of the existence of an atmosphere about the moon. It is to be remarked, however, that modern observations of eclipses have not indicated the slightest trace of a phenomenon analogous to that which Louville asserts to have noticed. The luminous ridges termed *faculæ*, which occasionally appear on the surface of the sun, are especially favourable for such observations, on account of their uniform aspect, and the consequent ease with which any variation in their brightness, resulting from the interposition of the lunar atmosphere, or any change in their form due to the refraction of the solar rays in passing through it, might be detected; but although M. Arago and his colleagues watched several of such ridges with great attention during the progress of the total eclipse of 1842, they were unable to discern the slightest change in their appearance as they ap-

The explanation of the luminous ring proposed by Delisle has been favourably received by several enquirers in recent times, but the attempts to obtain an experimental illustration of it have not been attended with uniform success. In some instances the sun was artificially eclipsed by placing an opaque disk at the focus of the telescope; but no appearance resembling the luminous ring was discernible around the edge of the disk. In 1846 Prof. Powell communicated to the Astronomical Society, a paper containing an account of a series of experiments executed by him, with the view of elucidating this interesting subject [†]. He ascribes the failure of some of the previous experiments of the same nature to the circumstance that the origin of light which was situate in the focus of the telescope was made to coincide with the opaque disk eclipsing it, whereas, in all cases of diffraction it is an indispensable condition that the intercepting body should be at *some* distance from the origin of light. Before proceeding to describe his own attempts to produce the phenomenon of the luminous ring, he remarks that in all those experiments which have for their object the exhibition of the ordinary diffractive fringes, it is essentially necessary, in the first place, that the origin of light be a point, or as nearly as possible so; and, secondly, that the area of the diverging rays extend beyond the edge of the opaque diffracting body [‡]. Having admitted the sun's light through a series of apertures varying from $\frac{1}{40}$ to $\frac{1}{4}$ of an inch in diameter, he found that the fringes ceased to be distinctly visible when the diameter exceeded $\frac{1}{8}$ of an inch. Moreover, he remarked, that in all such experiments the fringes could only be seen directly, as forming an optical image in the air, which was magnified by an eye-lens.

A phenomenon of a different kind was, however, perceptible, which continued to be discerned even when the conditions of the experiment were esentially varied. When a circular disk was employed to intercept the light, it was seen with the naked eye, or in a telescope, at a distance, bordered by *a luminous ring*, even in those cases wherein the diameter of the aperture exceeded $\frac{1}{4}$ of an inch, and whether the area of the rays fell without or within the opaque disk. It was found also, that although the ring was distinctly visible with the telescope when in focus for the opaque disk, it could not be seen with the eye-lens. Upon this ground Prof. Powell concludes that the phenomenon of the ring is not an optical image like that formed by the diffractive fringes, and, therefore, that it cannot be ascribed to the principle of diffraction, at least when considered according to the usual acceptation of the term.

Prof. Powell found that the ring ceased to be discernible when the area of the rays fell short of $\frac{1}{4}$ of the diameter of the diffracting disk, and that it increased in breadth as the area approached to an equality with the disk. When the area of the rays was not concentric with the disk, the luminous ring appeared broadest at the part where the rays approached nearest the edge of the disk.

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[*] Mém. Acad. des Sciences, 1715, p. 166, et seq.

[†] Mem. Ast. Soc. vol. xvi., p. 301, et seq.

[‡] It was in consequence of employing too large an aperture that Mariotte failed to verify the experiment of Grimaldi, and was hence led to doubt the reality of the

In order to establish the experiment more fully, the ring was examined in a telescope with a magnifying power of 20, furnished with cross wires in its focus. Before the light was allowed to fall upon the opaque disk, the edge of the latter was made to coincide with the intersection of the wires; but as soon as the light was admitted through the aperture, the ring was seen *extending sensibly beyond*. The intersection of the wires was now made to coincide with the outer edge of the ring, and upon the exclusion of the light the disk was seen to have *fallen sensibly within*.

When the aperture of the telescope was diminished to $\frac{1}{4}$ of an inch, the ring still continued to be distinctly seen, but the diffraction produced by the contraction of the aperture now caused the fringes to be also visible.

According to Prof. Powell, the colour of the ring was yellowish. There also appeared a faint bluish light extending within the dark circular area encompassed by it.

The circumstances of the above-mentioned experiment, abstractedly considered, bear a considerable resemblance to those which determine the appearance of the luminous ring around the moon during a total eclipse of the sun. Moreover, the results may be said also to agree in their main features. It would seem, therefore, not improbable, that they are attributable to the same physical cause. This conclusion, however, has been objected to, on the ground of the resemblance between the natural and artificial eclipses not being sufficiently perfect, since, in the case of the artificial eclipse, the diffracting body is surrounded by a medium possessing a considerable power of reflecting the solar light. Again, it has been remarked by Sir David Brewster, that in all experiments similar to those above alluded to, the breadth of the ring is totally independent of the *magnitude* of the diffracting body, and therefore the unavoidable conclusion is, that in the case of the natural eclipse, the ring would be utterly invisible on account of the comparatively immense distance of the moon from the earth *. This must be considered as a fatal objection to the above explanation, if the principle upon which it is founded be admitted to be true. But besides, the diffraction theory is incapable of offering any account whatever of many of the subordinate features of the ring, and therefore upon this ground alone, it cannot be considered as affording a faithful representation of the phenomenon.

While some persons have sought to explain the luminous ring, by assuming it to be an appendage of the moon, others have endeavoured to effect the same object by referring its origin to the sun. It has been already mentioned that the illustrious Kepler, who had the merit of first directing the attention of astronomers to the ring, suggested that it might arise from the refraction of the lunar atmosphere. Another hypothesis advanced by him, as probably involving the true explanation of the phenomenon was to the effect that it was occasioned by the combustion of the ether in immediate contact with the sun †. It cannot be denied that, at first sight this explanation appears very plausible, but it is to be remarked that the principle of an ethereal fluid pervading the celestial regions, upon which it is founded, is unsupported by any positive evidence of a trustworthy character, and therefore it cannot be viewed in any other light than as a gratuitous assumption.

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* Brewster's Edin. Encyclop., Art. "Astronomy."

† Ad Vitellionem Paralipomena, p. 301; Epitome Astronomiæ, p. 893. In order to account for the circumstance of the darkness not being equally intense during all *total eclipses* of the sun, Kepler imagined the ether to be sometimes very dense, and therefore easily inflamed; while, on the contrary, at other times being very rare and limpid, it was less liable to be affected by the action of the solar rays. In his explanation of this hypothesis he mentions an experiment which it may not be uninteresting to describe, as it ap-

Other persons have sought an explanation of the luminous ring in *the zodiacal light.* It is to be remarked respecting this appendage of the sun, that it does not encompass his disk symmetrically, being somewhat elongated in the direction of the ecliptic. Dominic Cassini, to whom its discovery is due, remarked, that if it were palpably visible, it would probably appear as a faint *chevelure* about the sun*. Some slight traces of an elongation of the luminous ring which appears around the moon during a total eclipse of the sun, have led to the surmise that it may possibly be no other than the zodiacal light. In the vast majority of cases, however, the ring has been found to be quite circular; and even in those cases wherein luminous expansions have been observed at opposite extremities of a diameter of the ring, the elongation resulting therefrom does not take place in the direction of the ecliptic.

The most probable explanation of the luminous ring is that according to which it arises from the presence of *an atmosphere about the sun.* Its round figure, its nebulous structure, and its gradually-diminishing density outwards, are all favourable to the supposition of its being due to an elastic fluid encompassing the solar orb, and gravitating everywhere towards its centre. It is true that precisely similar results would ensue from the existence of an atmosphere about the moon; but, in fact, there is no reason to suppose that the moon possesses an atmosphere capable of producing an appreciable effect. On the other hand, the hypothesis of a solar atmosphere is not only warranted by the analogy of the other bodies of the planetary system, but is also supported by evidence of a positive nature derived from observations on the physical constitution of the sun. The changes presented by that body when viewed in the telescope can only be consistently accounted for by the supposition of two dissimilar envelopes of matter suspended in a transparent atmosphere at different altitudes above his surface. That such a circumambient fluid really exists is shewn most unequivocally by the gradual diminution of the brightness of the solar disk towards the margin. With respect to this fact, Mr. Airy states it is so palpable to observation, that when in the course of his experiments he had occasion to throw a portion of the sun's disk upon a small screen, he was always able to determine whether the limb was approaching the edge of the fully-illuminated screen simply by the change of the inten-

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pears to contain the germ of the discovery of *the diffraction of light* usually ascribed to Grimaldi. Having inserted two small apertures in the window of a dark chamber, one of which was about the size of a grain of millet, and the other equal in magnitude to a pea, he drew two circles upon a screen placed opposite, the diameters of which bore the same proportion to each other as those of the apertures. The light of the sun being then admitted through the larger aperture, depicted an image on the screen which exactly coincided with the larger circle; but when this aperture was closed, and the light was admitted through the smaller one, *the image was no longer well defined, its margin was somewhat dusky, and finally it surpassed in magnitude the smaller circle.*

* Cassini first began to see the zodiacal light in March, 1683; and in the *Journal des Savans* for June of the same year, he made the remark alluded to in the text. For an account of the observations (extending over several years) by means of which he definitely established the existence of this phenomenon. See *Anc. Mém. Acad. des*

sity of illumination[*]. Now it is very manifest that the presence of an atmosphere about the sun would occasion a variation in the brightness of his disk similar to that above alluded to, since the rays emanating from the edge, by reason of their obliquity, would pass through a greater depth of the elastic fluid, and therefore would undergo a more considerable absorption than those proceeding from the regions nearer his centre. Again, it is to be remarked, that there are not, as in the case of the moon, any facts at variance with the supposition of an atmosphere about the sun, so extensive as that which the luminous ring would seem to indicate. Moreover, the hypothesis of a solar atmosphere adapts itself to the modifying phenomena by which the ring is occasionally characterised, with greater facility than any other. Thus what can be more natural than the conclusion that the reddish *maculæ* observed during the total eclipses of 1733 and 1842 were clouds suspended in the solar atmosphere? These phenomena are so intimately connected with the question relative to the existence of the luminous ring, that it will be desirable to present the reader with a detailed account of the more prominent features by which they were distinguished.

With respect to the spots which appeared during the total eclipse of 1733, Vassenius gives the following description of them in a short paper on the eclipse which he transmitted to the Royal Society of London:—"But what seemed in the highest degree worthy not merely of the admiration, but also of the attention of the illustrious Royal Society, were some reddish spots which appeared in the lunar atmosphere *without* the periphery of the moon's disk, amounting to three or four in number, one of which was larger than the others, and occupied a situation about midway between the south and west. These spots seemed in each instance to be composed of three smaller parts or cloudy patches of unequal length, having a certain degree of obliquity to the periphery of the moon. Having directed the attention of my companion to the phenomenon, who had the eyes of a lynx, I drew a sketch of its aspect. But while he, not being accustomed to the use of the telescope, was unable to find the moon; I again, with great delight, perceived the same spot, or, if you choose, rather the invariable cloud occupying its former situation in the atmosphere near the moon's periphery." He adds, that he continued to observe the phenomenon for the space of 40 seconds, until it finally vanished with the appearance of the first rays of the sun[†].

Vassenius, in the foregoing account, assumes without hesitation, that the luminous ring arises from the existence of a lunar atmosphere. His interesting statement respecting the reddish spots which he perceived in it, is corroborated by the pastor of Marstroom, who appears also to have witnessed a similar phenomenon. The following are the terms in which Celsius, while citing the observation of Vassenius, alludes to its confirmation by that individual:—"In the luminous ring itself, he (Vassenius) perceived three or four lucid *maculæ* of unequal form and magnitude near the lunar periphery, but still not adhering to it. He also affirms, that at Marstroom, M. Brag, the pastor of the place, also saw the same *maculæ* near the moon's limb, through an English telescope of excellent construction."[‡]

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[*] Mem. Ast. Soc., vol. xv. p. 11.

[†] Phil. Trans., 1733, p. 135.

It is probably a similar phenomenon which is alluded to by Sueblius, another Swedish pastor residing at Calmaria on the Baltic Sea. In his account of the same eclipse, he states that, "on the right of the sun towards the north, he perceived *two lucid streaks* parallel to each other and inclined to the horizon."*

The following is a description, by M. Mauvais, of the phenomena of a similar nature which appeared during the total eclipse of 1842, as they were seen by him at Perpignan.

"A few seconds after the total obscuration, while attempting to determine the breadth of the ring, I perceived a reddish point at the inferior limb of the moon, which did not, however, project sensibly. When *fifty-six* seconds had elapsed from the total obscuration, the reddish point of which I have been speaking transformed itself into two protuberances, similar to two contiguous mountains, perfectly well defined. They were not of a uniform colour; upon their flanks were seen streaks of a deeper tint. I cannot give a more exact idea of their aspect than by comparing them to the peaks of the Alps illuminated by the setting sun and seen afar off. *One minute and ten seconds* had elapsed from the total obscuration, when a third mountain was perceived to the left of the two others. It exhibited the same aspect as far as regards colour. It was flanked by some smaller peaks, but all were perfectly well defined.

"While this third mountain was in the process of issuing forth, the first two continued all the while to increase. They attained a height which, according to my estimation, subtended an angle of about 2′.

"The interval between the two groups appeared to embrace an arc of about 25° upon the moon's limb. The most considerable group, the most western in appearance, seemed to me to be a few degrees to the left of the lowest point of the lunar disk."†

M. Mayette, a captain of the French Engineers, who also observed the eclipse at Perpignan, compared the protuberances to *beautiful sheaves of flames*. He states, that he continued to see the most northern sheaf a few seconds *after* the emersion of the sun.

M. Petit states, that at Montpellier the two inferior points first appeared, the more western before the other. They gradually but rapidly increased in magnitude, as well-defined objects would have done, emerging from behind the lunar disk. By actual measurement M. Petit obtained 1′ 45″ for the angular magnitude of the largest of the luminous protuberances.

At Toulon, which was near the southern limit of the lunar shadow, Captain Bérard, of the French Marine, remarked that, during the interval of total obscuration, there was seen beyond the moon's limb, near the region where the sun was about to emerge, *a very slender red band, irregularly indented, or as it were furrowed here and there with hollows.*

M. Flaguergues, who observed the eclipse at the same place, remarked,

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upon the statement of Vassenius, inasmuch as it represents the spots to be *in contact* with the moon's limb, it may be desirable to give the precise words in which it is expressed in the original by Celsius, which are to the following effect:—"Has *etiam* maculas Marstrandii per Anglicanum optimæ notæ telescopium *juxta* lunæ limbum perceptas esse refert a pastore loci Dn. Brag." It is clear from the words in italics (which however are not marked so in the original) that the observation of M. Brag, so far from contradicting that of Vassenius, affords a most satisfactory confirmation of it.

* Acta Lit. et Scien. Suec., tom. iv., p. 63.

that previous to the reappearance of the sun, he perceived first one luminous point, then a second, and soon afterwards a third, all visible in the region *where the sun was about to emerge.*

M. Valz thus describes the appearance presented at Marseilles during the totality of the eclipse. "Forty seconds previous to the end of the total obscuration, I perceived near the part of the moon's limb where I expected the sun to reappear, two points near each other of very great brilliancy, more brilliant even than stars of the first magnitude. Their light was white like that of the sun. There was seen issuing from each of these points a train of light resembling that which is perceived in a dark room, into which the solar light penetrates through an aperture, but only more divergent; and *coinciding in direction with the rays issuing from the luminous ring.* . . . Fifteen seconds afterwards, a third brilliant point appeared, more to the north, accompanied also with a diverging train of light, and contributing to form a portion of the radial *glory* of the ring." M. Valz, who supposed that the luminous points perceived by him were situate a little *within* the margin of the lunar disk, was under the impression that they were occasioned by the sun shining through deep valleys on the moon's surface, which in each instance appeared to be closed at the top, by the projection of one of the sloping sides of the valley upon the opposite side. He states, that according to the relative movement of the sun and moon, the first luminous points were situate about $20''$ within the moon's border.

At Visan, which was situate close to the northern limit of the lunar shadow, M. Guerin perceived, immediately after the total obscuration, seven or eight *very distinct indentations*, which might have been taken for so many stars of different magnitudes. They were redder but less brilliant than Mars. If the whole periphery of the lunar disk had been bordered with similar luminous points, it would have presented the appearance of *a box of ebony garnished with rubies.*

At Digne, M. Eugene Bouvard perceived *two brushes of light* at the lower part of the moon's limb. They were formed of rays of a beautiful red mingled with some rays of orange colour. They *gradually became broader and fainter* as they receded from the moon's limb[*].

The accounts respecting the appearance presented by the luminous protuberances in Italy, do not differ materially from those of the French observers. Mr. Airy remarked, that in form they somewhat resembled saw teeth in the position proper for a circular saw, inclined in the same direction as that in which the hands of a watch turn. Their height was at least equal to a fourth of the breadth of the ring, or $1'$[†].

According to Mr. Baily, the luminous protuberances had the appearance of mountains of a prodigious elevation. Their colour was red, tinged with lilac or purple. "Perhaps," says he, "the colour of the peach blossom would more nearly represent their aspect." They somewhat resembled the snowy tops of the Alpine mountains when coloured by the rising or setting sun. Their light was perfectly steady, having nothing of the flickering appearance observable in the *corona.* The most considerable was bifurcated down to its base, insomuch that one might have said, there were two protuberances, the one being projected upon the other[‡].

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[*] Annuaire, 1846, p. 420.

At several of the stations in Lombardy, where the eclipse was observed, only two protuberances were visible.

Sig. Santini states, that at Padua, the luminous protuberances presented the appearance of irregular columns of smoke, undulating upon broad bases, and seeming to ascend from behind the body of the moon *.

Sig. Biela, who observed the eclipse at the same city, states, that there appeared at the lower part of the moon's limb, three pyramids of a dark reddish colour, resembling burning coal, or rather approaching to the colour of purple. Two of these pyramids were near each other; the third, which was the largest, was more to the left, but still was to the right of the point at which the sun was about to reappear. The light of the sun first appeared in isolated points, which in a few instants united together, and formed an excessively slender lunule. *The lunule had already been formed a few seconds,* when the reddish pyramids disappeared †.

The following interesting account of the luminous protuberances is given by M. Littrow of Vienna.

"Shortly after the formation of the ring there appeared luminous spots in several points of the contour of our satellite. Three of these spots were particularly remarkable. Their colour was red with a tinge of blue. They very much resembled the summits of the glaciers gilded by the rising or setting sun; only they did not terminate in peaks. The largest spot was about 5′ in height, and was about 2′ broad at its base. Their aspect, which was first white, changed to rose colour and then to violet; and afterwards passed in a reverse order through the same tints. . . . The protuberances were visible *before they assumed a coloured hue*, and they continued to be visible after their colour was dissipated."‡

M. Otto Struve has stated, that at Lipésk, a little before the reappearance of the sun, M. Schidlowsky perceived several rose-coloured flames which appeared suddenly to burst out at several parts of the lunar disk. They resembled mountains, whose height, by estimation, appeared to be about 2′. *A very large part of the lunar disk was garnished with a similar reddish bordering.* M. Schidlowsky was unable to examine the entire contour of the moon, for soon after he observed the phenomenon, the sun reappeared.

The first question which naturally suggests itself upon the perusal of the foregoing statements is this—do the luminous protuberances noticed by the various observers belong to the sun or to the moon? Admitting that they were appendages of the moon, they may be conceived as representing either mountains on her surface or clouds suspended in her atmosphere. To adopt the former of these explanations, however, would be assigning to the lunar surface inequalities, vastly exceeding those deducible from other phenomena of a less doubtful nature. Nor is the supposition of a meteorological origin more probable, since it appears from a multitude of considerations that if an atmosphere really exist about the moon, it is utterly incapable of holding in suspension clouds of such enormous magnitude as the apparent dimensions of the protuberances would indicate. The conclusion, therefore, seems to be unavoidable, that whatever be the real nature of the phenomena, they cannot be referable to the moon.

But, on the other hand, if they be admitted to be clouds suspended in the solar atmosphere, the explanation of the various facts relating to them becomes more consistent and probable.

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* Giornale dell' J. R. Istituto del Lombardo, tom. iv., p. 378.

1°. That they are solar and not lunar phenomena is evident from their being invisible for some time after the total obscuration, as well as from their appearance as luminous points, and their subsequent gradual increase of magnitude. All these facts are stated with great precision by M. Mauvais. They are the results which would obviously follow from the contiguity of the protuberances to the sun at the upper and western part of the limb, and their gradual disclosure by the motion of the moon towards the east. The last-mentioned fact of the protuberances being *gradually brought into view by the motion of the moon over the sun's disk* is distinctly asserted by M. Petit as having been very palpable to observation *.

2°. Near the *southern limit* of the lunar shadow the moon would project considerably beyond the superior part of the sun's limb, and therefore it might be expected that the large protuberances seen elsewhere would continue to be *almost totally concealed*, so as to appear only as luminous points. This circumstance would prevent them from attracting more peculiarly the attention of the observer, who might consequently be led to discover *similar points of equal minuteness*, which might otherwise have escaped his notice. The truth of this remark will be more apparent to the reader when he takes into consideration the extreme shortness of the interval of total obscuration, even at those places that were situate in the very centre of the shadow, and the variety of circumstances which call for the vigilant attention of the observer during the lapse of that interval. The phenomena observed at Toulon, which was situate only a short distance within the south boundary of the shadow, is quite consistent with the above conclusion.

3°. Towards the *northern limit* of the shadow, where the moon projected only in a small degree over the superior part of the sun's limb, it might be expected that the protuberances would become visible *immediately* after the commencement of the total obscuration. This circumstance actually occurred at Visan, as has been already mentioned. It is difficult, however, to account for the smallness of the angular dimensions under which the protuberances appeared at that place. Perhaps it arose from the shortness of the interval of total obscuration, not allowing the eye sufficient time to recover from the dazzling effect of the sun's light, so as to take due advantage of the obscurity which actually prevails. We have seen that the nonvisibility of the stars of the second magnitude during the total obscuration has been referred with great probability to the same cause.

Admitting, then, that the phenomena had an intimate connexion with the sun, there are various facts which render the supposition of their meteorological origin extremely probable.

1°. *The reddish colour* of the light emitted by them indicates that they possess the property of absorbing in a great degree all the rays of the spectrum, except the red, as in the case of the terrestrial clouds when they are seen illuminated by the sun after his disappearance under the horizon.

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* It is not to be denied that the size which the protuberances ultimately acquired, when taken into consideration with the fact of their first appearance *after* the total obscuration, as *mere luminous points*, considerably exceeds that which this explanation of their apparent growth would assign to them. It is to be remarked, however, that when they first

2°. The *radial appearance* which the protuberances exhibited to M. Eugene Bouvard can only be explained by the supposition of the rays of the sun forcing their way through the interstices of the clouds contiguous to his *luminous* surface, and illuminating the particles of the solar atmosphere situate beyond in the line of their emanation. So also with respect to the luminous points observed by M. Valz, it is impossible otherwise to account for the *constant emanation of the rays outwards* in a direction normal to the moon's limb.

3°. The zone of *a deep red colour* observed at Toulon towards the part of the moon's limb where the sun was about to emerge, clearly indicates the accumulation of nebulous matter in the lower regions of the solar atmosphere, as well as the condensation of the circumambient fluid of which the latter is composed, towards the surface of the sun, arising from the pressure of the superincumbent strata.

The indications of a solar atmosphere afforded by phenomena bearing a strong resemblance to *clouds and nebulous strata*, as well as by the appearance of *rays in a direction normal to the sun's limb* [*], although chiefly conspicuous during the total eclipses of 1733 and 1842, have presented themselves on numerous other occasions in a greater or less degree of developement. As this is a point of considerable importance in the question relative to the physical constitution of the sun, it perhaps may not be out of place to give a brief statement of all those observations which tend more especially to illustrate it.

Captain Stannyan, who observed the total eclipse of 1706 at Berne, states, that *the emersion of the sun was preceded by a blood-red streak of light* from its left limb, which continued not longer than six or seven seconds of time [†].

A similar phenomenon was remarked by both Halley and Louville on the occasion of the total eclipse of 1715. Halley, in his account of the eclipse, states, that about two or three seconds before the emersion, a *long and very narrow streak of a dusky but strong red light*, seemed to colour the dark edge of the moon on the western side where the sun was just coming out [‡]. Louville states that, towards the end of the total obscuration, there was seen about the moon's limb, at the place where the sun was about to emerge, an arc of *a deep red colour*. In order to obtain an assurance that the phenomenon was not due to the aberration of the telescope, he took the precaution of viewing it through the very centre of the object-glass, when he found that it exhibited the same colour as before [§].

Don Ulloa, in his account of the total eclipse of 1778, speaks of a point of red light which appeared on the edge of the moon's disk near the place where the sun was about to emerge. It first became visible about a minute and a quarter before the reappearance of the sun. It then equalled a star of the fourth magnitude, but it gradually became larger. When it attained the size of a star of the second magnitude, the edge of the sun emerged from behind the moon, and it then ceased to be visible. He remarks that it appeared so near the edge of the moon as to leave no doubt of its belonging to the body of the sun*. He, in fact, supposed the phenomenon to arise from the sun being visible *through a hole in the body of the moon.* There can be no doubt, however, when we take into consideration the contiguity of the luminous point to the edge of the lunar disk, as well as its deep red colour and its gradual increase of apparent magnitude, that it was exactly of the same nature with those luminous points that were seen at several places during the total eclipse of 1842.

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[*] It has been mentioned in the account of the appearance presented by the luminous ring during the total eclipse of 1733, that there were seen emanating from it rays of *unequal length*, which constantly maintained the *same position*. This description is clearly at variance with the supposition of the phenomenon being due to the irregular scintillation of a luminous object.

[†] Phil. Trans., 1706, p. 2240. [‡] Phil. Trans., 1715, p. 250.

With respect to the total eclipse of 1806, Ferrer remarks that, a few seconds before the emersion of the sun, he observed *a zone* to issue *concentric with the sun*, similar to the appearance of *a cloud illuminated by the solar rays* †.

It has been mentioned that, during the total eclipse of 1842, there was seen at Toulon *a band of red light* around a portion of the moon's limb. The following observations of the same eclipse afford evidence of a similar phenomenon.

Signor Santini states, that while engaged in watching the last fragment of the solar light, in order to determine the instant of total immersion, there appeared on the right as well as on the left several *luminous streaks separated by obscure and even absolutely dark intervals*, which vanished a little before the immersion of the last point of the solar disk ‡.

Signor Conti mentions in his account of the eclipse, that, previous to the emersion of the sun, there appeared a *reddish arc of light* on the right side of the lunar disk. When it became a little brighter, he perceived another vivid light *separated from the former by a dark interval.* The phenomenon was visible only for a short time §.

Sig. Pietropoli, who also observed the eclipse at Padua, states that previous to the total immersion, he perceived below to the right *two very beautiful patches of light* ‖.

Sig. Radman, another observer at Padua, appears to have witnessed the phenomenon both before and after the total obscuration. He asserts that no sooner was the sun totally obscured, than there appeared a *very slender streak of a reddish light*, surrounding somewhat less than half the circumference of the moon, disposed equally on each side of the place where the last ray of the sun was extinguished. He adds, that it soon vanished, but that it reappeared at the instant of the emersion of the sun ¶.

M. Schumacher thus describes the appearance presented by the moon's limb immediately previous to the termination of the totality of the eclipse. "A little before the emersion of the sun, there appeared near that part of the lunar disk from behind which the first ray of light was about to issue, *a narrow streak of rose-coloured red light*, which extended, perhaps, over an arc of 70° or 80° along the moon's limb, and which disappeared with the luminous ring and the rose-coloured mountains as soon as the first ray of the sun darted forth *.

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* Phil. Trans., 1779, p. 110.

† Trans. Amer. Phil. Soc., vol. vi., p. 274. The following account of this phenomenon is given by Mr. Garnett, who assisted Ferrer in his observations. "Before the end of the total eclipse, the *west limb of the moon begun to be illuminated*, and the light increased so rapidly, that I at last mistook it for the sun's egress, and called the time to Mr. Ferrer; but he saw the error, and still kept his eye to the glass, when the first solar ray nearly blinded him."—*Tilloch's Journal of Natural Philosophy*, vol. xix., p. 322.

‡ Giorn. dell' Ist. del Lomb., tom. iv., p. 379. § Ibid., p. 380.

M. Otto Struve was under the impression that, immediately before the disappearance of the sun, he perceived *a red streak of light* along the moon's limb, about 45° from the point where the total immersion took place †.

The radial appearance noticed during the total eclipse of 1842, immediately before and after the complete obscuration, is also a phenomenon which obviously indicates the existence of a solar atmosphere. Thus, in the description of the eclipse ascribed to Mrs. Airy, it is affirmed, that with respect to the place where the sun had gone in, and that where he was about to emerge, *the light came out successively like little beams from the moon's edge* ‡. Moreover, it is stated that the light of the ring did not present any remarkable change until the *little flame-coloured beams* shot out for a few seconds before the reappearance. This emanation of red rays in a direction perpendicular to the moon's limb, can only be accounted for by supposing the rays of the sun to be obstructed in their progress outwards, by a dense atmosphere enveloping the solar surface and exercising an influence on the primitive rays of the spectrum similar to that produced by the terrestrial atmosphere.

The accounts respecting *annular eclipses* are not wanting in allusions to phenomena indicative of a solar atmosphere, analogous to those which have been already noticed. A brief review of those which more especially bear on the subject, cannot fail to prove interesting to the reader.

Maclaurin, in his account of the annular eclipse of 1737, states that a little before the formation of the ring, a remarkable *point or speck of pale light* appeared near the middle of the part of the moon's limb that was not yet come upon the disk of the sun, and that a *gleam of light* more faint than this light seemed *to be extended from it to each horn*. This phenomenon was seen about fifteen seconds previous to the projection of the whole body of the moon upon the solar disk §.

Maclaurin also states that Lord Aberdour, who observed the same eclipse, remarked that a *narrow streak of a dusky red light* coloured the dark edge of the moon, immediately before the ring was completed, and after it was dissolved ||.

Van Swinden, in his account of the annular eclipse of September 7, 1820, as observed by him at Amsterdam, states that just before the annulus was formed, he perceived above the yet immersed limb of the sun, a very small arch of light, which was no part of the solar disk. He also adds that the space between it and the moon's dark limb *was not illuminated*. It might have been compared, as to colour and appearance, with the end of the flame of an argand lamp projecting beyond the chimney or glass tube ¶.

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* Annuaire, 1846, p. 437.

† Ibid., p. 437.

‡ Mem. Ast. Soc., vol. xv., p. 16.

§ Phil. Trans., 1737, p. 181.

|| Ibid., p. 182.

¶ Mem. Ast. Soc. vol. i., p. 145. Van Swinden mentions, in the account of his observations, that during the annular eclipse of 1764, a similar arc of faint light was seen around the moon's limb at Toulouse, by M. Darquier.

The late Mr. Henderson, who observed the annular eclipse of May 15, 1836, at Edinburgh, states, in an account of his observations of the phenomenon, that, previous to the formation of the annulus, when the cusps were about 30° or 40° from each other, *an arc of faint reddish light* was seen extending between them. This appearance lasted several seconds *.

The observations made in the United States of America during the annular eclipse of February 10, 1838, furnish additional illustrations of the same phenomenon. Prof. Henry, who observed the eclipse at Princeton College, New Jersey, states that about two minutes before the formation of the ring, he perceived *an arch of faint light* between the cusps, and shortly afterwards a *brush* of greater intensity, projecting from near the lower cusps †. A similar appearance was witnessed by Prof. Johnson, who observed the eclipse at Philadelphia. About a minute before the formation of the ring, he perceived *an arch of faint light* beyond the cusps, with *a speck or brush in the centre extending outwards to a distance of two degrees.* At the instant of the formation of the annulus, the arch of light had much increased in brightness. The brush which was originally in the centre, now extended from cusp to cusp, radiating outwards to the extent of nearly three digits ‡.

Two observations at least, tending to illustrate the subject under consideration, were made during the annular eclipse of October 9, 1847. Dr. Forster, who witnessed the eclipse at Bruges, has stated in an account of his observations that there was visible a very remarkable luminous arc or ridge of light, *differently coloured from the rest of the sun*, extending along and immediately on the limb of the moon between the cusps. The appearance lasted nearly five minutes §. Captain Jacob, who observed the eclipse at Bombay, states, that shortly before the annular phase, a *faint ray or brush of light was seen issuing from the sun's northern cusp*, which soon after extended in both directions as a *tangent to the sun's limb* ||.

It might be expected, that the arcs and specks of light observed around the moon's dark limb during the *total* and *annular* eclipses of the sun, might be also occasionally seen during *partial eclipses* of considerable magnitude. The following are a few instances in which phenomena of this nature appear to have been visible.

It has been already mentioned that an eclipse of the sun happened in the year 1605, which was total at Naples. Kepler states that at Antwerp, where the upper limb of the sun projected about a digit beyond the moon's limb, the circumference of the lower limb of the moon was rendered visible by *means of a ruddy light bordering it* ¶.

On the 22nd of June, 1666, there occurred a partial eclipse of the sun which was visible at London. In an account of the phenomenon as observed by Hooke and several other individuals, it is stated that "about the middle between the perpendicular and westward horizontal radius of the sun, viewing it through Bayle's 60-feet telescope, *there was seen a little of the limb of the moon without the disk of the sun*, which seemed to some of

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* Mem. Ast. Soc., vol. x., p. 37.

† Silliman's Journal, vol. xxxviii., p. 166.

‡ Ibid., vol. xxxviii., p. 159.

§ This statement is contained in a letter to Prof. Powell (Brit. Ass. Rep., 1848; *Transactions of the Sections*, p. 4).

|| Monthly Proc. Ast. Soc., vol. viii., p. 27.

¶ *De Stella Nova*, p. 116.

the observers to come from some *shining atmosphere* about the body either of the sun or moon." *

The eclipse of September 24, 1699, which was total in some parts of the north of Europe was very considerable at Leipsic, where 11·20 digits of the solar diameter was immersed. In an account of the eclipse, as it appeared at that city, it is asserted that when the cusps of the moon were almost parallel to the horizon, the extremity of the moon looking downwards *was somewhat enlightened, and of a kind of saffron colour* †.

The solar eclipse of July 14, 1748, although seen annular in various parts of Scotland, failed in a small degree from being so at Aberdour Castle, where it was observed by Lord Mortoun the proprietor, Lemonnier the French astronomer, and Short the famous optician. In his account of the eclipse, Short states that when the interval between the moon's cusps did not exceed one-seventh of the moon's circumference, *a brown light* was plainly perceived both by Lord Morton and himself, to extend from each cusp about one-third of the whole distance between them. He adds also, that he perceived at the extremity of this brown light, which came from the western cusp, *a larger quantity of light than usual* ‡.

The following is an account of the appearance presented by the partial eclipse of February 12, 1831, at Burlington, New Jersey, United States, where it was observed by Mr. Gummere :—" At the time of greatest obscuration," says that individual, " the cusps were very sharp and well defined, but a *thread of light extended from each along the circumference of the sun's disk.* About a minute after *a spot of light* was observed a few degrees from the south-western cusp, extending inwards from the thread which proceeded from that cusp." §

Bessel thus describes the appearance presented during the solar eclipse of May 15, 1836, which failed in a small degree to be annular at Konigsberg, where that astronomer observed it; although in reality it was of the annular species :— " About twenty-five seconds before the nearest approach of the centres of the two bodies, I perceived near the extremity of the superior cusp a luminous point, which, without having the brightness of the sun, was very distinctly visible with the powerful heliometer. As the cusps were then approaching each other, I hoped that the annulus was about to form, but this did not happen. With respect to the point which I have just alluded to, it became more luminous Other similar points appeared which soon united together, and rendered visible all the portions of the moon's limb, included between the extremities of the cusps." ||

Captain Smyth, who observed the same eclipse at Bedford, where it was also partial, has given the following description of a similar phenomenon :— " A little before the greatest obscuration, the sun having then an altitude of about 40°, the southern cusp was considerably the brightest, and I then distinctly perceived a small portion of the lunar limb *outside* the solar orb. As the moon passed over, *her following limb was brightened by a fine line of light.*" ¶

The eclipse of February 10, 1838, although seen annular in several parts of the United States, slightly failed to appear so at New York. An

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* Phil. Trans., 1666, p. 295. † Ibid., 1700, p. 622. ‡ Ibid., 1748, p. 586.

§ Trans. Amer. Phil. Soc., New Series, vol. iv., p. 128.

|| Mem. Ast. Soc., vol. x., p. 32.

Smyth's Cycle of Celestial Objects, vol. i., p. 142.

intelligent observer of the eclipse at that city alludes—in a note containing suggestions relative to the total eclipse of 1842—to a phenomenon which he says *may indeed have been observed at other times and recorded, although he has no knowledge that it has been.* On that occasion he noticed *several minutes* before the time of the nearest completion of the ring, the fine cusps of the sun's unobscured crescent prolonged by a *hair breadth line of brightness, totally diverse in colour and intensity from the sun's disk.* As the cusps approached, the line or thread of light in advance of each shot round the moon's edge, between them rapidly, till, at a certain time, the threads from the two, met and joined in one—thus uniting the cusps *.

The foregoing observations relative to the appearances noticed during annular and partial eclipses of the sun, present a satisfactory agreement with those made during total eclipses, in so far as they indicate the existence of a solar atmosphere. With respect to the luminous arc seen around the moon's dark limb, it would appear in some cases to indicate the ruddy streak of light which is usually perceived during total eclipses of the sun a few seconds before and after the complete obscuration; but in general the phenomenon in all probability represents a fragment of the luminous ring, rendered visible in consequence of the atmospheric light diffused around it being enfeebled by the partial obscuration of the solar disk. The specks or points of light observed on so many occasions are manifestly analogous to those seen at Toulon and elsewhere during the total eclipse of July 8, 1842. The brush of light observed during the annular eclipse of 1838 is very instructive. Its direct emanation *outwards* from a *faint* speck of light situate in luminous arc, shows that it was not the consequence of any irregular scintillation, but rather that it was occasioned by some extraneous cause acting constantly in a determinate direction, and totally independent of the brightness of the source from which it seemed to issue. In fact, it can only be explained by supposing the luminous speck to be a collection of nebulous matter, through which the rays of the sun had partially penetrated, so as to illuminate the particles of the solar atmosphere situated beyond in the line of their transmission.

According to the usual theory of the physical constitution of the sun, that body consists of an opaque nucleus, surrounded by an atmosphere in which are suspended at different elevations two enveloping substances of dissimilar physical properties, the lower envelope being imperfectly luminous, but capable in a high degree of reflecting light, while, on the other hand, the upper envelope forms a resplendent canopy of clouds, which are luminous in themselves, and constitute the source of the light diffused in every direction by the sun. The observations of solar eclipses would seem to indicate, that above the luminiferous envelope there exists a stratum of nebulous matter, which is visible only by means of reflected light. Various interesting questions present themselves for solution in connexion with the admitted existence of such a stratum. In the first place, does this third envelope exercise an influence in the production of any of the other phenomena which have been disclosed by observations on the physical constitution of the sun? M. Arago has very ingeniously suggested that the solar spots which exhibit the aspect of *a penumbra without a nucleus*, may arise from the superposition of masses of this nebulous matter above the luminiferous envelope. Secondly, the question arises, does any relation subsist between this non-luminous substance, which floats in the upper regions of the solar atmosphere, and either of the other two envelopes? Sir John Herschel, in his ingenious theory of the physical origin of the solar spots, supposes a perpetual circulation to be kept up in the solar atmosphere, analogous to that which in the case of the terrestrial atmosphere produces the phenomenon of the trade winds. Now, is it not reasonable enough to suppose that such a circulation will have the effect of continually agitating the non-luminous matter constituting the envelope nearest the solid nucleus of the sun, and throwing up masses of it above the luminiferous surface? This view of the subject, while it carries with it considerable probability, obviates the necessity of introducing into the theory of the physical constitution of the sun, the idea of a third envelope independent of the two others. Future observation, prosecuted more especially with reference to the position of the solar spots during the occurrence of eclipses of the sun, can alone be expected to throw any light upon this interesting question.

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* Silliman's Journal, vol. xlii., p. 396.

Although the physical connexion of the solar spots with the nebulous matter which appears to exist above the luminous surface of the sun, is a point which remains to be established definitively by future enquiry, there is some reason to suppose from the observations of Sig. Santini on the occasion of the total eclipse of July 8, 1842, that such a connexion really subsists.

In his account of the eclipse, that excellent astronomer states, that a few days previous to its occurrence, he observed a very extensive chain of spots on the sun, five of which appeared near the western margin of his disk. He adds that, although he unfortunately omitted to determine their precise position, he felt assured that at the time of the eclipse, they could not have been very far removed from the singular columns of smoke which appeared during the total obscuration[*].

The aspect presented by the moon during eclipses of the sun, has led to some curious speculations. Kepler has stated in one of his works, that during the obscuration consequent upon a total eclipse of the sun, the moon's surface is occasionally distinguishable by a *ruddy light*[†]. A similar appearance has been noticed in modern times during the occurrence of annular eclipses. Thus Mr. Baily, in his account of the annular eclipse of 1836, states, that "previous to the formation of the ring, the face of the moon was perfectly black; but on looking at it through the telescope, during the annulus, the circumference was tinged with a *reddish purple colour*, which extended over the whole disk, but increased in density of colour according to the proximity to the centre, so as to be in that part nearly black."[‡]

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[*] Giorn. dell' Ist del Lomb., tom. iv., p. 378. It has been already mentioned that the luminous protuberances seemed to Sig. Santini to resemble irregular columns of smoke, undulating upon broad bases. "*Come irregulari colonne de fuoco ondeggiante a larga base.*"

[†] Epitome Astronomiæ, p. 895.

[‡] Mem. Ast. Soc., vol. x., p. 17. Mr. Baily states that at the same time the *globular* form of the moon was very perceptible; he adds that Mr. Veitch (the gentleman at whose residence he observed the eclipse) also noticed the phenomenon, and that both agreed in the impression that the moon had the aspect of *a globe* of purple velvet. It is a remarkable fact, that an appearance exactly resembling that described by Mr. Baily was witnessed during the *total* eclipse of 1733; thus M. Tissel states, that at Skepshat in Sweden, "the moon's surface was brighter at the margin near the luminous ring; towards the middle it

Kepler ascribes the visibility of the moon's surface during her conjunction with the sun, to the light transmitted by reflexion from the earth, or the *earth-shine*, as it would be termed by the inhabitants of the moon[*]. This is doubtless the true explanation of the phenomenon.

The illustrious philosopher, just cited, while alluding to the visibility of the moon by a faint light during conjunction, does not affirm that the *irregularities of her surface* have been discerned on any such occasion. The first person who makes mention of this fact is Bigerus Vassenius the Swedish astronomer, to whose interesting observations of the total eclipse of 1733, allusion has already been made. In the account of that eclipse which he transmitted to the Royal Society, he asserts that with a telescope of 21 feet focal length, he perceived *several of the principal spots* on the moon during the total obscuration[†]. Ferrer also states, that during the total eclipse of 1806, the irregularities of the moon's surface were plainly discernible[‡].

The foregoing are the only two instances in which it appears that the irregularities of the moon's surface have been perceived during a central eclipse of the sun, whether total or annular. M. Arago, and several other observers, carefully sought to effect the same object during the total eclipse of 1842, but all their efforts proved fruitless. The fact of the lineaments of the lunar disk being discernible in some cases and not in others, is obviously attributable to no other cause than the variable condition of the atmosphere[§].

A singular appearance was noticed at Washington, U.S., during the progress of the partial eclipse of February 12, 1831. Mr. F. R. Hassler states, in an account of that eclipse, that "the most remarkable phenomenon was the distinctly painted inequalities of the moon, by the reflected light and shade upon its disk, presenting apparently elevations brilliantly illuminated, and intervals shaded in an ash-coloured shade, more or less dark and distinct as they were nearer to or farther from the sun, the edge of the moon towards the sun being always fully dark." He asserts moreover that the appearance, beginning when about one-eighth of the diameter of the sun was immersed, extended itself, with more or less variation, to about one-third of the moon's diameter, when it gradually faded into indistinctness, and the whole of the moon's surface appeared equally dark: the same phenomenon was witnessed in an inverted order towards the end of the eclipse[||].

The singular phenomenon alluded to appears to be totally inexplicable

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became black; and at the centre *seemed to protuberate.*" *Acta Lit. et Scien. Suec.*, tom. iv., p. 59. Sig. Piola also mentions that on the occasion of the total eclipse of 1842, as the moon advanced upon the solar disk, she seemed to all the spectators who were present with him *to protuberate at the centre*, and to be compressed at the sides. He adds that a similar appearance was noticed at Belluno. (*Giorn. dell' Ist. del Lomb.*, tom. iv., p. 303.)

[*] Epitome Astronomiæ, p. 895.

[†] Phil. Trans., 1733, p. 135.

[‡] Trans. Amer. Phil. Soc., vol. vi., p. 267.

[§] The following remark by Sig. Magrini relative to the appearance presented by the moon during the total eclipse of 1842, has some resemblance to the description contained in the text. "The light of the moon, termed *ashy*, seemed to be brighter towards the disk of the sun both before the immersion and after the emersion." (*Annuaire*, 1846, p. 375, cited from the *Gazetta Privileggiata* of Milan.)

[||] Trans. Amer. Phil. Soc., New Series, vol. iv., p. 131. The only explanation of this phenomenon that appears to possess any degree of plausibility is the following. It has been found, that during total eclipses of the sun, the irregularities of the lunar disk are sometimes discernible when the whole of the solar disk has been immersed. Let us suppose, that during the eclipse of 1831 the condition of the atmosphere was by any known physical principles. That it is connected in some way with the magnitude and relative position of the segment of the sun which has not undergone immersion, would seem obviously to follow from its mode of variation; but it is difficult to assign any cause why the moon's surface should appear more distinct on the side on which the sun is visible, than on the opposite side.

A phenomenon of a somewhat similar nature, but one admitting of a satisfactory explanation, was noticed by M. Arago during the total eclipse of 1842. Forty minutes after the commencement of the eclipse, while a considerable portion of the sun was still visible, he observed the dark contour of the moon *projected upon the bright sky;* it corresponded exactly with the portion of the limb that appeared upon the sun; he remarked also, that it was brighter at the parts where it met the solar disk*. He ascribes the phenomenon to the projection of the moon upon the solar atmosphere, the brightness of which, by an effect of contrast, rendered the outline of the moon's dark limb discernible. This explanation accounted for the fact of the limb being brighter towards the sun, since it appeared from all the observations of total eclipses, that the luminous ring exhibited the greatest concentration of light at those parts where it bordered upon the solar disk.

The contour of the dark limb of the moon was also visible to M. Eugene Bouvard at Digne, and to M. Flaguerges at Toulon; but it is remarkable that no other observer of the total eclipse of 1842 makes mention of a similar appearance.

Halley remarked, that during the total eclipse of 1715, he observed perpetual flashes, or coruscations of light, which seemed for a moment to dart out from behind the moon, now here and now there, on all sides, but more especially on the western side a little before the emersion†. Louville alludes to a similar phenomenon in his account of the same eclipse; he states, that during the time of the total obscuration, it seemed as if there had been a succession of trains of gunpowder constantly exploding on the

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so favourable for observation, that if the whole of the solar disk had undergone immersion, the dark and bright parts of the moon's surface would have been as plainly distinguishable as Ferrer states they were on the occasion of the total eclipse of 1806. According to M. Arago, the light by which the bright parts are visible in such a case, must exceed the light of the more obscure parts, by at least a sixtieth part. Now, at the commencement of the eclipse, the moon's surface is illuminated *unequally* by the *lumière cendrée,* or *ashy light,* and *equally* by the atmospheric light diffused by the *full sun;* and the former of these lights (or rather the excess of the light emitted by the bright parts over the light of the more obscure parts) bears so small a proportion to the latter, that the whole of the moon's surface is almost *equally* illuminated, and the various irregularities by which it is diversified will be completely effaced. As the eclipse, however, increases in magnitude, the light diffused by the visible segment of the solar disk diminishes at an *equal* rate, and in consequence, the light of the *lumière cendrée* continually bears *an increasing ratio* to the atmospheric light. At length, the latter is so much enfeebled, that the excess of the light of the bright parts of the lunar disk over the light of the more obscure parts, arising from the *lumière cendrée*, amounts to the sixtieth part of the atmospheric light, whereupon, according to the above remark, the irregularities of the lunar disk will become distinguishable. The American observer states, that the moon's surface became distinctly visible as soon as one-eighth of the solar diameter was immersed. It is difficult to conceive that at so early a stage of the eclipse the atmospheric light would be so much enfeebled, that the *lumière cendrée* would form a sensible fraction of it. Admitting, however, that the phenomenon was due to such a cause, it follows that the lunar surface will appear more illuminated towards the sun, on account of the greater intensity of the atmospheric light, except at the part bordering on the solar segment, *where it will naturally appear dark from the effect of contrast.*

* Annuaire, 1846, p. 372.

† Phil. Trans. 1715,

moon's surface. He remarks that the fulminations had a serpentine appearance, causing them to resemble flashes of lightning[*].

Weidler, in an account of the partial eclipse of August 4, 1739, as observed by him at Wurtemberg, makes mention of a similar phenomenon. He states that a friend of his, well skilled in astronomical observations, having viewed the moon towards the commencement of the eclipse with a telescope 9 feet long, noticed bright lights like flashes of lightning interspersed here and there upon her dark surface; and that after the lapse of one hour and nineteen minutes he again saw the same appearance[†].

Attempts were made during the total eclipse of 1842 to verify the foregoing observations, but it was only at Venice that any analogous coruscations of light were discerned. Sig. Zantedeschi, who observed the eclipse at that city, states, that from time to time there were visible upon the moon's surface faint illuminations of an intermittent character, resembling flashes of phosphoric light, intermingled with black streaks A similar appearance was noticed by M. Wullerstorf[‡].

The phenomena above referred to have been explained in various ways by different enquirers. Louville attributed them to the prevalence of electric storms in the lunar atmosphere; other persons suppose them to have been produced by volcanic energy. M. Arago is of opinion that they may have arisen from the occasional passage of aerolites across the field of view of the telescope. In support of this view of their origin, he cites one or two instances in which, during the total eclipse of 1842, there were seen meteoric lights traversing the regions of the heavens in which the sun and moon were then situate. From the circumstance of the phenomena having been visible only on two or three occasions, it is clearly impossible to arrive at any trustworthy conclusions on the subject.

At various places in France, a singular undulation of the solar light was remarked on the occasion of the total eclipse of 1842. In a letter to M. Arago, dated July 9, 1842, M. Fauvelle gives the following account of the phenomenon as witnessed by him at Perpignan:—"At the moment when the eclipse was about to become total, I perceived the last rays of the sun *undulate with great intensity and rapidity* upon a white wall of one of the military establishments of the rampart of St. Dominique. The effect might be compared with that which is observed when the light of the sun falls upon a wall or a ceiling after having been reflected from the surface of a sheet of water in a state of agitation. The same phenomenon reappeared at the instant of the emersion of the sun; the undulations which at first were very intense gradually died away, and disappeared altogether at the end of five or six seconds."[§]

M. Arago remarks also that during the few seconds which could be devoted by his colleagues and himself to the observation of such a phenomenon, the façade of the great tower of the citadel of Perpignan appeared to be illuminated by a *singularly-fluctuating light.*

M. Lenthéric thus describes the phenomenon as observed by him at Montpellier:—"A little before the commencement of the total obscuration, there were seen on the ground and on walls *undulating shadows* composed of a succession of arcs of three or four decimètres in length, but of much less breadth, which seemed to *turn upon themselves.* The effect was analogous to that produced by those moveable shadows which are seen at the bottom of a shallow basin filled with clear water, when the surface, slightly agitated, is illuminated by the rays of the sun."

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[*] Mém. Acad. des Sciences, 1715, p. 96.

[†] Phil. Trans., 1739, p. 228.

[‡] Annuaire, 1846, p. 364.

[§] Annuaire, 1846, p. 392.

A phenomenon similar to that mentioned in the foregoing accounts was also observed at Narbonne, Alais, and several other places in France. The following description of the appearance, as witnessed by M. Savournin at Seyne, is somewhat different from the other statements:—

"There were seen here shadows and luminous patches running after each other, the effect of which was similar to that produced by the passage of a succession of small clouds over the sun. These patches *were not all of the same colour. Some of them were red; others were yellow, blue, or white.* The children amused themselves in running after them, and trying to put their hands upon them. This extraordinary phenomenon was remarked only a few instants before the complete disappearance of the sun."*

It is remarkable that no observers of the total eclipse of 1842, out of France, make any allusion to the singular appearances noticed in that country, before and after the total obscuration. Almost equally silent upon this point are the observers of former eclipses of the sun. The following observations of the total eclipse of 1733, appear to have reference to an undulatory movement similar to that above mentioned.

Rydhenius, pastor of Forshem, states that "when the sun was about to lose his light, and also when he was about to recover it, he emitted rays that *undulated like the aurora borealis*, and were of a *fiery red colour*."†

Again the pastor of Flo, in his account of the same eclipse, affirms that, "towards the total obscuration stars were visible, and also a *singular fluctuation in the air*."‡

It might be supposed, although it is by no means probable, that the first cited of these two observations has reference to the appearance of the sun when viewed *directly*, a little before and after the total obscuration, and not to the effect which might be produced upon a surface illuminated by the solar rays. With respect to the second observation, it is still less improbable that it can be any other than a phenomenon of the same nature as that witnessed during the total eclipse of 1842; since it is difficult to believe that any agitation in the air could be recognised, except by viewing its undulations as projected upon some wall or building. The observation of the pastor of Forshem would also seem to have some analogy to that of M. Savournin, relative to the moveable shadows of *different colours* which appeared at Seyne during the total eclipse of 1842. As, however, the curious phenomenon witnessed in the latter instance does not seem to have been noticed, except at the place just cited, it would be desirable to obtain a more satisfactory verification of it by reference to some previous eclipse. The following statement made by Delisle, while citing some observations which appeared to him to support his theory of the luminous ring seen around the moon on the occasion of a total eclipse of the sun, manifestly refers to a phenomenon of a similar nature:—

"The second observation is one which a curious individual acquaints me with having made, by mere accident*. Having directed his attention to *a large white wall*, at the moment of the total immersion of an eclipse of the sun, he saw the moon's shadow pass upon the wall, *tinged with different colours.*"†

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* Annuaire, 1846, p. 393.

† Acta Lit. et Scien. Suec., tom. iv., p. 61. The following are the express words of the original:—"Sol lucem penitus amissurus eandemque recuperaturus, radios ejaculabat fluctuabundos, instar *auroræ borealis*, et rutilos."

‡ Acta Lit. et Scien. Suec., tom. iv., p. 61. The words of the observer are these:—

The three observations just cited are the only ones that have been found in the records of solar eclipses, confirmatory of the phenomenon remarked exclusively in France, during the total eclipse of 1842. The question naturally arises, to what physical cause are the singular undulations noticed on such occasions to be ascribed? It is reasonable enough to suppose that, towards the time of total obscuration, the equilibrium of the atmosphere is very much disturbed by the change of temperature consequent upon the rapid diminution of the visible segment of the solar disk. Hence will arise a sudden intermingling of strata of air of different densities, and consequently of different refractive powers, which may obviously tend to produce a succession of undulatory movements similar to those actually perceived. It is not improbable also that the continuous obstruction of the solar light by innumerable foreign substances floating in the atmosphere, and subject to incessant displacements from the agitation of the air, may contribute towards producing the observed effect. The extreme smallness of the solar crescent cannot fail to render the interposition of any such particles of matter, more readily perceptible than it would otherwise be. It is to be remarked also that the effects in all such cases will be magnified by diffraction. That the phenomenon is to a certain extent dependent on the operation of this principle, would seem to be indicated by the variety of colour which distinguishes the moveable shadows. Moreover, if light be propagated by means of undulations in a highly-elastic medium, it is not improbable that, as the origin of light rapidly diminishes in magnitude, causes of interference may arise leading to a conflict of undulatory movements, whence may ultimately result phenomena analogous to those alluded to in the foregoing observations.

A phenomenon which seems to be of a totally different nature from any of those hitherto alluded to, has been occasionally observed during solar eclipses, especially such as are of the annular species. In the latter case, previous to the formation of the annulus, the western limb of the moon exhibits an indented appearance, resembling a *succession of beads*. Almost immediately the beads become elongated, assuming the aspect of *long, black, parallel streaks*, uniting the limbs of the sun and moon. In the next instant these lines give way, as if they had been snapped asunder by the eastward motion of the moon, and the annulus then appears completely formed. A similar phenomenon ensues in a reverse order, when the eastern limb of the moon is approaching the sun's limb, previous to the dissolution of the annulus. At first several dark lines dart forward from the moon's limb, and unite themselves to that of the sun; then they suddenly contract so as to assume the appearance of beads, and immediately afterwards the annulus is dissolved. On the occasion of the annular eclipse of May 15, 1836, this phenomenon was especially observed in Scotland by Mr. Baily, who has given a lively description of it in a paper on the eclipse which appears in the tenth volume of the *Memoirs of the Astronomical Society.* It may not be uninteresting, however, to show that an appearance more or less analogous to it has been witnessed on several previous occasions, both of total and annular eclipses.

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* "Par le plus grand hasard du monde."

† *Mémoires pour servir à l'Histoire et au Progrès de l'Astronomie*, p. 258, 4to., St. Petersbourg, 1738. M. Arago states that having searched, page by page, the volumes of the various academic collections, he found *only one observation* corroborative of those made in France relative to the undulations which occurred on the occasion of the total eclipse of 1842; but that he was unable to cite it in detail, in consequence of the loose leaf, to which he had consigned the memorandum of it, having been mislaid. (*Annuaire*, 1846, p. 399.) It is not probable that either of the two Swedish observations cited in the text was that which fell under his eye; since, if he had met with the one, the other could hardly have escaped him. It is evident, at any rate, that the observation cited by Delisle, in a work which *does not form part of any academic collection*, cannot be the one referred to by the illustrious philosopher.

Halley mentions, in his account of the total eclipse of 1715, that about two minutes before the total immersion the sun was reduced to a very slender crescent of light, the extremities of which seemed to lose their acuteness and to *become round like stars* *.

During the total eclipse of 1724, Delisle remarked that the part of the moon's limb, where the total immersion was about to take place, appeared to him *irregular and indented* †.

But, as has been already mentioned, it is during *annular* eclipses of the sun, that the phenomenon witnessed by Mr. Baily is more especially conspicuous.

In his account of the annular eclipse of 1737, Maclaurin cites a letter addressed by Professor Bayne to Lord Aberdour, containing the following statement:—"What appeared to me *most entertaining, considered as an object of sight,* was when the extremities of the horns, formed upon the face of the sun, seemed as if they had been in the action of uniting their points; the inequalities on the extremity of the moon's disk gave the appearance *as it were of small bodies in particular motion* ‡.

In Short's paper on the annular eclipse of 1748, which has been already referred to, there appears a statement from the Rev. Mr. Irvine, who observed the eclipse at Elgin, in which he asserts that, previous to the joining of the cusps of the sun, as also at the breaking up of the annulus, *he perceived a quick tremulous motion and several irregular bright spots between the cusps,* which disappeared in a few moments. He adds that *both the formation and breaking* (of the annulus) were very sensibly to be observed and parted in a moment; affording a very pleasant sight, by the *irregular tremulous spots of the sun* §.

The following description of the phenomenon as witnessed in America by Mr. S. Webber, during the annular eclipse of April 3, 1791, is more precise than any that has been hitherto cited:—"At both the internal contacts there was a curious and striking appearance of what may be called drops, on account of their resemblance to drops of a fluid. At the first contact, when the horns of the sun were forming a ring about the moon, these luminous drops suddenly appeared, at several different points, with very little difference of time. At first they were nearly circular; but they rapidly extended themselves along the limb of the sun, till, uniting, they completed the annulus. At the second contact several breaches in the annulus almost instantaneously succeeded the first, at different distances from each other; and the oblong drops included between them contracted and vanished."||

A similar phenomenon was witnessed by Nicolai at Manheim, during

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[*] Phil. Trans., 1715, p. 248.

[†] Mém. Acad. des Sciences, 1724, p. 317.

the annular eclipse of September 7, 1820. In his account of the eclipse, he states that about a second before the annulus was formed, the fine curve of the moon's disk, then immediately in contact with the edge of the sun, appeared broken into several parts; *and in a moment these parts flowed together like drops of water or quicksilver near each other.* At the dissolution of the annulus, a similar appearance presented itself; for the delicate thread of light then formed by the annulus, instead of being broken into one place only, was in an instant divided into several places at once."*

The phenomenon was also observed on the occasion of the same eclipse at Bologna, by De Zach, who has given the following interesting description of it:—"The most beautiful spectacle was the end of the annular eclipse or the instant of the formation of the annulus. The mountains of the moon appeared very distinct. Her limb seemed to be indented, and when it was upon the point of touching the sun's limb, it appeared like a comb or a saw biting the edge of the solar disk. Before the contact of the two limbs was effected, there was visible, not a continuous thread of light, but a number of luminous points, resembling a row of so many pearl beads, separated by dark intervals. This beautiful phenomenon lasted only an instant; for the contact of the limbs, and the total disappearance of the last trace of light, was instantaneous."†

The same appearance was also witnessed at Amsterdam by several observers of the eclipse, accompanied in some instances with new features, but the description given by Mr. Baily, of the phenomenon as it presented itself to him on the occasion of the annular eclipse of May 15, 1836, embodies all the particulars by which it has been hitherto distinguished. He states that when the cusps of the sun were about 40° asunder, a row of lucid points, like a string of bright beads, irregular in size and distance from each other, suddenly formed round that part of the circumference of the moon that was about to enter on the sun's disk. Its formation was so rapid that it presented the appearance of having been caused by the ignition of a fine train of gunpowder. Mr. Baily was under the impression that this phenomenon indicated the correct formation of the annulus, and he was accordingly about to note the time of its occurrence. "My surprise, however, was great," says he, "on finding that these luminous points, as well as the dark intervening spaces, increased in magnitude, some of the contiguous ones appearing to run into each other, like drops of water; for the rapidity of the change was so great, and the singularity of the appearance so fascinating and attractive, that the mind was for the moment distracted and lost in the contemplation of the scene, so as to be unable to attend to every minute occurrence. Finally, as the moon pursued her course, these dark intervening spaces (which at their origin had the appearance of lunar mountains in high relief, and which still continued attached to the sun's border) were stretched out into long, black, thick, parallel lines, joining the limbs of the sun and moon; when all at once they *suddenly* gave way, and left the circumference of the sun and moon in those points, as in the rest, comparatively smooth and circular; and the moon perceptibly advanced on the face of the sun." In fact the phenomenon could not be more accurately imagined than by supposing, for the moment, that the edge of the moon was formed of some dark, glutinous substance, which by its tenacity adhered to certain points of the sun's limb, and by the motion of the moon was thus drawn out into long threads, which suddenly broke and wholly disappeared*.

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* Mem. Ast. Soc., vol. i., p. 142.

† Correspondance Astronomique, vol. iv., p. 171. Smyth's "Cycle of Celestia

Mr. Baily remarks that the scene so riveted his attention, that he could not take his eye away from the telescope to note down anything during the progress of the phenomenon. He estimated, however, that the whole took up six or eight seconds or ten at the utmost. The same phenomenon occurred in a reverse order at the dissolution of the annulus. While the limb of the moon was yet at some distance from the edge of the sun, a number of long, black, thick, parallel lines suddenly darted forward from the moon, and joined the two limbs as before. As these dark lines got shorter, the intervening bright parts assumed a more circular and irregular shape, and at length terminated in a fine curve line of bright beads, as at the commencement, till they ultimately vanished, and the annulus ceased to exist.

Mr. Baily also states that, although the parallel dark lines ought not to have exhibited any perceptible difference of length, the outer lines immediately before their rupture seemed to be nearly double the length of those in the middle. This circumstance manifestly indicated a protuberance on the moon's limb at the point where it had just entered upon the sun's disk. A similar phenomenon had been noticed on the occasions of the transit of Venus across the sun's disk in the last century.

The phenomenon of which Mr. Baily gave so vivid a description excited great interest among astronomers, and future opportunities of verifying it were anxiously looked forward to. The annular eclipse of Feb. 18, 1838, which was visible in the United States of North America, was peculiarly favourable for this purpose. On this occasion, however, the phenomenon was only partially observed. In several instances, the beads witnessed during previous annular eclipses at the instants of the formation and rupture of the annulus, were plainly visible, but no trace of the long black threads alluded to by Mr. Baily could be discerned by any observer. Moreover, it was found that even the beads were not perceptible in every case, their visibility seeming to be dependent on the colour of the screen glass employed in observing the sun. With a red glass the phenomenon was generally perceived; but, when a green glass was used, only a faint trace of it could be obtained, and in many cases it was altogether invisible. Thus Prof. Henry, who observed the eclipse at Princeton College, New Jersey, with a 3½ feet achromatic telescope, and a *red* screen glass, states that "an appearance similar to a row of beads was regarded as the formation of the ring." He adds that the drops endured only for a second or two†. On the other hand, Mr. R. T. Paine, who observed the eclipse at Washington with a telescope having a *green* glass applied to it, was unable to discern the slightest vestige of the phenomenon. "The ring," he remarks, "formed instantaneously, and broke nearly so. No beads were seen, nor the dark lines mentioned by Mr. Baily, nor the light round the moon, although all were looked for. No distortion of the moon's limb could be seen, and the cusps of the sun, before the ring formed, were as sharp as needles."‡

It was expected that the observations of the total eclipse of July 8,

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* Mem. Ast. Soc., vol. x., p. 7.

1842, would serve to throw some light upon this interesting phenomenon. On this occasion, however, astronomers were disappointed. In some instances a faint resemblance to beads of light was seen previous to the total immersion, but, generally speaking, the phenomenon was totally invisible. Even Mr. Baily, whose principal motive for proceeding to Italy was to verify, by means of this eclipse, his previous observations, was unable to discern any trace of the phenomenon.

The observations of the annular eclipse of October 9, 1847, afforded a partial verification of the phenomenon witnessed by Mr. Baily in 1836. Captain Jacob, who observed the eclipse at Bombay, states, that when the annulus was about being formed, the moon's limb seemed to be united with that of the sun by a dark ligament about 1′ in breadth, which became more elongated as the moon advanced upon the sun. The moon's limb was perfectly well defined, except at the part where the ligament was perceived. This phenomenon lasted about three or four seconds, when the ligament at length suddenly gave way, and the annulus was seen complete. The phenomenon exhibited at the dissolution of the annulus was somewhat different. When the moon's limb approached very close to that of the sun, a portion of it, about 30° in extent, *suddenly flowed over in dark lines*, leaving bright spaces between. This appearance lasted only about two seconds *.

The same eclipse was observed at several stations in Europe, but no decisive indication of the beads or dark lines which exhibited so striking an appearance during the annular eclipse of 1836, was obtained in any instance.

The question relative to the physical cause of the phenomena above mentioned has naturally excited considerable speculation, but it cannot be said that any definitive conclusion has yet been arrived at upon this head. That they are mainly illusions of an optical nature is generally admitted by all enquirers, but the *modus operandi* seems to be involved in much obscurity. The most probable hypothesis of their origin is that which refers them to the influence of irradiation. The enlargement of the apparent magnitudes of the celestial bodies, arising from this cause, has been already alluded to. The observations of solar eclipses tend especially to corroborate the truth of this remark. It has been already mentioned that Kepler adduced the appearance presented during the solar eclipse of 1590, as affording an obvious proof of the influence of irradiation. The *extraordinary agility* with which, according to Wyberd, the moon threw herself upon the solar disk on the occasion of the total eclipse of March 29, 1652, can only be accounted for by supposing that the sun was fringed with a border of spurious light which disappeared *all at once* as soon as the last portion of his limb was immersed. In his account of the annular eclipse of Feb. 18, 1737, Maclaurin states that the breadth of the annulus appeared much greater to the naked eye than could have been expected from the difference of the semi-diameters of the sun and moon. "This appearance," he remarks, "proceeded chiefly, I suppose, from the light encroaching on the shade, as is usual; but whatever was the cause, everybody seemed surprised that the moon appeared so small upon the disk of the sun."†

The enlargement produced by irradiation is also indicated by a remark of Mr. Fullarton, who, in a description of the same eclipse as it appeared at Crosby, near Ayr, states that the annulus was of a uniform breadth, during the greater part of the time of its continuance, but *seemed to go off very suddenly;* so that when the disk of the moon approached the concave line of the sun's disk, they seemed to run together like two contiguous drops of water on a table, when they touch one another."* A similar remark was made by the Rev. Mr. Irvine, of Elgin, relative to the annular eclipse of 1748. In his account of the eclipse, as cited by Short, he states that the moon's body seemed *to pass quicker about the time of the annulus,* than at any other time during the eclipse†.

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* Monthly Proc. Ast. Soc., vol. viii., p. 27.

† Phil. Trans. 1737, p. 183.

The observations of solar eclipses in more recent times have, in general, afforded similar indications of an enlargement of the apparent magnitude of the sun from the irradiation of his light.

The *dark ligament* observed by Capt. Jacob during the annular eclipse of October 9, 1847, is obviously an effect of irradiation, for when the moon is wholly within the solar disk, except at the point of interior contact where it still projects in a very small degree over the sun's limb, the enlargement produced by irradiation will be *wholly* wanting at this point, whereas, on the other hand, the rest of the limb being uncovered by the moon will exhibit its usual appearance. The sudden deficiency of light arising from this cause will manifestly produce an appearance resembling a dark ligament or neck, uniting the limbs of the sun and moon at the point where the contact of the two bodies is about to take place‡.

Prof. Powell's explanation of the *beads and threads* witnessed at the formation and dissolution of the annulus, is perhaps less liable to objection than any other that has hitherto been proposed. It is based upon two principles, the real existence of which has been established beyond doubt by observation and experiment. These are, the rapid degradation of the sun's light towards the margin of his disk, and the law of irradiation in virtue of which the intensity of its influence increases with the brightness of the luminous object. Admitting these two principles, it is easy to see that a series of fissures in the moon's limb will give rise to so many patches of light, which cannot fail to acquire a more elongated appearance as the moon advances upon the solar disk.

It is to the inferior brightness of the sun towards the margin of his disk, and the consequent diminution of the effect produced by his irradiation, that Prof. Powell ascribes the *protuberance on the moon's limb* towards the point of her interior contact with the sun, observed by Mr. Baily during the annular eclipse of 1836, when the ring was just formed, and when it was about to be dissolved. In this case it is clear that the solar light, being less intense towards the point of contact than at the other parts where it borders upon the moon's limb, will encroach less by irradiation upon the dark body of the moon than elsewhere; and that hence will arise an apparent protuberance of the contour of the lunar disk, which will be most sensible at the point where the limbs of the sun and moon are nearest to each other§.

The foregoing modes of explaining the phenomena observed during annular eclipses are not wanting in plausibility, but they cannot be said to afford a satisfactory account of all the observations. A difficulty which attends these, as well as all other explanations of the same nature, consists in the fact of the phenomena being visible in some cases and not in others, although sought for with the utmost care. In some instances the absence of the phenomena may be explained in accordance with the above-mentioned principles. Thus, in the case wherein they have not been seen, when the image of the sun has been received upon a screen, it may justly be supposed that this circumstance arose from the feeble irradiation of the solar image, whose brightness is so much inferior to that of the actual surface of the sun. In the great majority of cases, however, no explanation can be assigned, which is capable of reconciling the conflicting statements of different observers.

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* Phil. Trans., 1737, p. 183.

† Phil. Trans., 1748, p. 594.

‡ Lalande was the first who gave this explanation of the dark ligament.

Eclipses of the moon, although neither so impressive in their nature, nor so important in their results as those of the sun, attracted the attention of astronomers at a very early age, as in the absence of more refined methods of observation, they served to fix the moon's place in the heavens with considerable precision. Hipparchus did not fail to perceive that observations of lunar eclipses would furnish a solution of the great problem of the longitude. It was accordingly by means of such phenomena, that the difference between the meridians of places on the earth's surface was ascertained during a long succession of ages. The results so obtained were, however, very inaccurate, on account of the difficulty which is experienced in determining, by observation, the exact time of the occurrence of a lunar eclipse. The method was therefore speedily abandoned in modern times, upon the invention of others of a more trustworthy nature. Ptolemy, in his great work the *Syntaxis*, has determined the elements of the lunar orbit by means of observation of eclipses of the moon, made by the Chaldeans, at Babylon, about 700 years before the commencement of the Christian era. It has been already mentioned that, by a comparison of the same eclipses with others observed in more recent times, Halley was led to suspect the famous inequality of the secular acceleration of the moon's mean motion.

Observations of lunar eclipses are chiefly interesting in modern times on account of the physical peculiarities which they occasionally exhibit. The *ruddy hue*, by means of which the moon is rendered visible, even when she is totally immersed in the earth's shadow, was a phenomenon, the cause of which was long involved in great obscurity. By some persons it was supposed to arise from a light naturally inherent in the moon's surface, but it seemed impossible to reconcile this explanation with the appearance of the moon in other parts of her orbit. The illustrious Kepler was the first who shewed that the phenomenon was occasioned by the refraction of the earth's atmosphere, which had the effect of turning the course of the solar rays passing through it so as to fall upon the moon even when the earth was actually interposed between her and the sun *.

The deep red colour of the moon's surface arises from the absorption of the blue rays of light in passing through the terrestrial atmosphere, in the same manner as the western sky is seen frequently to assume a ruddy hue when illuminated in the evening by the solar rays. On account of the variable condition of the terrestrial atmosphere, the quantity of light which is actually transmitted through it to the moon is liable to fluctuate considerably, and hence arises a corresponding variation in the appearance presented by the moon's surface during her immersion in the earth's shadow. If the region of the atmosphere through which the solar rays pass, be everywhere deeply loaded with vapours, the red rays will be almost totally absorbed as well as the blue, and the illumination of the moon will be too feeble to render her surface visible. The records of lunar eclipses present several examples of this kind. Thus Kepler states that during the lunar eclipse which happened on the 15th of June, 1620, the moon totally disappeared, although the fixed stars of the fourth and fifth magnitudes in her immediate vicinity were plainly distinguishable *. Hevelius also mentions that during the lunar eclipse of April 25, 1642, the place of the moon could not be ascertained even with the aid of the telescope, although the air was sufficiently pure to discern the stars of the fifth magnitude †.

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* Ad Vitellionem Paralipomena, p. 276.

The account given by Wargentin, of the lunar eclipse of May 18, 1761, as observed by him at Stockholm, furnishes an interesting illustration of the same remark. The total immersion of the moon took place at 10^h 41^m P.M. The part of the margin of the lunar disk which had last entered the shadow was pretty conspicuous by means of a bright light for five or six minutes after the immersion, and to the naked eye exhibited a lustre equal to that of a star of the second magnitude; but at 10^h 52^m, this part, as well as the whole of the rest of the moon's body, *disappeared so completely, that not the slightest trace of any portion of the lunar disk could be discerned either with the naked eye or with the telescope*, although the sky was very clear and the stars in the vicinity of the moon were distinctly visible in the telescope. After a long search for the missing body of the moon, the Swedish astronomer at length discovered it at 11^h 30^m with a two-feet telescope, by means of a faint light which was visible about the eastern border of the disk. At 11^h 33^m, persons of acute vision were able to discern a trace of it with the naked eye, like an exclusively thin vapour; but more than half of the disk was still invisible. The boundary between the visible and invisible portions was very irregular; the light was also more intense at the margin of the moon than it was towards the centre. During the progress of the emersion, which commenced at 12^h 15^m, the part of the moon which was still involved in the earth's shadow was by no means palpable to observation, as is usually the case on similar occasions ‡.

If the region of the atmosphere, through which the solar rays pass, be everywhere very transparent, the red rays will be transmitted to the moon in great abundance, and her surface will, in consequence, appear strongly illuminated. Thus, during the lunar eclipse of December 22, 1703, the moon, when totally immersed in the earth's shadow, was visible at Avignon by a ruddy light of such brilliancy, that one might have imagined her body to be transparent, and to be enlightened by the sun from behind §. A similar appearance was remarked on the occasion of the total eclipse of the moon, which happened on the 19th of March, 1848. Mr. Forster, of Bruges, states, in an account of the eclipse, that *during the totality of the immersion, the light and dark places on the face of the moon could be almost as well made out as in an ordinary dull moonlight night.* So intense was the illumination of the moon's surface, that many persons could not be persuaded that she really was eclipsed ‖. Mr. Walkey, who observed the eclipse at Clyst St. Lawrence, near Collumpton, states that the appearance was as usual until 20 minutes to nine, when the whole of the moon's surface became very quickly and very beautifully illuminated, so as to resemble the glowing heat of fire from the furnace, rather tinged with a deep red. This appearance lasted about an hour ¶.

Sometimes it happens that the region of the terrestrial atmosphere through which the solar rays pass, is so loaded with vapour in some parts as in a great degree to absorb the rays, while in other parts it allows them to be transmitted with facility to the moon's surface. Hence during the totality of a lunar eclipse, the moon's surface will appear quite obscure in some parts, and in other parts will exhibit a high degree of illumination. Thus Kepler states that during the total eclipse of the moon, which happened on the 16th of August, 1598, while one-half of her disk was seen only with great difficulty, the other half was discernible by a deep red light of such brilliancy, that at first he was doubtful whether it was immersed in the shadow at all*. If dense clouds be interspersed throughout the whole of the atmospheric zone through which the rays pass, there will manifestly ensue an irregular distribution of light and darkness over the whole of the moon's surface. Such was the appearance, which, according to Sir John Herschel, the moon presented during the total eclipse of October 13, 1837†.

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* Ep. Ast., p. 825. † Selenographia, p. 117. ‡ Ph. Tr., 1761, pt. i., p. 208, et seq.
§ Mém Acad. des Sciences, 1704, p. 59. (Hist.)

The phenomena presented by the *transits of the inferior planets* over the sun's disk are among the most interesting that arise from the relative movements of the various bodies composing the solar system. The importance of the transits of Venus, in determining the solar parallax, has been already referred to. The physical features of such phenomena tend also to attract the attention of the enquirer. The transits of Mercury are of less utility than those of Venus in ascertaining the absolute dimensions of the solar system; but, as in the case of that planet, the phenomena accompanying such occurrences are calculated to throw some light on the physics of the celestial regions.

Long before the invention of the telescope it was asserted that Mercury had been seen upon the sun's disk. Even as early as the ninth century, it was stated by the author of the "Life of Charlemagne" that the planet had been visible upon the sun for eight days. As it was impossible, from the rapidity of his orbitual motion, that Mercury could have remained so long visible upon the sun, Kepler suggested that the expression *octo dies*, or eight days, might have been erroneously substituted for the barbaric Latin *octoties*, or eight times. It is now well known, however, that the angular magnitude of the planet is by far too small to allow its being seen with the naked eye on such an occasion, so that in all probability the phenomenon referred to was no other than a solar spot of more than ordinary magnitude. Coming down to a more recent age, Averroès, a famous physician of Cordova, in Spain, who wrote a commentary on the Almagest, positively asserted that he had seen Mercury on the sun at a time when, from the motion of the planet, it ought to have been passing through its inferior conjunction‡. This statement, however, must be rejected as unworthy of belief, for the same reason as that alluded to in the case just cited. Kepler himself was induced to believe on one occasion that he had seen the planet on the sun, but the invention of the telescope a few years afterwards having speedily led to the discovery of spots on the sun's disk, he retracted his opinion, and candidly acknowledged that the phenomenon seen by him was, in all probability, no other than one of such *maculæ*.

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says Mr. Walkey, "and during that period I have several times beheld an eclipse of the moon, but never before did my eyes behold the moon positively give *good light* from her disk during a total eclipse."

* Ad Vitellionem Paralipomena, p. 276.

† Outlines of Astronomy, p. 256.

In his "Supplement to Vitellionem," Kepler remarked that from the position of Mercury's nodes, it was possible that he might frequently pass over the sun's disk*. With respect to Venus, he asserted that although she might sometimes be interposed directly between the earth and sun, the instances of such an occurrence were very rare. He announced, in fact, that a transit of this planet would not take place during the whole of the seventeenth century†.

The completion of the Rudolphine Tables in the year 1627, having enabled their illustrious author to calculate the motions of the inferior planets upon a more accurate basis than any hitherto employed by him, he now arrived at results of considerable precision relating to the times of their transits over the sun's disk. In 1629 he published a small tract, in which he ventured to predict that both Mercury and Venus would pass over the sun's disk in the year 1631, the former on the 7th of November, and the latter on the 6th of December‡. He announced at the same time that there would not happen another transit of Venus before the year 1761. This, however, was a mistake, as we shall presently have occasion to show more particularly.

The transit of Mercury, which Kepler had predicted, was actually witnessed by Gassendi, at Paris. He has given an interesting account of his observations in a letter to Shickhard, Professor of Mathematics in the University of Tubingen. "The crafty god," says he, "had sought to deceive astronomers by passing over the sun a little earlier than was expected, and had drawn a veil of dark clouds over the earth in order to make his escape more effectual. But Apollo, acquainted with his knavish tricks from his infancy, would not allow him to pass altogether unnoticed. To be brief, I have been more fortunate than those hunters after Mercury who have sought the cunning god in the sun. I found him out, and saw him, where no one else had hitherto seen him."§

Gassendi made preparations to observe the transit of the planet, by admitting the solar light into a dark chamber through a small aperture in the window, and receiving the image of the sun upon a white screen, having a circle described upon it adapted exactly to the magnitude of the image. The diameter of the circle was three-fourths of a French foot in length, and was divided into sixty parts, so that by supposing the apparent diameter of the sun to be 30′, each of the subdivisions would represent an apparent magnitude of 30″. The circumference of the circle was also divided into 360 degrees. In order to obtain an accurate knowledge of the time, he placed a person in the room above him, with a quadrant of two feet radius, charging him to observe the altitude of the sun whenever he should hear him stamp with his foot. In order that the transit of the planet might not escape him, he resolved to commence his observations a day or two earlier than that which Kepler had fixed for its occurrence. The 5th of November proved unsuitable for this purpose, being all day rainy. The 6th turned out to be equally unfavourable, the sky being overcast during almost the whole day. The 7th, which was the day appointed for the transit of the planet, was very changeable in the morning, but was for most part cloudy. A little before eight o'clock the sun was visible through the openings of the denser clouds, but a thin veil of nebulous matter still rendered it impossible to distinguish any minute object, either upon the actual disk of the sun or upon his image in the dark chamber. Towards nine o'clock, the sun being distinctly visible, he perceived a *black spot* upon the image. It was a little to the east of the vertical diameter, and about a fourth of its length from the lower extremity. He roughly marked its position, not having the remotest suspicion that it was Mercury on account of its extreme smallness, for its diameter scarcely seemed to exceed the half of one of the minute parts into which he had divided the diameter of the white circle. He was rather inclined to believe that it was a solar spot, which, although not visible on the preceding day, might have been formed during the intermediate interval, as had happened on several previous occasions, in the course of his experience. The sun having again appeared through the clouds at nine o'clock, he measured the distance of the spot from the centre of the image, intending to compare its position with that of the planet, if the latter should eventually enter upon the sun, for it occurred to him that an observer at some other station might be induced to do so likewise, and that suitable data would thereby be obtained for determining the parallax of the planet. A little afterwards, when the sun again became visible, he measured anew the distance of the spot from the centre of the image, and was surprised to find that it had *considerably increased* during the intermediate interval. From its rapid motion, he felt assured that it could not be one of those ordinary spots that appear from time to time on the surface of the sun, and he now began to entertain some suspicion of its real nature, but still he could not persuade himself that it was Mercury, so much was his mind pre-occupied with the idea that the planet would exhibit a larger apparent magnitude. While he was pondering whether he had not made a mistake in his first measurement, the sun shone forth, whereupon, having again ascertained the position of the spot, he found its distance from the centre to be *even greater* than on the previous occasion. He now concluded without hesitation, that it was in reality the planet which the black spot represented. He therefore immediately gave the preconcerted signal to his assistant in the chamber above him, but unfortunately he had abandoned his post, and for some time could not be found. He returned, however, before the planet had gone off the sun, and made the necessary observations.

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* Ad Vitellionem Paralipomena, p. 306. † Ibid, p. 305.

‡ The following is the title of this small tract:—" Admonitio ad Astronomos rerumque celestium studiosos, de miris rarisque anni 1631 phænomenis, Veneris puta et Mercurii in solem incursu." *Lips.*, 1629.

§ Opera Omnia, tom. ii., p. 537.

Gassendi states that the planet, as seen depicted on the solar image, was somewhat diluted, and of a ruddy colour around the margin. Its diameter seemed to be equal to about two-thirds of the interval between two consecutive points of the diameter of the image. The apparent diameter of the planet was therefore only 20″. This was far below the angular magnitude which astronomers usually assigned to it.

The planet had nearly gone off the sun when Gassendi first saw it. From its observed motion during the short interval of its visibility, he calculated that it had entered upon the sun's disk at 5^h 28^m A.M., and that its final egress took place at 10^h 28^m A.M. The transit had therefore lasted five hours precisely. By reducing the calculations of Kepler to the meridian of Paris, he found that the observed time of the transit was in advance of the computed time by 4^h 49^m 30^s.

The transit of Mercury observed by Gassendi, took place when the planet was passing through the ascending node of its orbit. The second transit of the same planet that was actually observed, happened on November 3, 1651. On this occasion also the planet was passing through its ascending node*. The phenomenon was observed at Surat in India, by Shakerley, a young Englishman, who, having found by calculation that it would be visible only in Asia, proceeded to India for the express purpose of witnessing its occurrence†.

The third transit of Mercury recorded by astronomers, happened on the 3rd of May, 1661. Hevelius, who observed it at Dantzic, was astonished to find that the apparent diameter of the planet was so small. We have seen that Gassendi estimated it to be equal to 20″; Hevelius made its greatest value to amount only to 12″, and the mean value to 6″ 3‴‡. These results agree very nearly with the modern determinations of the apparent magnitude of the planet. The same transit was observed at London by Huyghens, Street, and Mercator§.

The fourth transit of Mercury that is recorded to have been observed, was that of November 7, 1677. It was witnessed by Halley at St. Helena, where he was then residing, as well as by several persons in Europe. Halley was the first who observed both the ingress and egress of the planet.

The transits of Mercury that have been subsequently observed, are chiefly interesting on account of the indications they afford of the accuracy of the existing tables of the planet. With respect to the physical phenomena accompanying such occurrences, they are not of such importance as might be expected. It is only on a few rare occasions, indeed, that any peculiarity worthy of notice has been seen. In each of such instances, the phenomenon is manifestly referrible to some optical illusion. A brief description of the appearances observed during two or three transits of the planet will serve to confirm the truth of this remark.

In an account of the transit of Mercury which happened on the 6th of May, 1753, M. De Barros, a Portuguese, who witnessed the phenomenon at Paris, states that when the planet was about to leave the sun's disk, he perceived the interior contact through a green glass placed before the smoked glass of his telescope (which was an excellent Gregorian reflector, four feet long), and that immediately afterwards, viewing the planet through the smoked glass alone, he found that a small thread of light was still visible between the limbs of the two bodies. A second interior contact of the planet was therefore observed at the commencement of the egress, which did not take place till four seconds after the first contact. A similar appearance presented itself at the completion of the egress. The exterior contact was first observed by means of the two glasses; but upon removing the green glass, the planet was again brought upon the sun's disk, and did not go off until six or seven seconds afterwards‖.

The phenomena above referred to are, in all probability, connected with the irradiation of light. It has been already mentioned that the colour of the glass employed in viewing a luminous object, exercises a material influence on the enlargement arising from this cause. The observation of De Barros was called in question by several of his contemporaries, but when the extraordinary phenomena of a similar nature witnessed in more recent times are taken into consideration, there seems no reason to doubt the authenticity of his statement.

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* The transits of Mercury happen necessarily, from the heliocentric position of its nodes, always in May or November. When the transit takes place in May, the planet is passing through the *descending node* of its orbit. The occurrence of the transit in November indicates the passage of the planet through the *ascending node*.

† Wing, *Astronomia Britannica*, p. 312.

‡ "Mercurius in Sole Visus." Gedan, 1662, p. 92.

§ Wing, *Astronomia Britannica*, p. 312. This transit happened on the day of the coronation of Charles II. The observations of Huyghens and his companions are said by Wing to have been made at Long Acre, with a telescope of excellent workmanship.

‖ Phil. Trans., 1753, p. 362.

Plantade remarked that, during the transit of 1736, the disk of Mercury, when projected upon the sun, appeared to be surrounded by a luminous ring. A similar phenomenon has been witnessed during several subsequent transits. MM. Schroeter and Harding, who observed it on the occasion of the transit of 1799, describe it as a nebulous ring of a dark tinge approaching to violet colour*. A ring agreeing with this description was noticed by Dr. Moll, of Utrecht, during the transit of 1832†. On the other hand, many persons who observed the transits above mentioned, did not perceive any indications of a ring around the planet, nor have the observations of more recent transits of the planet served to confirm the existence of such a phenomenon. It is therefore very probably a spurious appearance depending upon some optical cause.

A curious phenomenon noticed by Schroeter and Harding on the occasion of the transit of Mercury which occurred May 7, 1799, consisted in the appearance of two small spots of a greyish colour on the disk of the planet‡. A similar phenomenon was witnessed by Dr. Moll during the transit of 1832, except that in this instance only one spot was visible§. During the transit of 1848, a spot was also seen on the planet's disk by the Rev. J. B. Reade and Mr. Dell. It is described as a greyish spot, shading off indefinitely on all sides from the centre‖. It appears, also, from a statement made by Captain Sir Edward Belcher, R.N., to Prof. Powell, that it was seen in Sir James South's great refracting telescope, the aperture of which (11.95 inches) *had been reduced by a diaphragm outside to 3 inches*¶.

Prof. Powell has proposed an ingenious explanation of the above-mentioned phenomenon, founded upon the principle of the diffraction of light**. It is a consequence naturally flowing from the theory of the diffraction of the object glass of a telescope, as explained by Mr. Airy, that a small opaque disk, projected upon a luminous surface, should not only undergo a diminution of apparent magnitude, but should also exhibit one or more bright concentric rings in the interior, and if the disk be very small, it is not difficult to conceive that the rings might condense into a bright spot in the centre. Prof. Powell verified this result experimentally, by viewing a small opaque disk, on a ground glass, strongly illuminated behind, with a telescope having its aperture reduced to one-fourth of an inch. Within the disk he distinctly perceived a ring concentric with it, which contracted into a spot in the centre when the disk was very small.

Nothing can be more satisfactory than the agreement between the result of the above-mentioned experiment and the phenomenon observed during the transit of 1848. So far the explanation of Prof. Powell appears to be unexceptionable. Unfortunately, the same degree of consistency does not present itself when the question relates to the phenomenon of a similar nature witnessed on previous occasions of the transit of Mercury. Thus, although Dr. Moll saw a small spot on the disk of the planet during the transit of 1832, it is quite clear from the drawing he has given of its position, as well as from the terms in which he alludes to it, that it was not situate in the centre of the disk. The explanation of Prof. Powell, however, requires that the spot should be *exactly in the centre.* Again, we have seen that, on the occasion of the transit of 1799, two spots were seen on the disk of the planet, but, according to the diffraction theory, there should be *only one* visible. It is manifest that the observations of future transits of the planet can alone throw additional light upon the subject.

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* Mem. Ast. Soc., vol. vi. p., 116.

† Ibid.

‡ Ibid.

§ Ibid.

‖ Monthly Proc. Ast. Soc., vol. ix., p. 23.

¶ Mem. Ast. Soc., vol. xviii., p. 88.

** Ibid., p. 87.

On the occasion of the transit of Mercury in November, 1848, the limb of the planet was seen by one of the observers at Greenwich to be connected for some time with the sun's limb by black lines, similar to those noticed by Mr. Baily during the annular eclipse of 1836. It is worthy of remark, however, that although seven other individuals observed the transit with separate telescopes, no such phenomenon was seen by either of them*. These dark lines seem much more difficult to explain than either the dark ligament or the protuberance observable when the moon or planet appears upon the solar disk wholly detached from the sun's limb†.

The transits of Venus across the sun's disk are phenomena of much rarer occurrence than those of Mercury. It has been already mentioned, that in 1629, Kepler predicted the occurrence of a transit of this planet on Dec. 6, 1631. According to his calculation, it appeared that the planet would not enter upon the sun's disk till towards sunset; it was not improbable, therefore, that the phenomenon would be altogether invisible in Europe. Gassendi, however, was not without hopes that in the present instance, as had previously happened with respect to Mercury, there might be a considerable error in the calculated result, and that in reality the planet would begin to be visible on the sun's disk several hours earlier than the time assigned by the tables. He accordingly made arrangements to observe the transit similar to those which he had employed so successfully in the previous month on the occasion of the transit of Mercury. He has given an account of his fruitless observations on this occasion in a second letter to Schickhard. He intended to watch the appearance of the sun on the 4th and 5th of the month, but an impetuous storm of wind and rain rendered the face of the heavens invisible on both of those days. On the 6th he continued to obtain occasional glimpses of the sun, till a little past three o'clock in the afternoon, but no indication of the planet could be discerned upon his disk as depicted upon the white circle. On the 7th, he saw the sun during the whole forenoon, but he looked in vain for any trace of the planet. It is now well known that the transit of the planet took place during the night between the 6th and 7th of December‡.

It has been mentioned that Kepler had announced that, after the transit of 1631, Venus would not again be seen upon the sun's disk previous to the year 1761. Astronomers were therefore under the impression, after the failure of Gassendi's attempt to obtain a view of the planet during the transit of 1631, that the existing generation of the human race would long have passed away before another occasion would present itself of observing a phenomenon of so interesting a nature. In the year 1639, however, the planet passed again over the sun's disk. On this occasion the transit took place unknown to any person living, with the exception of two individuals who enjoyed the gratification of witnessing the phenomenon. The fortunate observers were Jeremiah Horrocks and William Crabtree, two young men residing in the north of England, devoted enthusiastically to the study of astronomy. Horrocks has given an account of the transit of the planet, as seen by himself and his friend Crabtree, in an interesting little dissertation on the subject entitled "Venus in Sole visa." He had been engaged in calculating the places of the planets by means of Lansberg's tables, which their author had boasted to be unequalled in point of accuracy; but, on comparing the results with observation, he was mortified to find that the discordances were of such magnitude, as to render his labours almost of no value. This circumstance calls forth the indignation of the young astronomer, who contrasts the tumid arrogance of the Belgian calculator with the unobtrusive merits of the illustrious Kepler. Lansberg, with as little modesty as truth, had vauntingly cited the words of the Roman poet,—

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* Monthly Proc. Ast. Soc., vol. ix., p. 69.

† Dr. Foster, who observed this transit at Bruges, states that the planet, when seen on the sun, had rather the appearance of a *globe* than a disk. (*Monthly Proc. Ast. Soc.*, vol. ix., p. 4.) A similar remark has been made on the occasion of solar eclipses, see p. 401, (*Note*).

‡ The two letters of Gassendi to Schickhard respecting the transits of Mercury and Venus were published under the title of "Mercurius in Sole Visus et Venus invisa." (Opera Omnia, tom. iv., p. 537, et seq.)

> "Quantum lenta solent inter viburna cupressi,"

as affording a just idea of the superiority of his tables over all other existing labours of a similar kind. Horrocks remarks, that however inappropriate the simile of the poet may be when so applied, it may be used very justly to represent the surpassing excellence of the Rudolphine Tables. Indeed, there could not be wanted a clearer indication of the genius of Horrocks than is afforded by the intuitive sagacity with which he seems to appreciate the value of Kepler's discoveries; for it is to be borne in mind that the prejudices in favour of the ancient system of astronomy had not yet been wholly eradicated from men's minds*.

It is a curious fact, however, that the tables of Lansberg, however erroneous, were instrumental in revealing to Horrocks the interesting fact that Venus would pass over the sun's disk in the month of December, 1639. He had found that Kepler's tables displaced the planet in latitude about 8′ towards the south, while on the other hand the tables of Lansberg indicated a much larger error in the opposite direction. During its conjunction with the sun in December, 1639, the planet appeared by Kepler's tables to pass a little *below* the sun, while, on the other hand, those of Lansberg brought it upon the upper part of his disk. He suspected, therefore, that the planet would actually pass over the solar disk towards its lower extremity, and a more complete investigation of the subject assured him of the accuracy of his surmise. By an exact calculation of the time of conjunction, he found, in fact, that the planet would enter upon the solar disk on the 24th of November, O.S., 1639, a little before sunset. Owing to the shortness of the interval that was to elapse previous to the actual occurrence of the transit, he was unable to announce with sufficient publicity the interesting result at which he arrived, so as to induce astronomers generally to observe the phenomenon. He did not fail, however, to acquaint his friend Crabtree, who lived in the neighbourhood of Manchester, with the approaching transit, and accordingly these two individuals made suitable preparations, each at his own residence, to observe its occurrence.

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* Montucla states, that Riccioli, Bouillaud, and many other celebrated astronomers, who were contemporary with them, read Kepler's works without comprehending them. (*Histoire des Mathématiques*, tom. ii., p. 254.)

Horrocks employed a mode of observation similar to that practised by Gassendi on the occasion of the transit of Mercury in 1631, as already explained. He divided the diameter of the white circle upon which he received the image of the sun into 30 parts, and each of these into 4 smaller parts, so that by supposing the apparent diameter of the sun to be 30′, each of the more minute subdivisions would represent an apparent magnitude of 15″. After rectifying the motion of the planet, he found that its conjunction would not take place before three o'clock in the afternoon of the 24th of November. However, as all the tables of the planet indicated the conjunction to be earlier—some of them even made it to take place on the 23rd—he did not consider it prudent to trust too implicitly to his own calculations. He therefore did not omit to examine attentively the image of the sun from time to time on the 23rd.

On the 24th he continued to observe the solar image from sunrise till the hour appointed for going to church*. During all this time, he saw nothing upon the sun except an ordinary spot of small dimensions which he had noticed on the preceding days, and which could not, therefore, be Venus. At a quarter past three o'clock in the afternoon, as soon as he was again at leisure, he proceeded to resume his observations. "At this time," says he, "an opening in the clouds, which rendered the sun distictly visible, seemed as if divine Providence encouraged my aspirations, *when, Oh most gratifying spectacle! the object of so many earnest wishes*†, *I perceived a new spot of unusual magnitude, and of a perfectly round form, that had just wholly entered upon the left limb of the sun, so that the margins of the sun and the spot coincided with each other, forming the angle of contact.*" The planet, in fact, had then entered upon the eastern limb of the sun, at the distance of 62° 30′ from the lower extremity of the vertical diameter of his disk. By a careful comparison of the relative magnitudes of the image and the round spot visible upon it, he concluded their diameters to be in the proportion of 30 to $1\frac{1}{6}$ or $1\frac{1}{5}$ at the utmost. The apparent diameter of the planet did not therefore exceed 1′ 12″.

Owing to the near approach of sunset, Horrocks was unable to observe the planet longer than half an hour. During this brief interval, he measured its distance from the sun three different times. His observations of the phenomenon were made at Hoole, a small village in Lancashire, about fifteen miles to the north of Liverpool.

Crabtree, who resided at Broughton near Manchester, had made arrangements for observing the transit similar to those employed by Horrocks. The sky, however, unfortunately continued overcast during the whole day, and he had abandoned all hopes of witnessing the phenomenon, when, just a little before disappearing below the horizon, the sun burst through the clouds. Repairing immediately to the chamber in which he had made preparations to receive the image of the sun, he beheld, to his unspeakable delight, the round black spot representing the planet depicted upon the white circle. According to Horrocks, he was so captivated by the spectacle, that he gazed upon it immovably for some time, and when he recovered himself, the clouds had again obscured the sun, so that he was unable to make any accurate measurements. He, however, drew a diagram of the position of the planet, which Horrocks found to agree exactly with his own observations. He estimated the diameter of the planet at $\frac{7}{100}$ths of the solar diameter, or about 1′ 3″, supposing with Horrocks that the latter was equal to 30′.

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* The words of Horrocks on this occasion, "*ad majora avocatus quæ utique ob hæc parerga negligi non decuit,*" manifestly have reference to religious duties, and this inference is further confirmed by the fact that the 24th of November, 1639, fell upon Sunday, as will be apparent to any person by a slight computation.

Thus did two young men, cultivating astronomy together in a state of almost complete seclusion in one of the northern counties of England, enjoy the privilege of witnessing a phenomenon which human eyes had never before beheld, and which no one was destined again to see until more than a hundred years had passed away[*]. Unfortunately, a premature death deprived their country of the two individuals who exhibited such enthusiasm in the cause of science. Horrocks was a young man of extraordinary genius, whose name would assuredly have formed a household word to future generations, if his career had not been so soon brought to a close. It may perhaps not be uninteresting to mention some of the few facts which are known respecting the two youthful astronomers.

Jeremiah Horrocks was born at Toxteth, near Liverpool, in the year 1619, of parents who appear to have been in rather straitened circumstances. Having received the rudiments of instruction at his native place, he subsequently completed his education at Emmanuel College, Cambridge. About the year 1633, he seems to have been first led to turn his attention to astronomical pursuits. In one of his posthumous fragments, he has described, with all the fervour of youthful enthusiasm[†], the state of his feelings at this time, and the means by which he succeeded in vanquishing the difficulties of his position; for it must be borne in mind that, in those days, there was no branch either of mathematical or physical science taught at Cambridge. "I felt great delight," says he, "in meditating upon the fame of the great masters of science, such as Tycho Brahé and Kepler, and sought at least to emulate them in my aspirations. I imagined that nothing could be nobler than to contemplate the manifold wisdom of my Creator amid so great a profusion of works; and to behold the pleasing variety of the celestial motions, the eclipses of the sun and moon, and other phenomena of the same kind, no longer with the unmeaning gaze of vulgar admiration, but with a desire to *know their causes*, and to feed upon their beauty by a closer inspection of their mechanism."[‡] Serious obstacles, however, opposed themselves to the realisation of these ideas by Horrocks. His humble condition in life was by no means favourable to the tranquil prosecution of astronomical researches. He had no teacher who could give him any instruction in the elements of the science, nor even a companion to cooperate with him in his studies. But these disadvantages did not effectually depress his ardour. He resolved to cultivate the science alone, by the aid of such books as his limited means could from time to time supply him with. About the year 1636, he at length had the good fortune to obtain the acquaintance of Crabtree, who had already been engaged in similar pursuits. The two young friends corresponded together for several years on astronomical subjects, occasionally communicating with Samuel Foster of London, who was subsequently Professor of Astronomy in Gresham College[*]. In a letter dated October 3, 1640, Horrocks stated to his friend that he intended soon paying him a visit, but he wished previously to bring to a conclusion his Dissertation on the transit of Venus. On the 12th of December following, he expressed in another letter his regret that the inconstant state of his affairs, and the daily performance of duties of a harassing nature, prevented him so long from enjoying the gratification of a personal interview with his friend. At length, on the 19th of the same month, he wrote to say that if nothing unusual should prevent him (*nisi quid præter insolitum impediat*), he would be at Broughton on the 4th of January. Alas! his expectations were not destined to be realised. Dr. Wallis found on the back of this letter a statement in Crabtree's own handwriting to the effect that his dear friend Horrocks died very suddenly on the morning of the 3rd of January, being the very day previous to that on which he intended to visit him. Crabtree survived his friend only a very short time. Dr. Wallis, in 1672, was unable to obtain any authentic particulars respecting his death. The general belief was, that he had perished in the civil wars which soon afterwards broke out.

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[*] The transits of Venus happen invariably either in June or December, according as the planet is passing through the *descending* or the *ascending* node of its orbit. The intervals between the successive transits counted in years are—8, 105½; 8, 121½; 8, 105½; &c., &c. Two transits of the planet at the ascending node will happen in the nineteenth century, one on Dec. 8, 1874, and another on Dec. 6, 1882. The transit of 1639, witnessed by Horrocks and Crabtree, is the only phenomenon of this kind that has been hitherto observed at the ascending node.

[†] "On voit que Horrocks était jeune et enthousiaste, mais cette jeunesse et cette enthousiasme annoncaient un homme vraiment distingué." (Delambre, Hist. Ast. Mod., tom. ii., p. 497.)

[‡] Opera Posthuma, p. 2.

Amid the angry din of political commotion, the name of Horrocks was completely forgotten, until at length, after the lapse of twenty years, the manuscript of his "Venus in Sole Visa" having been shown to several members of the Royal Society, which had just been instituted, a general feeling of admiration was excited by its perusal, and a strong desire was expressed that it should be published. Huyghens, who happened to be then in London, was so much struck with the genius of the youthful author of the Dissertation that he caused a copy of it to be taken, and transmitted it to Hevelius, who published it at Dantzic in 1662, along with his own "Mercurius in Sole Visus." It does not redound to the credit of England, that this exquisite relic of one of her most gifted sons should have been allowed first to see the light in a foreign land[†]. Nor can it assuredly be urged, in extenuation of her indifference, that its author, less fortunate than some of his successors, was struck down by the remorseless arm of death at the very commencement of his brilliant career.

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[*] It appears from Ward's "Lives of the Professors of Gresham College," that Samuel Foster completed his education at Emmanuel College, Cambridge, so that in all probability he was a fellow student of Horrocks.

[†] In fact it has never since been published anywhere else. Delambre expresses his astonishment at not finding it in the work edited by Wallis. A short postscript to the preface, however, serves to explain the omission. It is stated therein that Flamsteed purposed soon publishing a new and more correct edition of the tract "Venus in Sole Visa," from a manuscript in the author's own handwriting. It is well known that Flamsteed never fulfilled the expectation thus held out respecting him. In the list of MSS. of Flamsteed's, preserved in the Royal Observatory of Greenwich, and inserted at the beginning of Baily's "Life of Flamsteed," there is mention of a copy of Horrocks' "Venus in Sole Visa," which, in all probability, is the very copy alluded to by Wallis. If such be the case, it would be only paying a just tribute to the memory of the author to

Having thus been made acquainted with the loss which their country had sustained from the premature death of Horrocks, the Royal Society took steps to collect together all the memorials respecting himself and his friend Crabtree, which might still be in existence, with a view to their publication. It was found that much of what Horrocks had written was irrecoverably lost. Some of his papers, which, for greater security, had been kept in a secret place during the civil wars, were burned by a party of soldiers who entered his father's house in quest of plunder. Some were carried to Ireland by his brother, who died there and was no more heard of. Another portion, which had been deposited in a bookseller's shop in London, was destroyed during the great fire of 1666. The task of editing all the remaining fragments that could be procured was committed to Dr. Wallis, who was then Savilian professor of geometry in the University of Oxford. They were finally published at London, in the year 1672, under the title of "Jeremiæ Horroccii Opera Posthuma." They consist of a defence of the Keplerian astronomy, a selection of letters from Horrocks to Crabtree, astronomical observations of the two friends, and an exposition of the lunar theory of Horrocks by Flamsteed. In the preface to the work, Dr. Wallis has mentioned such facts as came to his knowledge respecting the life of Horrocks, nor has he failed to allude, in terms of burning indignation, to the apathy with which for more than twenty years the manuscripts of the youthful astronomer were regarded by his countrymen*. It must be acknowledged, however, that the act of reparation which was finally effected forms a bright episode in the early history of the Royal Society, and that throughout all the proceedings connected with the execution of the task assigned to him, Dr. Wallis exhibited a spirit of disinterestedness and zeal which reflects the highest honour on his character†.

When the early death of Horrocks is considered, his posthumous fragments may be readily supposed to derive their interest rather from the indications they afford of what might have been expected from him if a longer term of life had been granted to him, than from any positive influence which they were calculated to exercise on the progress of physical science. In the lunar theory, however, he effected an improvement which would alone suffice to obtain for him an imperishable reputation. His beautiful explanation of the inequality in the moon's longitude, termed the evection, by means of a libratory motion of the apsides and a variable eccentricity, was the last great step made in the development of the laws of the planetary movements previous to the establishment of the theory of gravitation by Newton; and there can be little doubt that it afforded material aid to that philosopher in his exposition of the general principles of perturbation as detailed by him in the sixty-sixth proposition of the first book of the "Principia" and its famous corollaries. Horrocks has thus won for himself a place among those great men who, from Hipparchus downwards, by their successive efforts, established the fundamental facts relative to the movements of the planets without reference to their physical cause.

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* "Non possum, inquam, non indignari, pretiosum hoc spectaculum (Venus in Sole Visa) nullo auro redimendum, descriptum, preloque paratum, delituisse per annos integros viginti duo; neminemque interea repertum esse, qui tam bellam patris mortui prolem susciperet, qui rem tanti ad astronomiam momenti in lucem mitteret, qui nostræ gentis famæ, vel omnium commodo, eatenus inserviret."

† The account of the various proceedings connected with this matter will be found

Although the posthumous works of Horrocks can only be regarded as mere fragments, their perusal cannot fail to excite a feeling of deep regret that astronomy was so soon deprived of the benefit of his labours. It would be inconsistent with the object of this work to attempt a complete analysis of their contents, but a glance over some of his letters to Crabtree may serve to confirm the truth of Delambre's remark, that if their author had lived he would have proved himself the worthy successor of Kepler. These letters were written originally in English, but were translated into Latin by Dr. Wallis.

In a letter dated November 23, 1637, Horrocks states that he had recently spent some time in meditating upon the physical principle in virtue of which the planets revolve in oval orbits. "Kepler," says he, "attributes their movements to the action of magnetic fibres, but I entertain serious objections to this hypothesis. It appears to me, however, that I have fallen upon the true theory, and that it admits of being illustrated by means of natural movements on the surface of the earth, *for nature everywhere acts according to a uniform plan, and the harmony of creation is such that small things constitute a faithful type of greater things.*"

In a letter dated July 25, 1638, he ascribes the motion of the lunar apsides to the *disturbing force of the sun.* This very remarkable idea of a perturbative influence, exercised by the various bodies of the planetary system upon each other, had not yet been suggested by any philosopher. The circumstance of its being perfectly true in the particular case alluded to by Horrocks, tends in a still greater degree to enhance the merit of the surmise. In the same letter he exhibits an illustration of the planetary movements by suspending a weight from a fixed point by a long cord, and, having drawn the weight a little aside from the vertical direction, applying to it a slight tangential impulse. This beautiful experiment, illustrative of the action of a central force, has been generally ascribed to Hooke, who merely reproduced it, at a meeting of the Royal Society, about thirty years after Horrocks had devised it*. Horrocks remarked that the pendulous body would describe an ellipse, the apsides of which would advance slowly in the direction of the body's motion. It did not escape his observation that this experiment did not represent the movements of the planets with sufficient fidelity, inasmuch as there were two perihelia and two aphelia, the centre of the ellipse being in fact the centre of force, instead of the focus, as in the case of nature. In order to make the parallel more complete, he supposed a gentle wind to blow constantly upon the pendulum in the direction of the major axis of the ellipse. If the representation was still imperfect, it must be admitted that the step here taken was at least in the right direction. Horrocks, in his researches on the physical cause of the planetary movements, laboured under the disadvantage of having only accessible to him the erroneous ideas of Kepler on the principles of mechanical science. A remark, however, contained in the letter just cited, will serve to show that he possessed a mind adequate to detect the fallacy of such ideas, and to substitute others more in accordance with nature in their stead. Kepler had supposed the planets to be whirled round in their orbits by the transverse action of magnetic fibres; but as their revolutions round their axes seemed to him to offer an impediment to this action, he had recourse to the strange supposition of the exterior stratum alone of each planet being endued with a rotatory motion. Horrocks remarked that such a supposition was totally unnecessary, since the rotatory motion of the planets could not impede their motion of translation, any more than the rotation of a stone thrown with the hand impedes the motion which it has acquired in the direction of the impulse.

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* Hooke first gave an account of his experiment at the meeting of the Royal Society, which was held on the 23rd of May, 1666 (Birch's "History of the Royal Society," vol. ii., p. 92). It might be supposed that as Horrocks' posthumous works were not published till 1672, Hooke could not have been indebted to them for the original idea of the experiment. It appears, however, that Wallis had completed his task of preparing the writings of Horrocks for publication as early as September 21, 1664 (Birch, vol. i., p. 470). The printing of the work was deferred by the Council until the president should give his opinion respecting it. The circumstance of its remaining so long in manuscript arose, doubtless, from the impoverished state of the Society's treasury during the early period of its

The following passages exhibit a distinct perception of the famous inequality in the mean motions of Jupiter and Saturn arising from their mutual disturbance. It may be remarked that, throughout the whole of the sixteenth century and the first half of the seventeenth century, the mean motion of Jupiter was increasing with great rapidity in virtue of that inequality, while, on the other hand, the mean motion of Saturn was undergoing a corresponding retardation.

Writing on the 3rd of June, 1637, he mentions that he had at length obtained possession of the Rudolphine Tables. A comparison of them with various modern observations convinced him that the mean motion of Jupiter was in reality *quicker* than Kepler had made it. On the 19th of January, 1638, he makes the same remark in nearly the same terms (*Æqualis motus Jovis est notabiliter velocior quàm apud Keplerum*). On the 25th of July, 1638, he proposes to correct the motion of Jupiter by *adding* 1° 30′ to the aphelion, and about 2′ to the mean longitude. In a letter dated September 29 of the same year, he states that the observations of Jupiter made in the time of Waltherus agree with the most recent observations in indicating, beyond all doubt, that the mean motion of the planet is more rapid than it appeared to be from the Rudolphine Tables. The quantity of the acceleration seemed to him to amount to 1′ in ten years. It was very certain, he added, that from the time of Tycho the mean longitude of the planet was at least 4′ or 5′ greater than the tables made it. On the 14th of September, 1639, he proposes to add 1′ to the mean longitude of Jupiter in the beginning of 1600, and 11′ in the beginning of 1700, with a proportional quantity in any intermediate period. In order to show how nearly Horrocks arrived at the true acceleration, it may be mentioned that the increments which the mean longitude of Jupiter actually received in the first half of the seventeenth century, during successive periods of ten years, were 1′ 22″, 1′ 20″, 1′ 18″, 1′ 15″, 1′ 12″. The time when Horrocks lived was, in fact, exceedingly favourable for detecting the great inequality of Jupiter and Saturn, on account of the rapidity with which it was then developing itself*. In a letter dated July 30, 1640, he still adheres to his previous conclusion. He proposes to add 5′ to the mean longitude at the beginning of 1640, and 6′ at the beginning of 1650. Throughout the whole of the current century, the mean motion of the planet seemed to him to be more rapid than it was according to the Rudolphine Tables, the acceleration being equivalent to an increase in the mean longitude of 1′ in ten years. "Whether the acceleration will continue or not," says he, "I do not know; but between 1490 and 1590 the mean motion was sensibly more rapid than it is now." He here seems to hint at the possible *periodicity* of the phenomenon.

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* The truth of this remark will be more apparent when it is stated that from 1800 to 1850 the accessions to the mean longitude of Jupiter, arising from the great inequality during successive periods of ten years, were only 10″.8, 17″.0, 22″.9, 28″.5, and 34″.1. These results, as well as those of a similar nature in the text, are calculated from the expression for the inequality given by Pontécoulant. (*Théorie Analytique du Système*

The *retardation* of Saturn's mean motion did not escape the sagacity of Horrocks, although he does not seem to have retained such a firm hold of the inequality as in the case of the corresponding *acceleration* of Jupiter's mean motion. On the 3rd of June, 1637, he proposes to *subtract* 4′ from the mean longitude of Saturn at the beginning of 1600. On the 15th of October, 1638, he writes that the observations of Saturn, in the time of Waltherus, indicated the mean motion of the planet to be *slower* than Kepler had made it in the Rudolphine Tables. On the 14th of September, 1639, he again alludes to the irregularity in the mean motion of the planet. "Saturn," he says, "seems to experience sometimes a singular retardation in its motion. (*Videtur Saturnus miram aliquam aliquando habere motus sui retardationem.*) He remarks, however, that the phenomenon would occasion him greater annoyance were it not that there was some consolation in being probably the first who discovered it (*nisi quod hoc aliquid solatii est, nos, credo, primos esse qui detegimus*). He requests Crabtree to watch the phenomenon carefully by making constant observations of the planet. With respect to the true correction to be applied to the Rudolphine Tables he was unable to pronounce a positive opinion; "but if we discover anything hereafter," says he, "the *retardation* of the planet will allow an easy correction of the Ephemerides."

In a letter dated July 25, 1638, he makes some conjectures relative to the nature and movements of comets. He supposes them to be projected from the sun, and to move with a continually slower velocity as they recede from that body, until at length they become stationary, and then begin to return with accelerated speed, like the sine in the circle, deflecting somewhat in the direction of the sun's rotation. He requests Crabtree to send him Tycho Brahé's observations of comets, contained in his *Progymnasmata*, in order that he might compare them with his hypothesis. In a letter dated September 29, 1638, he states that the observations of the comets of 1577 and 1590 confirmed his conjecture of comets in general being projected from the sun. He supposes, in accordance with Kepler's ideas of a whirling force, that they are subsequently *carried round the sun* with a motion which is, in all probability, *variable**.

On the 3rd of October, 1640, he announced to Crabtree that he had undertaken the prosecution of a series of observations on the tides, in order that, by the aid of experiment, he might be enabled to arrive at some definite conclusions respecting their real nature.

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the eccentricities and inclinations of the two planets, and also those due to the square of the disturbing force.

* Wallis, in a letter to the Royal Society dated January 21, 1664-5, states that Horrocks made the comet to return to the sun in an *elliptical figure, or very nearly so*. According to this hypothesis he traced the comet of 1577. He requests that the paper relative to this comet, being in Horrocks' own handwriting, be carefully preserved (Birch, Hist. Roy. Soc., vol. ii. p. 11).

On the 12th of December, 1640, he expressed to his friend a strong desire to obtain some of Gascoigne's measurements of the lunar diameter*. He also announced to him that his observations of the tides had already revealed to him many interesting particulars. They were withal very regular, although subject to many strange inequalities hitherto remarked by no person. He had only prosecuted his researches three months, but he hoped that, by continuing his observations for a year, he should obtain some valuable results†. In the same letter he entreats his friend to persevere in his astronomical pursuits, adding, that as soon as his own affairs would permit, he purposed resuming his favourite studies. Alas! ere another month had elapsed, his noble spirit had fled from its mortal tenement.

It cannot fail to excite the admiration of the reader, that a youth of twenty-two years of age should have exhibited in his researches such sagacity of thought and fertility of invention, such enlightened and judicious views on the various subjects which engaged his attention, and such unwavering confidence in the resources of his own mind. Who can doubt that, if his days had been more numerous, the history of physical science in the seventeenth century would read very differently from what it now does. Justly may it be affirmed, in the language of the illustrious editor of his fragments,—"*Qui enim tam paucis annis, auxiliis parvis, tantisque obsitus difficultatibus tantos progressus fecerat; quid non fecisset, si Deus hucusque vitam protelavisset! si necessariis omnibus instructus, doctorum etiam consortio, adjutus fuisset!*"

The utility of the transits of the inferior planets in furnishing an accurate method of determining the value of the solar parallax, was first pointed out by James Gregory, the celebrated mathematician, in his treatise entitled "Optica Promota," which was published at London in 1663. As the credit due to the original suggestion of this method has been generally ascribed to Halley, it may not be out of place to cite the passage in which Gregory alludes to it. In a scholium to the eighty-seventh problem, the object of which is to determine the parallaxes of two planets by observations of their conjunctions, he makes the following statement:—"This problem has a very beautiful application, although perhaps laborious, in observations of Venus or Mercury when they obscure a small portion of the sun; for by means of such observations the parallax of the sun may be investigated‡. It would be impossible to establish the claims of Gregory to priority of discovery upon a more unequivocal basis than is afforded by this passage. Halley's attention was first directed to the subject on the occasion of his observation of the transit of Mercury in 1677, that is to say, fourteen years after the publication of the *Optica Promota**. Whatever development the method acquired from him, cannot affect the merits of the original discoverer. Captain Smyth, while vindicating the just claims of Gregory, has truly remarked that Halley needs no borrowed plumes. Even in the present instance it must be admitted, that the ability with which he expounded the peculiar advantages attending the determination of the solar parallax by observations of the transits of Venus, the earnestness with which he recommended the practical application of the method, and the weight of his authority on questions relating to astronomical science, were mainly instrumental in inducing the different governments of Europe to adopt those liberal proceedings for observing the transits of 1761 and 1769, which have led to a more accurate knowledge of the dimensions of the solar system than could otherwise be hoped for.

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* Gascoigne was the original inventor of the micrometer, as will be shown in the next chapter, and it is to the results obtained by the use of this instrument that Horrocks here alludes. Crabtree had just returned from a visit to Gascoigne in Yorkshire, and one of the principal objects of Horrocks' contemplated visit to his friend was to obtain an account of the very remarkable improvements in practical astronomy which Gascoigne had recently effected. See, in the next chapter, an extract of a letter from Crabtree to Horrocks, in which there is contained an interesting allusion to the micrometer, which Gascoigne showed Crabtree on the occasion of this visit.

† Horrocks appears to have been the first person who undertook the prosecution of a continuous course of observations of the tides, for the express purpose of obtaining a series of facts which might form the groundwork of a philosophical investigation of the subject.

‡ Optica Promota, p. 130. The words in the original are:—"Hoc problema pulcherrimum habet usum, sed forsan laboriosum, in observationibus Veneris vel Mercurii parti-

The physical appearances noticed during the transits of Venus which happened in the last century have given rise to a good deal of speculation, but it must be admitted that the conclusions arrived at upon this point cannot be regarded as altogether satisfactory. The most remarkable of such appearances was that witnessed when the planet was just wholly within the sun's limb. It was found on the occasion of each of the transits of 1761 and 1769, that the interior contact of the planet with the sun did not take place regularly at the ingress, the planet appearing, for some time after it had wholly entered upon the solar disk, to be connected with the sun's limb by a dark ligament. A similar phenomenon was observed at the egress of the planet. It was also found that, even after the planet had wholly separated from the sun's limb, it did not acquire its round form till after the lapse of several seconds. In order that the reader may form a more accurate conception of these phenomena, it may not, perhaps, be unacceptable to cite in detail a few of the observations relating to them.

Mr. Hirst, who observed the transit of 1761 at Madras, states that "at the total immersion, the planet, instead of appearing truly circular, resembled more the form of a bergamot pear, or, as Governor Pigott then expressed it, *looked like a nine-pin*: yet the preceding limb of Venus was extremely well defined." With respect to the end of the transit he remarked, "that the planet was as black as ink, and the body truly circular, just before the beginning of the egress, yet it was no sooner in contact with the sun's preceding limb, than it assumed the same figure as before at the sun's subsequent limb: the subsequent limb of Venus keeping well defined and truly circular."†

A similar appearance was observed by Lalande at Paris, by Bergman at Upsal, and also by several other individuals.

Dr. Maskelyne, who observed the transit of 1769 at Greenwich, gives the following description of a phenomenon of a similar nature witnessed by him at the ingress of the planet:—

"The irregularity of Venus's circular figure was disturbed towards the place where the internal contact should happen, by the addition of a protuberance, dark like Venus, and projecting outwards, which occupied a space upon the sun's circumference which bore a considerable proportion to the diameter of Venus. Fifty-two seconds before the thread of light was formed, Venus's regular circumference (supposed to be continued as it would have been without the protuberance) seemed to be in contact with the sun's circumference, supposed also completed. Accordingly, from this time, Venus's regular circumference (supposed defined in the manner just described) appeared wholly within the sun's circumference, and it seemed, therefore, wonderful that the thread of light should be so long before it appeared, the protuberance appearing in its stead. At length, when a considerable part of the sun's circumference (equal to one-third or one-fourth of the diameter of Venus) remained still obscured by the protuberance, a fine stream of light flowed gently round it from each side, and completed the same in the space of three seconds of time; and Venus appeared wholly within the sun's lucid circumference. *But the protuberance, though diminished, was not taken away till about twenty seconds more; when, after being gradually reduced, it disappeared, and Venus's circular figure was restored.*"[*]

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* James Gregory, one of the most eminent mathematicians of the seventeenth century, was born at Aberdeen in 1639, and died at Edinburgh in 1675, at the early age of thirty-six years. He was only twenty-four years old when he published the *Optica Promota*, which, besides the important remark alluded to in the text, contained also the original explanation of the principle of the reflecting telescope. Halley's earliest allusion to the utility of observations of the transits of the inferior planets in determining the solar parallax, is contained in his *Catalogus Stellarum Australium*, published in 1679. He subsequently returned to the subject in the volumes of the *Philosophical*

Dr. Bevis states in the account of his observations that "the planet seemed quite entered upon the disk, her upper limb being tangential to that of the sun; but instead of a thread of light, which he expected immediately to appear between them, he perceived Venus to be still conjoined to the sun's limb by a slender kind of tail, nothing near so black as her disk, and shaped like the neck of a Florence flask. The said tail vanished at once; and *for a few seconds after, the limb of Venus, to which it had been joined, appeared more prominent than her lower part*, somewhat like the lesser end of an egg, but soon resumed its rotundity."[†]

The Rev. Mr. Hirst thus describes the appearance presented during the transit:—"The same phenomena of a protuberance which I observed at Madras in 1761, at both internal contacts, I observed again at this last transit. At both times the protuberance of the upper edge of Venus diminished nearly to a point before the thread of light between the concave edge of the sun and the concave edge of the planet was perfected, when the protuberance broke off from the upper edge of the sun: *but Venus did not assume its circular form till it had descended into the solar disk some distance*[‡].

Mr. Dunn, who observed the transit at Greenwich, remarks that "he saw the planet held as it were to the sun's limb by a ligament formed of many black cones whose bases stood on the limb of Venus, their vertices pointing to the limb of the sun."[§]

Mr. Pigott states that Venus, before she separated from the sun, was considerably stretched out towards his limb, which gave the planet nearly the form of a pear; *and even after the separation of the limbs, Venus was twelve or more seconds before she resumed her rotundity.*"[‖]

With respect to the physical cause of the phenomena above referred to, the most probable hypothesis of their origin is that which attributes them to the influence of irradiation. Lalande first shewed that the dark ligament connecting the limbs of the planet and the sun might be satisfactorily accounted for upon this principle[¶]. The most consistent explanation that can be given of the protuberance on the limb of the planet, when it appeared wholly separate from the sun's limb, is that proposed by Prof.

[*] Phil. Trans. 1768, p. 358. [†] Ibid., 1769, p. 190. [‡] Ibid., p. 229.

Powell in the analogous case of annular eclipses. The appearance noticed by Mr. Dunn seems to be totally inexplicable, unless it be assimilated to the dark lines witnessed during the annular eclipse of 1836, and other similar occurrences.

It was remarked by several observers of the transits of 1761 and 1769, that both at the ingress and egress, the portion of the limb of the planet that was off the sun, was visible by means of a faint light surrounding it in the form of a ring. La Chappe, who observed the transit of 1761 at Tobolsk, in Siberia, states that the light of the ring was of a very deep yellow near the body of the planet, but that it became more brilliant towards the outer border[*]. MM. Stromer, Mallet, Bergman, and Melander, who observed the same transit at Upsal, remarked that when three-fourths of the planet's limb had entered upon the sun, the remaining fourth was visible by means of a faint ring which appeared around it[†]. A similar phenomenon was observed on the same occasion by Wargentin at Stockholm, by Planman at Cajainbourg, and in several other instances[‡].

Dr. Maskelyne, who observed the ingress of Venus upon the sun's disk at Greenwich on the occasion of the transit of 1769, states that, when the planet was little more than half entered upon the sun, he saw her whole circumference completed, by means of a vivid but narrow and ill-defined border of light, which illuminated that part of her circumference that was off the sun. He adds that it disappeared two or three minutes before the internal contact[§]. A similar phenomenon was witnessed during the same transit, by Wales and Dymond at Hudson's Bay[‖], by Pingre and De Fleurieu at Cape Francis in the Island of St. Domingo[¶], and by various other observers in different places.

Several of the observers of the transits of 1617 and 1769 remarked that, when Venus was *wholly* entered upon the sun, there appeared a faint ring around her limb. Dunn, in his account of the transit of 1769, describes it as a lucid annulus about five or six seconds in breadth, *somewhat dusky towards the limb of the planet,* and at the outer margin tinged a little with blue[**]. Mr. Hitchins, alluding to the same phenomenon, states that it was excessively white and faint, and that *it was brightest towards the body of the planet*[††]. Nairne asserts that it appeared brighter and whiter than the body of the sun[‡‡].

It is worthy of remark that, in general, those individuals who observed a faint light around the part of the limb of Venus that was off the sun, on the occasion of the transits of 1761 and 1769, do not seem to have perceived the complete annulus around the limb of the planet when it was wholly entered upon the sun's disk. On the other hand, most of those observers who witnessed the latter phenomenon do not make any allusion to the former as having been seen by them. The observations of La Chappe, Maskelyne, and several other individuals, are very clear upon this point. It would seem, therefore, that the two phenomena are not of the same nature. With respect to the physical origin of either, no satisfactory explanation has yet been offered. The appearance noticed when the planet was only partially on the sun, has a strong analogy to that alluded to by M. Arago and several other persons in France relative to the visibility of the *whole* of the moon's limb during the progress of the total eclipse of the sun of

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[*] Mém. Acad. des Sciences, 1761, p. 363.
[†] Ibid., p. 364.
[‡] Ibid.
[§] Phil. Trans., 1768, p. 357.
[‖] Ibid., 1769, p. 482.
[¶] Ibid., 1770, p. 498.

July 8, 1842. If it be assumed that the atmospheric light diminishes with such rapidity from the sun's limb that the light of the solar atmosphere forms an aliquot fraction of it at a very small distance from the limb, it may then be inferred, as in that case, that the visibility of the planet's limb arises from the obscurity of its surface, when contrasted with the brightness of the ground upon which it appears projected. Such a supposition, however, is by no means probable. The faint ring that was seen by some observers around the planet's limb when it had wholly entered upon the sun's disk, has been supposed to indicate the existence of an atmosphere about the planet, but the various statements respecting its appearance are not sufficiently consistent with each other to warrant such a conclusion, although at the same time it seems to be justified by other considerations, to which allusion has already been made in a former chapter.

Sometimes the planets are eclipsed in consequence of the moon passing between them and the earth. The earliest recorded phenomenon of this nature is an occultation of Mars by the moon, which Aristotle makes mention of in one of his works. He states that when the moon was half-full, the planet entered behind the limb on the unenlightened side, and subsequently emerged on the bright side[*]. Kepler calculated the date of this occultation, and found that it occurred on the night of the 4th of April in the year 357 A.C.[†]. The rapid and tortuous motion of the moon produces occasional occultations of this nature, but they are not of much importance to the astronomer. It was expected that the circumstances accompanying such occultations would serve to throw light upon the much-disputed question of the existence of a lunar atmosphere, but no such advantage has hitherto been derived from them.

The occultation of a star by the moon is a phenomenon which, although of frequent occurrence, never fails to prove interesting. When the dark limb of the moon comes up to the star, the occultation is invariably found to take place *instantaneously*, whence it is manifest that the apparent magnitude of the star must be excessively small. The effect in such cases is most striking when the whole body of the moon is invisible, as occasionally happens when she is totally eclipsed. A phenomenon of this kind was witnessed by Wargentin during the lunar eclipse of May 18, 1761, to which allusion has already been made. Previous to the total disappearance of the moon, he perceived a star near her eastern limb, which seemed to be about to undergo occultation. He therefore followed it with great attention until at length, at $10^h\ 52^m\ 39^s$, when even the faintest trace of the moon had ceased to be visible, it vanished *in less than the twinkling of an eye*[‡].

Occultations of stars by the moon serve to fix the apparent position of the latter body with great accuracy, and in consequence they have proved very serviceable in correcting the elements of her motion. For a similar reason they are of great use in determining the differences of longitude of places on the earth's surface.

A singular phenomenon of a physical nature is sometimes observed on the occasion of an occultation of a star. When the limb of the moon has come up to the star, the latter appears to advance upon the moon's disk, continuing visible in this manner for several seconds previous to its occultation[*]. No satisfactory explanation of this strange anomaly has hitherto been advanced by any person. One of the most recent, as well as one of the most specious modes of accounting for its physical origin is due to Prof. Stevelly. According to the hypothesis proposed by him, the phenomenon is purely an effect of the diffraction of light. Newton had shewn by experiment that the rays of light which pass *very near* the edge of a body are bent away from it, so as for a short distance to describe curves which are convex with respect to it. Prof. Stevelly conceives the visible contour of the moon to be such a diffracting edge to the slender beam of light which reaches the eye from a fixed star. Under such circumstances, it is manifest, from the convexity of the course pursued by the beam of light while under the influence of the diffracting force, that its final direction when it enters the eye ought, if produced toward the moon's surface, to fall *within* her limb[†].

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[*] De Cælo., lib. ii., cap. 12.

[†] Ad. Vitellionem Paralipomena, p. 307.

[‡] "Oculi ictu citius" (*Phil. Trans.*, 1761, p. 210).

If the above explanation were true, the phenomenon ought to be visible on the occasion of every occultation of a star by the moon. Such, however, is far from being the case. This is a defect which may be said to characterise in a greater or less degree all the explanations that have hitherto been offered relative to a phenomenon, which is manifestly an optical illusion, although its true source is not easy to be discovered.

Sometimes the planets occult one another in the course of their motion round the sun. Such phenomena, however, are manifestly of very rare occurrence. Kepler states that on the 9th of January, 1591, Mæstlin and himself witnessed an occultation of Jupiter by Mars. The red colour of the latter on that occasion plainly indicated that it was the inferior planet[‡]. He also mentions that on the 3rd of October, 1590, Mæstlin witnessed an occultation of Mars by Venus. In this case, on the other hand, the white colour of Venus afforded a clear proof that she was the nearer of the two planets to the earth[§]. It is to be borne in mind that these observations were made before the invention of the telescope, so that it is doubtful whether in either of these cases the one planet was actually superposed above the other, for the peculiar colour of the light might arise simply from the predominating influence of the brighter planet[‖].

Sometimes there happen occultations of the fixed stars by the planets. On the 1st of October, 1672, the planet Mars eclipsed a star in the constellation *Aquarius*. Cassini, who was then engaged in researches on the parallax of Mars, had previously resolved to observe this interesting phenomenon, but he was prevented by cloudy weather from effecting his object. Phenomena of a similar nature have occurred in more recent times, but no physical consequences have been deduced from them.

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[*] See a collection of observations of this nature in a paper by Sir James South on the occultation of δ *Piscium* by the moon (Mem. Ast. Soc., vol. iii., p. 303, et seq).

[†] Brit. Assoc. Rep., 1845. (*Trans. of the Sections*, p. 5.)

[‡] Ad Vitellionem Paralipomena, p. 305.

[§] Ibid., p. 305.

[‖] For an account, by Dr. Bevis, of an occultation of Mercury by Venus, on May 17, 1737, see the *Philosophical Transactions* for 1737, p. 394. On this occasion, the interesting phenomenon alluded to by Kepler does not seem to have been remarked by any observer.

CHAPTER XVIII.

Early Methods of observing the Celestial Bodies.—Instruments of the Greek Astronomers.—Accurate Principles of Observation first employed by the Astronomers of the Alexandrian School.—Improvements effected by Hipparchus.—Ptolemy substitutes the Quadrant for the Complete Circle.—Arabian Astronomers.—The Method by which they indicated the Time of an Observation.—Revival of Practical Astronomy in Europe.—Labours of Waltherus.—Tycho Brahé.—Landgrave of Hesse.—Hevelius.—Close of the Tychonic School of Observation.—Observatory of Copenhagen established.—The Pendulum applied to clocks by Huyghens.—The Royal Society of London, and the Academy of Sciences of Paris, established.—Invention of the Micrometer.—Application of the Telescope to divided instruments.—Observatories of Paris and Greenwich established.—Labours of Roemer.—Transit Instrument invented.—The use of Circular Instruments for taking Altitudes introduced.—Labours of Flamsteed and Halley.—Royal Observatory of Paris.—Commencement of the era of accurate observation.—Bradley.—Lacaille.—Mayer.—Maskelyne.—Pond.—Airy.—Reduction of Planetary and Lunar Observations.—Present state of Practical Astronomy.

The history of astronomy does not exhibit a more interesting picture than that which represents the progress of the art of observation, from the rude essays of early ages to the refinement and precision which characterise its present state of advancement. The Chaldeans, to whom the origin of astronomy is usually ascribed, do not seem to have attained any excellence in this important department of the science. Their observations of eclipses of the moon, as cited by Ptolemy in the Syntaxis, are as rude as can possibly be imagined. The time is expressed only in hours, and the quantity eclipsed in terms of the half and quarter of the moon's diameter. Herodotus states that the Greeks were indebted to the Babylonians for the *pole*, the *gnomon*, and the division of the day into *twelve* hours. The pole seems to have been a concave hemispherical sun-dial, having a vertical style in the centre, by means of which the interval included between sunrise and sunset, for each day throughout the year, was divided into *twelve* equal parts. The construction of such an instrument does not require any acquaintance with the principles of trigonometry or dialling; it merely implies a knowledge of the uniform motion of the celestial sphere. The gnomon, besides being an imperfect instrument for astronomical purposes, is limited in its application. It is probable that, by the use of it, the Chaldeans succeeded in obtaining an approximation to the length of the solar year, but there is not the smallest reason to suppose that they employed it in determining any other of the fundamental facts of astronomy. Indeed, they do not seem to have made observations at all for the purpose of forming materials to serve as the groundwork of future reasoning. They simply contented themselves with noting the more remarkable phenomena as they occurred, and hence deducing a few rough conclusions of a general nature. It is manifest that so long as astronomy continued to be cultivated in this manner, it could not attain a high state of perfection.

The Greek philosophers were too much preoccupied with the idea of arriving at final conclusions on all subjects by the mere force of abstract discussion, to devote their attention to the prosecution of astronomical observations. It would appear, however, that by comparing together the Chaldean records of eclipses, extending over a long succession of ages, some of the earlier of the Greek mathematicians ascertained with considerable accuracy several periods relating to the motion of the moon. The earliest astronomical observation recorded as having been made by the Greeks previous to the establishment of the Alexandrian School, is a determination of the summer solstice by Meton, in the year 430 A.C. The instrument, termed a *heliometer*, which was used by Meton on this occasion, was, in all probability, no other than a modification of the gnomon. The date of this solstice has been chosen for the epoch of the Metonic cycle of nineteen years, which is employed in regulating the occurrence of religious festivals.

A new era commenced in the history of astronomical observation when Alexandria became the capital of the civilised world. Under the liberal patronage of the Ptolemies a magnificent building was erected, in which were deposited *circular* instruments for determining the positions of the heavenly bodies, and every facility was given to astronomers for prosecuting a continuous series of observations. Timocharis and Aristillus are the earliest individuals mentioned in connexion with this school. These astronomers appear to have flourished about the year 300 A.C. Ptolemy cites several of their observations in the Syntaxis. Among these are the declinations of a few of the principal stars. It is evident that such results could not be established without a knowledge of the position of the equator in the celestial sphere. It does not appear that the earlier astronomers of the Alexandrian School were acquainted with any method for determining the right ascensions of the stars. At any rate Ptolemy does not cite any observations of this kind as having been made by them. Indeed, there are no grounds for supposing that they knew the *exact* position of the equinoctial colure upon which the right ascension of a star depends. It is a remarkable fact, however, that Hipparchus was enabled by means of certain eclipses of the moon, observed by Timocharis, to determine the place occupied by the equinox among the stars in the days of that astronomer. Thus, Ptolemy states that Hipparchus having compared several eclipses of the moon, observed very accurately by himself, with other more ancient eclipses, observed by Timocharis, found that while the bright star, *Spica Virginis*, preceded the Autumnal equinox 8° in the time of the last-mentioned astronomer, it preceded the same equinox only 6° in his own time[*]. No account whatever is given of the process by which these interesting results were arrived at.

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[*] Syntaxis, lib. vii., cap. ii. With respect to the mode in which Hipparchus determined the place of the equinox by means of his own observations of eclipses of the moon, it was doubtless the same as that which will be presently alluded to in the text. But it is difficult to imagine by what process he was enabled to deduce a similar result from the observations of Timocharis, since there are no grounds for supposing that the latter astronomer possessed an intrument for measuring the difference of the right ascensions of two celestial bodies. It is probable that if a bright star *happened* to be very near the moon at the time of an eclipse, its position in the direction of the zodiac with respect to that body was roughly determined with the instrument employed in measuring the declinations of the stars, and the result recorded as part of the observation of the eclipse. It is not difficult to conceive that an instrument composed of a circle fixed in the plane of the equator, and another circle of equal magnitude moveable about its poles, might serve for the approximate measurement of the difference of the right ascensions of two celestial bodies situate very near the equator. The earlier astronomers of the Alexandrian School, in all probability, did not contemplate any definite object in noting the positions of the stars in the vicinity of the moon during the eclipses of that body. Indeed, it would have been impossible to deduce the place of the equinox among the stars from such results, even if they had been accurately determined with the astrolabe; for the

We owe to Eratosthenes, another of the astronomers who observed at Alexandria, the earliest determination of one of the most important elements of astronomical science. This astronomer is said to have measured the distance between the tropics, and to have found it to be equal to $\frac{11}{83}$ parts of the circumference of the circle. This gives 23° 51′ 19″.5 for the obliquity of the ecliptic—a result which exhibits a remarkable agreement with that assigned by the theory of gravitation, as the true value of the element, in the age of Eratosthenes.

It is manifest that neither the distances of the stars from the equator, nor the obliquity of the ecliptic, could have been determined even roughly without the use of instruments. The information which Ptolemy has supplied upon this subject is exceedingly scanty. In treating of the obliquity of the ecliptic, he describes an instrument for determining the meridional altitude of the sun. It was composed of two concentric circles, placed exactly in the plane of the meridian, one of which revolved within the other about their common centre. The inner circle carried two small prisms attached to the opposite extremities of a diameter, and when the sun was on the meridian, it was turned round until the shadow of the upper prism fell exactly upon the lower one. An index, affixed to the latter, then marked upon the graduated limb of the outer circle the meridional altitude of the sun *. It was, in all probability, by means of an instrument of this construction that Eratosthenes determined the altitude of the sun at each of the solstices, and hence deduced the distance between the tropics, the half of which distance gives the obliquity of the ecliptic.

From the observed altitudes of the sun at the summer and winter solstices, it was easy to infer the position of the equator in the celestial sphere. In order to ascertain the passage of the sun through the equinox, a circular ring of metal was disposed in the plane of the equator, and the shadow of the upper half was watched until it fell upon the inner or concave surface of the lower half. As the shadow did not cover the entire breadth of the surface on which it fell, it is manifest that the instant when the surface was equally illuminated on each side of the shadow, indicated the presence of the sun in the plane of the equator. Ptolemy cites a passage from Hipparchus, in which that astronomer refers to two circles of this description that were used at Alexandria for observing the passage of the sun through the equinoxes. They were constructed of copper, and were placed in the square portico of the Museum †.

Ptolemy has given no account of the instrument by means of which the earlier astronomers determined the distances of the stars from the equator. It in all probability resembled, in principle, the astrolabe which Hipparchus employed at a subsequent period in observing the stars, only it was more simple in construction, since the use of it was confined to one measurement relative to each star. In fact, if we suppose a circle to be placed in the plane of the equator, and another circle of equal magnitude to be movable around its poles, the distance of each star from the equator would be ascertained by turning the latter circle round until the star appeared in its plane, and then noting the place of the star on its divided limb.

Whatever credit may be due to the earlier astronomers of the Alex-

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quainted with any method of reducing the motion of the sun to calculation until Hipparchus undertook his researches on the subject.

* Syntaxis, lib. i., cap. x.

Ibid., lib. iii., cap. ii.

andrian School for the sound principles of observation which they appear to have practised, as well as for the care with which they determined some elements of fundamental importance, it is to Hipparchus alone that the establishment of astronomy, as a science of calculation based upon observed facts, is to be attributed. The mode in which he availed himself of the results of observation, in the execution of this great work, is in the highest degree interesting. One of the most important elements of astronomical science is the length of the tropical year. Hitherto it had been supposed to consist exactly of 365¼ days. Hipparchus, however, having compared a solstice observed by himself with one observed by Aristarchus 147 years earlier, found that the sun arrived in the same place 12 hours sooner than he ought to have done, if the year had consisted exactly of 365¼ days. He, therefore, concluded that the true length of the year was less than this quantity by $\frac{1}{300}$th part, and consequently he determined it to be equal to $365^d\ 5^h\ 55^m\ 12^s$. This is the earliest example, in the history of astronomy, of the *correction* of a fundamental fact of the science by the comparison of two *distant* observations. The result obtained by Hipparchus on this occasion exceeds the true length of the tropical year by about six minutes; but the magnitude of the error is not to be wondered at, when the difficulty of determining the exact instant of the solstice is taken into account *.

Another beautiful example of the simplicity of the means employed by Hipparchus in establishing the basis of his theories, is exhibited in his researches on the elements of the solar orbit. Having found that an interval of $94^d\ 12^h$ elapsed between the vernal equinox and the summer solstice, while only $92^d\ 12^h$ were included between the summer solstice and the autumnal equinox, he from these two facts deduced the eccentricity of the solar orbit, and also the place of the apogee. His mode of deriving the analogous elements of the lunar orbit from three observed eclipses of the moon, is also equally worthy of admiration.

This illustrious astronomer did not content himself with merely observing the moon on those occasions during which she was eclipsed, and determining her place in the heavens by means of the sun, which was then opposite to her. He also observed her position in other parts of her orbit by means of the astrolabe, an instrument of which he is supposed, with great probability, to have been the original inventor †. Ptolemy has given a description of the astrolabe in the beginning of the fifth book of the *Syntaxis.* It was used for the purpose of determining the latitude of a celestial body, and its longitude relative to another body, with which it was compared, whose absolute longitude was known. Two equal circles, whose planes were perpendicular to each other, were firmly fastened together, and were so disposed that the one represented the celestial ecliptic, and the other the solstitial colure. At the points of the solstitial circle, corresponding to the poles of the ecliptic, there were placed two cylinders, which projected both within and without the circle. To the outer cylinders was adapted a movable circle of latitude, the interior of which coincided exactly with the exterior of the solstitial circle. A similarly movable circle was adapted to the inner cylinders, which was so constructed as to be embraced exactly by the concave surface of the solstitial circle. The whole machine was made to turn round the two points in the solstitial colure corresponding to the poles of the equator, and its position was adjusted by directing the axis of revolution to the poles of the celestial equator.

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* The equinoxes admit of more accurate determination than the solstices, but there were no early observations of this kind accessible to Hipparchus. He did not fail, however, to observe several equinoxes with great care, in order that the results might serve as useful materials, at some future period, for the advancement of astronomical science. Ptolemy, coming 160 years after Hipparchus, might have deduced a further correction to the length of the tropical year, but he has sadly damaged his reputation as an observer by his researches on this subject. Lalande, however, by comparing 9 equinoxes determined by Hipparchus, with the observations of modern astronomers, has obtained, $365^d\ 5^h\ 48^m\ 48^s$ for the length of the tropical year; a result which does not perhaps differ one second from the true value. (*Mém. Acad. des Sciences*, 1782, p. 249.)

† Certain passages in the works of Pliny and several other ancient writers, concur in supporting this assertion. But, indeed, apart from all positive statements, there is a strong presumption that Hipparchus was the inventor of the astrolabe, since it is almost impossible to conceive that it could be of any service to astronomy previous to his time. Thus, one of the objects of the astrolabe was to determine the longitude of the moon relative to the sun in different parts of her orbit; but no such observations were made by the earlier astronomers, since they were unacquainted with any theory of the moon's motion, and therefore had no obvious motive for observing that body except in eclipses.

When it was required to determine the position of a celestial body with this instrument, by means of another body whose position had been already ascertained, the exterior circle was made to revolve until the latter body appeared in its plane, and the interior circle was similarly directed to the body whose position was to be found. The distance between these two circles, measured upon the ecliptic, then gave the difference of the longitudes of the two bodies, and the distance of the second body from the ecliptic, measured upon the interior circle, gave its latitude *.

The astrolabe thus constructed served to determine the position of a celestial body by means of its longitude and latitude. It is manifest that, by placing a circle in the plane of the equator, the same object might be accomplished with still greater facility by observing the right ascension and declination of the body Hipparchus must have used the astrolabe in this form in the early part of his career, since it is by means of their right ascensions and declinations that he originally indicated the places of the stars. He was doubtless induced to abandon this practice upon his discovery of the precession of the equinoxes, since an obvious advantage then appeared to result from the designation of the place of a star by its longitude and latitude.

It does not appear that *regular* observations of the moon were made by any astronomer previous to Hipparchus. The earlier observers confined themselves to notices of eclipses and occultations, and it was by an examination of records of the former class of phenomena, extending over a long succession of ages, that the Greek mathematicians arrived at those secular periods relative to the moon's mean motion, which Ptolemy has alluded to in the Syntaxis. Hipparchus also, as has been already mentioned, deduced the elements of the lunar orbit from observations of eclipses, and so long as he confined his researches on the moon's motion to her syzygees,

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or longitudes of two stars. Such observations would have been of no value unless it had been possible, in each case, to ascertain also the absolute place of either of the stars with respect to the equinox. This, however, could not be effected without the use of solar tables (at least according to any method known in ancient times), and such were not available to the earlier astronomers of the Alexandrian School, since Hipparchus was the first who established the theory of the sun's apparent motion.

* The place of the star upon the interior circle of latitude was determined by means

he found a satisfactory agreement between the results of theory and those of observation. But when he was enabled by means of the astrolabe to observe the position of the moon in other parts of her orbit, and more especially in the quadratures, he detected a series of discordances between her actual and computed places, which seemed irreconcilable with the theory of her motion as established by him. The true explanation of these anomalies was reserved for Ptolemy; but it must be borne in mind that Hipparchus first detected their existence, and that he also executed a great number of the observations which served as the basis of Ptolemy's researches on the subject*.

Hipparchus did not fail to make numerous observations of the planets, which at a subsequent period formed precious materials to Ptolemy in establishing the theory of their movements. But the most remarkable result which crowned the labours of this great astronomer was his discovery of the precession of the equinoxes. It was doubtless with the view of enabling posterity to arrive at a more accurate knowledge of this singular phenomenon, that he conceived the grand design of determining the positions of all the stars visible in the firmament, so as to give a faithful representation of the celestial sphere as it appeared in his time. It was necessary for this purpose to determine the *exact* place of either of the equinoxes among the stars, but a serious obstacle stood in the way of effecting this object. The position of the equinox in the celestial sphere is determined by the passage of the sun across the equinoctial circle; but when the sun is above the horizon there are no stars visible whose position might be determined with respect to him, and when the stars have become suitable objects for observation, the sun has already disappeared. This seems to have formed an insuperable impediment to the earlier astronomers in their endeavours to ascertain the absolute right ascension or longitude of a star. Hipparchus devised two distinct modes of obviating this difficulty. One of these was by observations made during eclipses of the moon. Since the angular distance between the sun and moon is exactly 180° on all such occasions, it follows that the place of the moon during an eclipse is directly deducible from that of the sun; and as the place of the sun with respect to the equinox is known by the solar tables, the place of the moon with respect to the equinox hence becomes known also. It is clear, then, that if the difference of the longitudes or the right ascensions of the moon and a star be determined with the astrolabe during the occurrence of a lunar eclipse, the absolute position of the star with respect to the equinox may hence be ascertained†.

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* It is a curious fact that the inequality here referred to, termed the evection, confounds itself in syzygees with the elliptic inequality or the equation of the centre. Hence it happened that observations confined exclusively to eclipses failed to indicate its existence. Ptolemy, in his preliminary exposition of the inequality, cites two observations, one by Hipparchus and the other by himself, both made when the moon was in the second quadrature. The observation of Hipparchus is of so exceptional a nature, that it is impossible not to conclude that it was taken from a great mass of observations, although Ptolemy himself is silent upon this point. In the first place the apsides were in syzygees; secondly, the moon was in quadratures; thirdly, she was in the nonagesimal, or that point in her diurnal course at which the effect of her parallax took place wholly in latitude. (*Syntaxis*, lib. v., cap. iii.) With the view of confirming his explanation of the inequality, Ptolemy subsequently cites two additional observations of the moon, *both of which* are in this case by Hipparchus. (*Syntaxis*, lib. v., cap. v.)

† If an eclipse of the moon happened when the sun was in either of the equinoxes, the moon would necessarily be in the opposite equinox, and hence the position of the

This method, however ingenious, laboured under the disadvantage of being practicable only on rare occasions. The other method contrived by Hipparchus for attaining the same end was more general in its application. When the sun was still above the horizon, he determined the difference of the longitudes of the sun and moon with the astrolabe, and as soon as the star became visible after sunset, he ascertained, in a similar manner, the difference of the longitudes of the moon and star. It is clear that from these two observations he obtained the difference of the longitudes of the sun and star, taking into account the correction due for the motion of the moon during the intermediate interval; and as the place of the sun with respect to the equinox was known by the solar tables, he hence deduced the absolute longitude of the star. When the longitude of one star was accurately found by this process, the longitude of any other star might be directly derived from it by simultaneous observations of both stars with the astrolabe. It was thus that the genius of Hipparchus triumphed over a difficulty which long appeared to be insurmountable, and thereby enabled him to effect with complete success one of the most stupendous undertakings recorded in the annals of science.

Ptolemy, although the most eminent astronomer of antiquity, after Hipparchus, does not rank high as an observer. It has been already mentioned that his catalogue of the stars has been strongly suspected to be no other than the catalogue of Hipparchus, reduced to his own time by the application to all the longitudes, of what he conceived to be the true quantity of the alteration occasioned by the precession of the equinoxes. An examination of his labours on other subjects of astronomy tends to confirm this impression. He nowhere enters into that minuteness of description which indicates a *boná fide* observer anxious to persuade his readers of the excellent qualities of his instruments, and of the precautions which he employed to ensure the accuracy of his results. But what is most to be deplored is his suppression of all the ancient observations, including those of Hipparchus, with the exception of the few which squared with his own calculations. The loss of these precious materials has involved many interesting points of the ancient astronomy in impenetrable obscurity.

An account has been given of the circular instrument employed by the ancient astronomers in determining the meridional altitude of the sun. Ptolemy rejected the complete circle, adopting in its stead a quadrant, which he considered to be preferable. The verticality of the instrument was established by means of the plumb-line. A small cylinder fixed at the centre threw its shadow upon a similar body which moved along the divided limb, and thereby indicated the altitude of the sun *. The Greek astronomers appear to have divided their instruments only to every ten minutes. It is remarkable that Ptolemy does not describe any method for placing his instruments in the plane of the meridian.

Ptolemy's substitution of the quadrant for the complete circle was a

ing stars. Even if the eclipse occurred *a few days* before or after the equinox, the place of the sun, and consequently that of the moon, might still be determined within the limits of the errors of the ancient observations, by supposing the sun to revolve uniformly round the earth in 365¼ days. It is possible that Timocharis may have found in this manner that the star *Spica Virginis* preceded the autumnal equinox 8° in his time, but it is more probable that Hipparchus, who cited this fact as a proof of the precession of the equinoctial points, was in reality the individual who first established its existence by a discussion of the eclipses of the moon observed by Timocharis, *and the use of his own solar* retrograde step in practical astronomy. This circumstance, however, was generally overlooked by all his successors down to the time of Roemer, who has the merit of being the first to restore the use of the circle in astronomical observations. Indeed, it is only in our own day that the quadrant has been definitively abandoned by astronomers as an essentially imperfect instrument for determining the apparent positions of the celestial bodies.

Although Ptolemy does not seem to have been an observer of the first order, he possesses many claims to the admiration of posterity. Abundant proofs of his sagacity are to be found in his great work, the *Mathematica Syntaxis*, but his discovery of the inequality in the moon's longitude, termed the evection, by a comparison of his own observations and those of Hipparchus with the computed places of the moon, would alone entitle him to be ranked with the greatest astronomers of ancient or modern times.

Very little is known respecting the mode in which *time* was measured by the ancient astronomers. In the daytime this object seems to have been effected by means of dials. During the absence of the sun they used clepsydras of various sorts, but all such contrivances were found to be very imperfect, on account of the unequal flow of the water. In some instances the time of an observation was indicated by the point of the zodiac which was on the meridian. This method, when practicable, was doubtless preferable to any other with which the ancient astronomers were acquainted.

The Arabian astronomers do not seem to have effected any essential improvement in the methods of observation. Their instruments, however, were generally larger and better constructed than those of the Greek astronomers, and they appear to have taken greater precaution to ensure the accuracy of their results. The astrolabe, as used by them, was, in some instances, a complicated instrument, since it carried circles representing the equator, the ecliptic, and the other principal circles of the celestial sphere. In this form it acquired the appellation of an armillary sphere. Instruments of a similar construction continued to be used in making observations of the celestial bodies down to the beginning of the seventeenth century.

The Arabians usually indicated the time of an observation by means of the apparent altitude of a star whose position was known. It has been mentioned that Ptolemy has given no account of the method he employed in tracing a meridian line. The Arabian astronomers effected this object by equal altitudes of the sun when he was east and west of the meridian*. It does not appear that they took into account the effect of the motion of the sun during the interval included between the two observations.

Upon the revival of science in Europe towards the close of the fifteenth century, the cultivation of astronomy was for some time confined almost exclusively to Germany. Waltherus, a native of Nuremburg, to whom we have already had occasion to allude, may be considered the earliest individual of modern times whose observations have contributed to the advancement of astronomical science. He first introduced the practice of determining the apparent place of a planet by observations of its altitude, and its distance from two stars whose places had been already ascertained.

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* Delambre, Hist. Ast. Moyen Age, p. 129. The astronomers of India appear also to have practised this method by tracing upon the sand a circle around the foot of the

He is also the first astronomer who used clocks moved by weights for the purpose of measuring time. These pieces of mechanism were introduced originally from eastern countries. Pacificus, Archdeacon of Verona, who flourished in the ninth century, was the first European who constructed a clock with wheels. Others were soon fabricated in different countries of Europe, but they were used only in the public edifices of great cities before Waltherus applied them to the purposes of astronomy.

In consequence of the observations of Waltherus being the earliest of modern times, they were continually used at a subsequent period in rectifying the elements of astronomical science by comparing them with similar determinations of a more recent date. Lacaille has employed them for this purpose in his researches on the solar theory. This is probably the last important occasion on which the observations of Waltherus will be consulted. In the present day it is found that the modern observations more than compensate by their superior accuracy for the shortness of the time that has elapsed since they were made.

Copernicus, about the middle of the sixteenth century, had restored the true system of the world, but the epicyclical fabric still remained almost in its original state. To Tycho Brahé is due the immortal honour of having executed the series of observations which led to the discovery of the real nature of the planetary movements. This illustrious individual expended his whole private fortune, amounting to a hundred thousand crowns, upon objects connected with the improvement of practical astronomy. His collection of instruments was one of the most magnificent which the world has ever seen. Waltherus, and the earlier astronomers of modern Europe, possessed only wooden appliances for observing the positions of the celestial bodies. The instruments of Tycho Brahé were mainly constructed of metal. They were also larger and more accurately divided than any that had been hitherto applied to the purposes of astronomical observation. The ancient astronomers appear to have divided their instruments only to every ten minutes. Some of Tycho Brahé's were actually divided to every minute; and by employing the method of transversals, which came into use in his time, they were, in some instances, subdivided to every ten seconds*.

Not only were Tycho Brahé's instruments vastly superior to those hitherto used by astronomers, but his methods of observation were also such as to assure a greater degree of precision to the final results than had been hitherto attained. By taking into account the effect of refraction, he avoided, to a great extent, a source of error which had vitiated the observations of all preceding astronomers. His method of determining the absolute right ascensions of the stars was also a vast improvement over that employed by the ancient astronomers. It has been already mentioned that Hipparchus determined the places of the stars with respect to the equinox, by using the moon as an intermediate object of observation between the sun and the stars. This method, however, was very imperfect, on account of the rapid motion of the moon and the uncertainty that prevailed respecting the exact value of her parallax. Some of Tycho Brahé's contemporaries, among others the Landgrave of Hesse Cassel, sought to determine the absolute place of a star by means of its *azimuth* and *altitude*, combined with the *time* of observation. A serious objection to this method consisted in the difficulty of measuring time with sufficient accuracy. Notwithstanding all the efforts of Tycho Brahé and the Landgrave, they were unable to procure clocks that could be relied upon for such a purpose. Tycho has not failed to enumerate the various sources of their irregularity. They were perpetually liable to be affected by changes of temperature and currents of air, even although the precaution was used of keeping them in heated apartments. Again, it was found impossible to construct the teeth of wheels with such regularity as to ensure a perfectly uniform motion; and hence, even although the daily times of restitution of such clocks should appear equal, by comparing them either with the sun or the stars, they would fail to indicate the intermediate intervals of time with the same regularity. Finally, the heavy body which occasioned the motion did not act uniformly during its descent, for when it was in the lower part of its course, the weight of the cord by which it was attached tended in some degree to increase its efficacy, however slender the cord might be; and the smallest irregularity could not be overlooked, since an error of four seconds in time was equivalent to one of a minute in space *.

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* It may be remarked, however, that in his actual observations Tycho Brahé by no means attained the degree of precision contemplated in the construction of some of his instruments; for even although the divisions of the limb had been faultless in any case, the advantage of the instrument in this respect would have been vitiated by the error committed in the pointing of it to any celestial body, an error which it was impossible to avoid so long as observations were made with plain sights. With respect to the method of transversals, or, as it is more commonly termed in England, the *diagonal scale*, Tycho Brahé mentions (*Progymnasmata*, p. 671, edit. 1610) that he first became acquainted with it when he was a student at the university of Leipsic, having obtained a description of it from one of the professors named Homelius, but that he is ignorant of the real author of the invention. Thomas Digges (*Alæ seu Scalæ Mathematicæ, Capitulum Nonum; Londini*, 1573) asserts that it was invented by Richard Chanzler, an English artist famous for his skill in the construction of mathematical instruments, and that it had been long well

While pondering on the difficulties of this subject, the happy idea occurred to Tycho Brahé of substituting Venus for the moon, as the intermediate object of observation between the sun and the stars. This planet has a much slower motion than the moon, and at the same time has a smaller parallax. It is therefore obviously better adapted than that body for ascertaining an element of fundamental importance, such as the absolute right ascension of a star. Before sunset he made simultaneous observations on the sun and the planet, and when the sun disappeared he made similar observations on the planet and the star. In this manner he determined the absolute right ascensions of several of the most brilliant stars, and by means of them he easily obtained the right ascensions of all the others †. It is worthy of remark that Waltherus had already employed Venus for a similar purpose, but this circumstance seems to have totally escaped the notice of Tycho.

Allusion has been made to the mode of determining the position of a star by means of its *altitude* and *azimuth* combined with the *time* of observation. This method seems to have been practised by the Landgrave of Hesse Cassel, on the occasion of his observations of the new star which appeared in the constellation of *Cassiopeia*, in the year 1572. Tycho, in his work on the same star, has given the observations of the prince relative to its apparent place in the heavens, extending from Dec. 3, 1572, till March 14, 1573 ‡. These appear to be the earliest observations in which the element of *time* was employed in determining the right ascension of a star.

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* Progymnasmata, p. 149. † Ibid., p. 491. ‡ Ibid., p. 152.

It appears from the work of Tycho Brahé, above cited, that Thaddeus Hagecius, another contemporary astronomer who wrote upon the new star, advanced a step further than the Landgrave; for he shewed that both the declination and right ascension of a star might be determined by observing its *altitude* on the meridian, and noting the *time* of observation. Tycho has given, as an example of this method, Hagecius's determination of the place of the new star. He takes occasion, at the same time, to point out the various defects of the method, and it must be admitted that, in the existing state of practical astronomy, his objections were not without weight. If the time was to be indicated by clocks, it would be impossible, he remarked, to avoid errors of serious magnitude, as he had already shewn on a former occasion. If the observed altitude of a star was employed for that purpose, the chance of error would be equally great, for neither could the absolute place of the star be ascertained beforehand with sufficient precision, nor could its altitude, corresponding to each observation, be satisfactorily determined. Moreover, when a star is approaching the meridian, its altitude increases very slowly, so that it is exceedingly difficult to assign the precise instant when it is actually upon the meridian. This source of error might be avoided by observing the star near the horizon, where it rises with great rapidity; but, on the other hand, the uncertain effect of refraction at low altitudes would tend to vitiate the observation *.

Tycho Brahé, for the above reasons, did not deem it safe to employ the element of *time* in determining the apparent place of a celestial body. In fact, the observations of the Landgrave on the new star, to which allusion has already been made, supplied him with a practical proof of the justness of his views on this subject. In each of these observations the *altitude*, the *azimuth*, and the *time* were different; but as the position of the star in the celestial sphere was found to be invariable, it followed, as a necessary consequence, that each combination of these three data ought to assign to the star the same values of declination and right ascension. Tycho calculated these elements by means of the Landgrave's observations; but he found that although the declinations exhibited a pretty satisfactory agreement with each other, the right ascensions differed, in some instances, to the extent of 2° of space †. This may not be an improper place for a few words on the construction of the clocks of those days. They consisted essentially of a vertical wheel, which was made to revolve with a slow motion by the gradual descent of a weight. The balance was a happy contrivance, devised for the purpose of constantly checking by its inertia, the descent of the weight, and thereby rendering the motion of the wheel more uniform. It consisted of a crossbar, with two weights at its extremities, capable of turning freely in a horizontal direction, upon a vertical axis, which was connected with the wheel by means of two pallets. The distance between the pallets was equal to the diameter of the wheel, and they were so disposed on the vertical axis as to engage alternately the highest and lowest tooth of the wheel. When the highest tooth engaged the upper pallet, the balance was pushed round, and its resistance checked the descent of the weight. As soon as this pallet became freed from the wheel, the lowest tooth engaged the other pallet, and the balance was then pushed in the opposite direction. This alternate movement of the balance continually restrained the descent of the weight, and thereby prevented the acceleration of the motion of the wheel, which would otherwise have necessarily ensued. Clocks upon this principle continued to be fabricated down to the middle of the seventeenth century; but, notwithstanding all the attention and skill bestowed on their construction, they could not be relied upon for the delicate purposes of astronomy.

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* Progymnasmata, p. 523.

† Ibid., p. 502.

Although Tycho Brahé generally disapproved of employing the element of time in determining the apparent position of a celestial body, he adopted the practice in one or two instances, which it may not be improper to notice. In the first place, he determined the successive positions of the comet of 1577, by means of altitude and azimuth observations. The instrument he used on this occasion was a quadrant revolving in azimuth. He possessed several instruments of this construction. The largest had a radius of six feet. It was divided by transversals to every quarter of a minute. The azimuth circle had a diameter of nine feet. The zero point of division was at the extremity of the meridional diameter, the direction of which was determined by observing the greatest eastern and western digressions of the pole stars.

Another instrument designed by Tycho Brahé, to be used in connexion with the clock, was what he termed the *Mural* or *Tychonic* quadrant. It was a large quadrant of copper, placed exactly in the plane of the meridian against a solid wall, to which it was firmly attached. The radius of this instrument was equal to nine feet, and the limb was divided by transversals to every 10″. Tycho remarks that by determining the altitude of a star with it, and noting the time of observation, the declination and right ascension of the star may be ascertained. He appears, however, to have chiefly used this instrument in making observations of the meridional altitude of the sun, for the purpose of rectifying the elements of the solar theory[*].

The Landgrave of Hesse Cassel, whom we have had occasion to allude to in connexion with Tycho Brahé, was a prince no less distinguished by his liberal patronage of astronomical science, than by his actual labours as an observer of the heavens. Unlike most other sovereigns who have affected a disposition to promote the objects of astronomy, he did not content himself with a mere *dilettante* exhibition of his favours towards the science, but directly contributed towards its advancement by his personal energy and perseverance. It is to be remarked, also, respecting this excellent prince, that he maintained at his own expense some of the most eminent mathematicians and astronomers of his time; nor should his kindness towards Tycho Brahé be forgotten in considering his claims to the grateful recollection of posterity.

We shall now briefly allude to the labours of Hevelius, before proceeding to give some account of the modern improvements in practical astronomy. This individual, who was a rich citizen of Dantzic in Prussia, commenced his career as an astronomer in 1630; and throughout a period of nearly fifty years he continued to labour with indefatigable assiduity in making observations of the celestial bodies[†]. His instruments were similar to those of Tycho Brahé; but they were divided with greater precision, and were more easily managed. The application of the vernier to the measurement of fractional magnitudes, was an improvement of which he did not fail to avail himself. This elegant method of subdivision was invented in the year 1631 by Pierre Vernier, a native of Franche Compté*. Hevelius himself devised an ingenious method of effecting the same object by means of a tangent screw, working in the extremity of the movable radius upon which the pinnules were placed. Knowing the number of revolutions of the screw which were necessary to transport the radius over the interval included between any two consecutive points of division of the limb, it was hence easy, by counting the revolutions and parts of a revolution of the screw, in moving the radius from any point on the limb to the nearest point of division, to ascertain the magnitude of the space included between the two points. This must be considered as one of the earliest of those *mechanical* contrivances that have been devised for measuring the fractional parts of divided instruments. Hevelius, by refusing to employ the telescope in his observations, was unable to take due advantage of this ingenious method of subdivision; but it was applied with complete success in several instances in the following century.

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[*] Astronomiæ Instauratæ Mecanica, No. 4.

[†] Hevelius was born in 1611, and died in 1687. Although he observed an eclipse of the sun as early as the year 1630, he did not commence his career of regular observation till 1641. One of his most useful assistants was his wife, who was an expert observer,

In 1679 the observatory of Hevelius was totally consumed by fire. All his instruments and manuscripts, and, with the exception of a few copies, the whole edition of the second part of his great work, the *Machina Cœlestis*, comprehending the numerous observations which he had made during his long career, unfortunately perished in the flames. The aged astronomer bore this calamity with great fortitude; and, instead of abandoning himself to despair, he recommenced his labours with renewed energy†. One of the most remarkable of his works is a catalogue of the places of upwards of 1500 stars, which was published a few years after his death. The great ability of Hevelius, as an observer, would have rendered this catalogue a valuable record of the state of the heavens in his time, if, in making his observations, he had availed himself of telescopic sights, which came into general use during the latter part of his career. Unfortunately, however, he persevered to the end of his life in rejecting such appliances, adhering to the use of simple pinnules, under the impression of their affording a more trustworthy indication of the true direction in which a celestial body appears.

With the labours of Hevelius closes the ancient school of astronomical observation. By constructing instruments of very large size, and employing ingenious methods of subdivision, astronomers had succeeded in determining the apparent position of a celestial body within about 1′ of the truth. If the magnitude of the instrument in any instance, however, exceeded a certain limit, it obviously became unmanageable; and besides, its greater tendency to derangement from its own weight more than counterbalanced the advantage gained by enlarging the scale of division. But apart from these considerations, the difficulty of pointing truly to a celestial body an instrument furnished for that purpose merely with pinnules, presented an insuperable obstacle to the attainment of a high degree of precision in the ascertainment of its apparent position. This was a constant source of error which the astronomer had to contend with in all his operations, whose vitiating influence was essentially beyond the reach either of the ingenuity of the mechanician or the skill of the artist. It is to be remarked, however, in favour of the Tychonic school of observation, that it served effectually to demolish the whole of the ancient fabric of epicycles, and to establish the elliptic theory of the planets upon an incontrovertible basis. This was unquestionably a noble triumph of genius and practical skill; but in so far as regards the more delicate phenomena of the celestial regions, the methods practised in that school were utterly powerless. The sublime theory of planetary perturbation would have proved a subject of barren research to Newton and his successors, and those minute movements of the stars, in connexion with which so many brilliant triumphs have been achieved in recent times, would have for ever continued to be involved in impenetrable mystery, unless a complete revolution had been effected in the condition of practical astronomy as it existed about the middle of the seventeenth century.

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* A method somewhat similar in principle, but more complicated in practice, had been invented by Nonius, a Portuguese mathematician, about the middle of the sixteenth century. Tycho Brahé used it in the early part of his career, but he soon abandoned it as too troublesome, adopting the method of transversals in its stead.

† What Hevelius regretted on this occasion more than all his most precious effects were his researches on the planetary motions, and his notes on the stars, which he had been making throughout the long period of his astronomical career. Several years afterwards he assures the reader, in one of his works, that the recollection of their loss never

The establishment of public observatories in several countries of Europe about the time referred to, could not fail to exercise a direct influence of a favourable character upon the progress of astronomy in all its branches. When it is considered that the various movements of the celestial bodies require for their development a series of cycles of time, many of which are of long duration, it is manifestly indispensable, in order to arrive at an accurate knowledge of the laws of their movements, that the various bodies should be made the objects of *continuous* observation throughout a long succession of ages. But, on the other hand, it is not less obvious that the limited resources of private individuals are inadequate to a task of such magnitude. Much, indeed, may be effected by genius and perseverance, as in the case of Hipparchus, and several astronomers of modern times; but, generally speaking, it is only by the steady application of national resources to the observation of the celestial bodies, that a series of facts can be established which shall possess sufficient value to form a sure groundwork for the investigation of the laws of their movements. The earliest public observatory, and indeed the only one which history makes mention of as having existed in ancient times, was that which Ptolemy Soter established at Alexandria, and which continued for many centuries to furnish an asylum to the Greek astronomers in the midst of a declining civilisation. The magnificent establishment which Frederick III., of Denmark, assigned to Tycho Brahé, in the island of Huena, for the purpose of enabling him to prosecute his labours more effectually, cannot be called a public observatory, since it was not designed to promote any ulterior object connected with astronomy. But indeed, even in so far as Tycho Brahé himself was concerned, it was soon apparent, from the disgraceful treatment which he experienced in the latter period of his life, that the establishment of Uranibourg was held by a very precarious tenure. It would seem, however, that Christian IV., under whose minority Tycho Brahé was compelled to abandon his native country, felt a disposition to make some reparation for his early indifvatory on a permanent basis. Longomontanus, an astronomer of some celebrity, who had assisted Tycho in his labours at Uranibourg, was chiefly instrumental in promoting the views of the monarch. The foundation stone of the observatory was laid by Christian IV., on the 7th of July, 1637, but it does not appear that the building was completely finished until the year 1656, during the reign of his successor, Frederick IV. The structure was erected in the form of a tower. It was 115 Danish feet in height, and 48 feet in diameter. The observations were made in an apartment at the top of the tower, which was reached by means of a spiral avenue winding gently round the interior of the wall*. We shall presently have occasion to allude more particularly to this observatory, which must be regarded as the earliest of the numerous public institutions for the promotion of practical astronomy that have been established in modern times.

It has been already mentioned that all contrivances for measuring time by means of clocks composed of a combination of wheels moved by weights, had been found totally inadequate to the delicate purposes of astronomy. Another mode of effecting the same object had been devised by Galileo. Having discovered that the oscillations of a pendulum are isochronous†, it occurred to him that a mechanism might be constructed upon this principle which would serve to indicate the divisions of time with great accuracy; but although he devoted much attention to the realisation of this idea, all his efforts to procure a clock that would be practically serviceable proved fruitless. In all such contrivances it was found that the pendulum, when once set in motion, was soon again brought to a state of rest, so that the oscillations could only be maintained by applying to the pendulum a succession of impulses at regular intervals. Huyghens eluded this inconvenience, by substituting the pendulum for the balance in the ancient clocks, so adapting it, that the isochronism of its oscillations regulated the downward motion of the weight. By this admirable contrivance he combined the advantages of the two methods above referred to, while, at the same time, he eluded their respective defects. The idea of so applying the pendulum to the measurement of time first suggested itself to him in the year 1656, and in the following year he presented to the States General of Holland a clock constructed upon this principle. The invention soon spread abroad throughout Europe, and was hailed with universal applause. Its successful application to the purposes of astronomical observation will be noticed presently.

The establishment of the Royal Society of London, and of the Academy of Sciences of Paris, exercised a powerful influence on the progress of astronomy, as well as on every branch of physical science. The origin of the Royal Society may be traced to the period of the civil wars in the time of Charles I., when individuals of a contemplative cast of mind sought relief from the prevailing commotion in the calm retreat of scientific discussion. Dr. Wallis states that, about the year 1645, when academical studies were much disturbed at both the universities, several persons inquisitive into natural philosophy, and other parts of human learning, did, by agreement, meet weekly in London on a certain day to treat of such matters. Among the individuals who thus used to assemble together, were Wilkins, Goddard, Wallis, and Samuel Foster, Professor of Astronomy in Gresham College. In 1648–9 the attendance at the weekly meetings was considerably reduced by the removal of several members to Oxford, who established a similar association in that city. The original minutes of the Oxford Philosophical Society, dated the 23rd of October, 1651, are still preserved in the Ashmolean Museum of that city. The embryo Society of London still continued to hold its meetings, but it seems to have fallen into a very languishing condition until the year 1660, when the restoration of Charles II., by holding out the prospect of future tranquillity, had the effect of infusing renewed vigour into its proceedings. The earliest official document of the Society is dated November 28, 1660. It was finally incorporated by Royal Charter on the 15th of July, 1662. The first President was Lord Brouncker, one of the most eminent mathematicians of the age. Among those members who took an active part in promoting the objects of the Society at this period, may be mentioned the President Lord Brouncker, Boyle, Wilkins, Wren, and Sir Robert Moray. A few years after the establishment of the Royal Society upon a permanent basis, a similar association was formed at Paris under the designation of the *Royal Academy of Sciences.* The first meeting of this celebrated body was held on the 22nd of December, 1666. Some of the earlier members were Roberval, Auzout, Picard, and Richer. A few years later, it was enriched by the accession of Huyghens, Roemer, and Cassini, who were enrolled as members.

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* Horrebow, in his *Basis Astronomiæ*, states that in 1716 Peter the Great of Russia frequently ascended the tower on horseback along this spiral avenue, and that the Empress Catherine occasionally ascended at his side in a four-wheeled chariot, drawn by six horses!

† They are not *exactly* isochronous, but for small arcs the times of oscillation may be regarded as equal without committing any sensible error.

Numerous institutions for promoting the cultivation of science have been established in the different countries of Europe at a later period than that above referred to; but it is no depreciation of the well-earned reputation of many of them to assert that, at no epoch of their history have they in any instance exercised an influence so unequivocally beneficial as that exercised by the Royal Society of London, and the Academy of Sciences of Paris, during the early period of their existence. It is to be borne in mind that the last-mentioned institutions were founded at a time when the inductive philosophy was still engaged in mortal conflict with the scholastic system of Aristotle, and when, consequently, an organized union of the scattered energies of its adherents could not fail to accelerate its final triumph. Practical astronomy participated largely in the advantages derived from these institutions. New improvements, both in the construction of instruments and in the methods of observation, were disseminated throughout Europe with a rapidity hitherto unknown, and thereby facilitated in a vast degree the attainment of still further excellence.

The invention of the micrometer was one of the earliest of those improvements which in the seventeenth century had the effect of establishing the methods of astronomical observation upon a totally new basis. The measurement of small angles was found by astronomers, in all ages, to be one of the most difficult objects connected with the practice of observation. Archimedes, in an attempt to determine the apparent magnitude of the sun, was unable to arrive at a result of greater precision than that the apparent diameter was greater than 27′, and less than 32′. Coming down to modern times, we have seen that Tycho Brahé was so misled by his measurements of the apparent diameters of the sun and moon, as to come to the conclusion that a total eclipse of the sun was impossible. Indeed it obvious that a mode of accurately determining the variations in the apparent diameters of the sun and moon, could not fail to lead to an improvement of the theories of the movements of those bodies. Moreover, the invention of the telescope, by revealing the round figures of the planets, rendered it desirable to devise some means of measuring small angles with a degree of precision conformable to the existing condition of theoretical astronomy. The invention of the micrometer, by means of which this object was effectually accomplished, is originally due to William Gascoigne, a young man who was the friend of Horrocks and Crabtree, and who, like them also, was unfortunately cut off by a premature death in the very outset of his career. It will be desirable, however, to give a previous account of the successive steps by which the same admirable invention was independently effected on the Continent about twenty years afterwards. The person who first suggested the idea of the micrometer in the latter instance was Huyghens. In his *Systema Saturnium*, which was published in 1659, he remarked that an object placed in the focus of a common astronomical telescope, appears as distinct and as well defined as the image of a remote body. Hence, in order to determine the apparent diameter of a celestial object, he inserted a slip of metal of variable breadth at the focus of the telescope, and observed at what part it exactly covêred the object. Applying, then, a finely-pointed compass to the slip of metal, he measured its exact breadth, and, knowing the focal length of the telescope, he hence deduced the apparent magnitude of the object. In this manner he determined the apparent diameters of the principal planets.

The Marquis Malvasia, in his "Ephemerides," published at Bologna in 1662, describes a method of measuring small angles which may be regarded as the next step in the invention of the micrometer. At the focus of the telescope he placed a net-work of fine silver threads at right angles, which formed by their mutual intersection a number of small squares. In order to determine the mutual distances of the threads that were parallel to each other, he caused a star situate near the equator to traverse a thread perpendicular to their common direction, and counted, by the aid of a pendulum clock, the times which it took to pass over the successive intervals*.

The micrometer of Malvasia was a decided improvement of that of Huyghens. The distances between the threads, when once accurately determined, would serve for all future measurements. The method was also more generally applicable than that of Huyghens, since, besides the apparent diameters of the celestial bodies, it might serve to measure the angular distance of two stars that were at the same time in the field of view of the telescope. It laboured, however, under the disadvantage that, if the two stars did not appear exactly on the wires, something was unavoidably left to estimation in the measurement of their mutual distance. Auzout got rid of this defect by substituting for the reticule of Malvasia two silk threads, one of which was fixed, while the other was moveable in a direction parallel to it by means of a fine screw. It is evident that, when once the distance traversed by the moveable thread during one revolution of the screw was ascertained with sufficient accuracy, the angular distance between two stars very near to each other might be determined, by causing the moveable thread to traverse the interval between the stars, and noting the number of revolutions and parts of a revolution of the screw which were required to effect this object.

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* Mém. Acad. des Sciences, 1717, p. 78, et seq.

Auzout first announced his improvement of the micrometer in a letter to the Royal Society of London, dated December 28, 1666, which is inserted in the 21st Number of the *Philosophical Transactions.* Birch states that, upon this letter being read before the Society, two of the members, Wren and Hooke, took occasion to mention several ways of measuring small angles which were already known in England. These philosophers have nowhere given a detailed description of the micrometers devised by them. Sprat, however, in his "History of the Royal Society," alludes, in a general summary of the labours of the Society, to their construction of telescopes of different lengths, with *several contrivances in them for measuring the diameters and parts of the planets* *. In another part of the same work, he states that Wren had added many sorts of retes, screws, and other devices to telescopes, for taking small distances and apparent diameters to seconds †. In a work which he published in 1665, Hooke proposes to determine the parallax of the moon or any of the planets by observing its distance from the neighbouring stars in two different latitudes, by the aid of a telescope fitted with a *rete or divided scale* ‡. It is evident that the micrometrical contrivances of Huyghens, the Marquis Malvasia, and Hooke, were all essentially similar to each other. Indeed, as soon as telescopes composed of two convex lenses began to be commonly used, the invention of such a micrometer as that devised by either of these philosophers was a very obvious step.

In the 25th Number of the *Transactions* of the Royal Society there is a letter from Richard Townley, of Lancashire, relative to the micrometer of Gascoigne. The date of this letter is March 25, 1667 §. "Finding," says he, "in one of the last *Philosophical Transactions*, how much M. Auzout esteems his invention of dividing a foot into near 30,000 parts, and taking thereby angles to a very great exactness, I am told I shall be looked upon as a great wronger of our nation, should I not let the world know that I have, out of some scattered papers and letters that formerly came to my hands of a gentleman of these parts, one Mr. Gascoigne, found out that, before our late civil wars, he had not only devised an instrument of as great power as Mr. Auzout's, but had also for some years made use of it, not only for taking the diameters of the planets and distances upon land; but had further endeavoured out of its preciseness to gather many certainties in the heavens ‖ . . . . I shall only say of it that it is small, not exceeding in weight, nor much in bigness, an ordinary pocket watch, exactly marking above 40,000 divisions in a foot, by the help of two indexes; the one showing hundreds of divisions, the other divisions of the hundred." Mr. Townley concludes his letter by stating that, if it should be found desirable, the Society might obtain a more complete description of the instrument.

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* "History of the Royal Society," p. 250, 4to, London, 1667. The account of the scientific labours of the Royal Society, inserted in this work, was compiled from the registers of the Society down to 1665.

† Ibid., p. 314.

‡ Micographia, p. 237, fol., Lond., 1665. Hooke, on a subsequent occasion, described a species of micrometer of a more perfect construction, but he was then acquainted with the contrivances for a similar purpose devised by Gascoigne and Auzout.

§ The date of the letter is not mentioned in the number of the *Philosophical Transactions* cited in the text, but the omission is supplied by Birch, who alludes to the letter in his *History of the Royal Society* (vol. ii. p. 164). This letter, as well as the statement respecting it by Birch, has entirely escaped the notice of Delambre, who censures the tardiness of the English astronomers in not bringing forward any account of Gascoigne's micrometer before the 25th of July, 1667.

‖ Townley here states that Gascoigne determined the value of the lunar parallax by measuring with his micrometer the apparent diameter of the moon when she was in the horizon, and again when she was on the meridian. He mentions also that he had in his possession the very micrometer that Gascoigne originally used in his observations.

Mr. Townley's micrometer was actually produced before the Society at the meeting held on the 25th of July, 1667, and a detailed description of it was subsequently given in No. 29, of the *Philosophical Transactions.* In principle it exactly resembles the micrometer of Auzout. Two straight edges of metal are made to approach each other at the focus of the telescope by means of a screw, the mechanism being so contrived that the optical axis of the telescope is always situate midway between the two edges. The number of revolutions and parts of a revolution of the screw which are required in order to bring the edges into any determinate position will manifestly indicate the extent of the interval which separates them [*]. Hooke suggested an improvement of this micrometer by substituting human hairs for the solid edges.

It appears from the observations of Gascoigne, which are still in existence, that he actually used the micrometer in several delicate measurements. Flamsteed, in the first volume of the *Historia Celestis*, has inserted a series of celestial observations extending from 1638 to 1643, which were made by Gascoigne and his friend Crabtree, and among these are contained a considerable number of micrometrical measurements by Gascoigne. They consist of a determination of the mutual distances of the Pleiades, a few values of the apparent diameter of the sun, several distances of the moon from the neighbouring stars, and a great number of measurements of the lunar diameter. The results in most instances are given in revolutions and parts of a revolution of the screw. In the forty-eighth volume of the *Philosophical Transactions*, Dr. Bevis has also given several measurements of the apparent diameters of the planets and the moon, made by Gascoigne with his micrometer. They were extracted by him from a letter written by Gascoigne to Oughtred, the original of which, he stated, was still preserved in the library of the Earl of Macclesfield [†]. The following comparison instituted by Delambre, between certain measurements of the semidiameter of the sun by Gascoigne, and the corresponding values in the *Connaissance des Temps* of the present day, will suffice to convince the reader that the original inventor of the micrometer did not fail to bring his instrument to considerable perfection:—

Gascoigne.		Connaissance des Temps.
October 25, O.S.	16′ 11″ or 10″	16′ 10″
October 31,	16 11	16 11.4
December 2,	16 24	16 16.8

Gascoigne makes the greatest variation of the apparent diameter of the sun to be 35″; according to the *Connaissance des Temps* it amounts to 32″.3 *.

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[*] Sherburne, in his translation of the Sphere of Manilius (1675), states that Crabtree, taking a journey to Yorkshire, in 1639, to see Gascoigne, thus wrote to Horrocks:— "The first thing Mr. Gascoigne shewed me was a *large* telescope, amplified and adorned with new inventions of his own, whereby he can take the diameters of the sun or moon, or any small angle in the heavens or upon the earth, most exactly through the glass to a second (see the work cited, p. 92). This is the letter alluded to in the note at the foot of p. 428.

[†] Phil. Trans., 1753, p. 190.

It appears from the foregoing statements that Gascoigne invented and actually employed the micrometer twenty years before even the original germ of such an instrument had suggested itself to any person on the Continent. At the same time, there is not the smallest reason to suppose that any person out of England had any knowledge of his labours previous to the reinvention of the instrument elsewhere. On this point Delambre makes the following just remark:—"*Il faut remarquer que les observations des deux amis n'ont été publiées que 80 ans après leur date réelle. Ainsi, en accordant à Gascoigne, la première idée et même la première exécution du micromètre, il est juste de réserver leurs droits aux astronomes qui, sans avoir aucune connaissance des observations Anglaises, ont été conduits par leur propres réflexions à la même découverte.*" †

There can be no doubt that the micrometer of Gascoigne and Auzout is immeasurably superior to any reticule or graduated scale placed in the focus of the telescope, such as was used by Huyghens and the Marquis Malvasia on the Continent, and by Hooke in England. It is right to bear in mind, however, that both Wren and Hevelius had suggested the measurement of small angles, and of the fractional parts of large angles, by means of the revolutions of a screw, although it does not appear that on any occasion they reduced their ideas to practice.

William Gascoigne, the individual just referred to, was the son of Henry Gascoigne, Esquire, of Middleton, in Yorkshire. While only eighteen years of age he appears to have been actively engaged in observing the celestial bodies, and in advancing the state of optics and practical astronomy. He continued to prosecute his labours with remarkable success till the outbreak of the civil wars in 1643, when he joined the cause of Charles I., and fell at the battle of Marston Moor on the 2nd of July, 1644, in the twenty-fourth year of his age. To his untimely fate may be applied the language of a distinguished historian in reference to the accomplished young statesman who, in the previous year, fell at the battle of Newbury, fighting in the same cause:—"*Il mourut, victime d'un temps trop rude pour sa pure et tendre vertu.*" ‡ Richard Townley states that Gascoigne had a Treatise on Optics just ready for the press, but that notwithstanding all his efforts he was unable to obtain any trace of it after his death §. Sherburne, in his translation of "The Sphere of Manilius," asserts that Gascoigne was the first person who made a telescope composed of two convex lenses ||. Whether this be true or not, it is very certain, at all events, that he was the first who applied a telescope of such a construction, to those purposes for which it is peculiarly designed.

The application of the telescope to divided instruments, so as to serve in ascertaining the apparent direction of a celestial body, was another of those great improvements which distinguished the progress of practical astronomy in the seventeenth century. The idea of employing the telescope in this manner seems to have first suggested itself to Morin, a French astronomer, notorious for his opposition to the Copernican theory of the universe, and his devotion to astrological pursuits. In 1635 he ascertained the interesting fact, that it was possible to see the stars in the daytime by the aid of the telescope, and he sought to avail himself of

* Hist. Ast. Mod., tome ii., p. 590.

† Ibid.

‡ Guizot, *Histoire de la Revolution d'Angleterre*, livre iv.

§ Phil. Trans., 1667, p. 457.

his discovery in devising a new solution of the problem of the longitude. His method was founded on a comparison of the observed and computed distances of the moon from the stars, but although he succeeded in seeing the stars in the daytime, he in vain sought to fix the positions of either the moon or the stars by the aid of the telescope. Indeed, so long as astronomers confined themselves to the telescope as originally constructed by Galileo, it was impossible to accomplish such an object, since there was no place of distinct vision at which two cross threads could be placed to indicate, by their intersection, the invariable position of the optical axis of the instrument. It is a remarkable fact, that the unfortunate Gascoigne was the first person who successfully used the telescope in ascertaining by observation the apparent positions of the celestial bodies. Derham, in a paper which appears in the *Philosophical Transactions* for the year 1717, has satisfactorily established the claims of Gascoigne upon the evidence of certain letters which passed between him and his friend Crabtree *. These letters were originally in the possession of Richard Townley, the individual to whom we have already had occasion to allude in connexion with the invention of the micrometer. It may conduce to a clearer understanding of the following extracts from these letters, to bear in mind that the last-mentioned invention affords demonstrative proof that Gascoigne employed, in his astronomical observations, telescopes composed of two convex lenses.

In a letter to Crabtree, dated January 25, 1640–1, Gascoigne thus writes:—"If here (in the focus of the telescope) you place the scale that measures . . . . . or if here a hair be set that it appear perfectly through the glass . . . . . you may use it in a quadrant, for the finding of the altitude of the least star visible by the perspective wherein it is. If the night be so dark that the hair, or the pointers of the scale, be not to be seen, I place a candle in a lanthorn, so as to cast light sufficient into the glass, which I find very helpful when the moon appeareth not, or it is not otherwise light enough." In another letter to Crabtree, dated on Christmas eve, 1641, he thus writes:—"Mr. Horrocks his theory of the moon I shall be shortly furnished to try, for I am fitting my sextant for all manner of observations, by two perspicills with threads. And also I am consulting my workman about the making of wheels like β, γ, δ, ϵ, of diagram 3, to use two glasses like a sector. If I once have my tools in readiness to my desire, I shall use them every night. I have fitted my sextant by the help of the cane, two glasses in it, and a *thread*, so as to be a pleasant instrument, could wood and a country joiner or workman please me."

In another letter, the date of which was found by Derham to be worn out, but which was marked as his tenth letter, in Crabtree's own handwriting, he says:—"I have caused a very strong ruler to be exactly made, and intend to fit it with cursors of iron, with glasses in them, and a thread for my sextant."

The following extract of a letter from Crabtree to Gascoigne, dated October 30, 1640, will tend still further to establish the same point:—"You told me, as I remember, you doubted not in time to be able to make observations to seconds. I cannot but admire it, and yet, by what I saw, believe it; but long to have some farther hints of your conceit for that purpose. One means, I think, you told me was, by a single glass in a cane, upon the index of your sextant, by which, as I remember, you find the exact point of the sun's rays." In another part of the same letter he says:—"Could I purchase it with travel, or procure it for gold, I would not be without a telescope for observing small angles in the heavens; nor want the use of *your other device* of a glass in a cane upon the moveable ruler of your sextant, as I remember, for helping to the exact point of the sun's rays."*

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* Phil. Trans., 1717, p. 603.

The perusal of the foregoing extracts cannot leave a shadow of a doubt in the mind of the reader that Gascoigne distinctly perceived the advantage of telescopic sights in fixing the position of a celestial body, and that he had actually reduced his ideas to practice, in making observations with the quadrant and the sextant. To him, therefore, must be awarded the credit due to the original invention of this important method of observation. It has been already remarked that the application of the Galilean telescope to that purpose was impracticable. The above-cited extracts from Gascoigne's letters clearly prove that he had succeeded in fixing the position of the optical axis of his telescope, and in this respect they tend to corroborate the assertion of Sherburne that he was the first who constructed a telescope composed of two convex lenses.

The materials which serve to establish the interesting fact that Gascoigne employed telescopic sights in observing the celestial bodies, were not published until early in the beginning of the eighteenth century, when the same mode of observation had been already known to astronomers for about fifty years. The improvement which he thus effected can only be regarded, therefore, as forming the groundwork of an interesting episode in the history of practical astronomy, rather than as constituting a link in the chain of gradual advancement. In this respect it differs from his invention of the micrometer, which continued after his death to be made use of in the north of England, although it appears to have been unknown for many years both in London and on the Continent.

The earliest allusion to the use of the telescope in observing the positions of the celestial bodies is contained in Sprat's "History of the Royal Society." In a general exposition of the labours of the Society, to which reference has already been made, he makes the following statement: "They (the Fellows of the Society) have suggested the making of a perfect survey, map, and tables of all the fixed stars within the zodiac, both visible to the naked eye and discoverable by a six-foot telescope with a large aperture; towards the observing the apparent places of the planets with a telescope both by sea and land. This has been approved and begun, several of the Fellows having their portions of the heavens allotted to them."†

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* The following extracts from the same collection of letters exhibit a beautiful illustration of the feeling which ought to reign between persons engaged in scientific pursuits, whether during the period of youthful enthusiasm or that of mature manhood. In a letter to Gascoigne, dated December 8, 1640, Crabtree thus writes:—"My friend, Mr. Horrocks, professeth that little touch which I gave him of your inventions, hath ravished his mind quite from itself, and left him in an extasie between admiration and amazement. I beseech you, sir, slack not your intentions for the perfecting of your begun wonders. We travel with desire till we hear of your full delivery. You have our votes, our hearts, and our hands should not be wanting if we could further you." In another letter dated December 6, 1641, he thus writes to Gascoigne:—"No man that hath written of the diagram (of Hipparchus) understood it fully or described it rightly, but only Kepler and our Horrocks, *for whose immature death there is yet scarce a day which I pass without some pang of sorrow.*"

† Sprat's "*History of the Royal Society,*" p. 190. The close resemblance of this project to that in which the Berlin Academy of Sciences is engaged in the present day, cannot fail to strike the reader.

In another part of the same work the author states that the Society have made "observations of the fixed stars for the perfecting of astronomy by the help of telescopes."[*] In an account of their optical labours he asserts that they have constructed "several excellent telescopes of divers lengths of six, twelve, twenty-eight, thirty-six, and sixty feet long, with a convenient apparatus for the managing of them; and several contrivances *in them* for measuring the diameters and parts of the planets, *and for finding the true position and distances of the sun, fixed stars, and satellites*."[†]

In an account of the inventions of Wren (afterwards Sir Christopher), one of the Fellows of the Society, Sprat makes the following statement:—"He has made two telescopes to open with a joynte like a sector, by which observers may infallibly take a distance to half minutes, and find no difference in the same observation reiterated several times, nor can any warping or luxation of the instrument hinder the truth of it."[‡]

This seems to be the same instrument as the one mentioned by Hooke in his "Animadversions upon the *Machina Celestis* of Hevelius," wherein he states—in reply to an objection made by Hevelius that telescopic sights had never been applied to large instruments—that he had then by him an instrument of Sir Christopher Wren's invention, furnished with two perspective sights of six feet long each, which he made use of for examining the motions of the comet of 1665 [§].

It appears that as early as the year 1665, Hooke wrote a letter to Hevelius strongly recommending him to employ telescopic sights in preference to the ordinary pinnules [‖]. At the meeting of the Royal Society held on the 4th of November, 1667, there was read a letter from Hevelius, dated October 21 of the same year, in which he expressed a desire "of having one of the longest telescopes made in England provided for him, and of being gratified with a full description, *formerly* promised him by Mr. Hooke, of the way of applying telescopical sights to sextants."[¶]

It appears from the foregoing statements that at least as early as the year 1665 Wren and Hooke, two of the most distinguished philosophers of the age, had employed telescopic sights in observing the celestial bodies. The earliest observations made by the use of the telescope in this way, of which the details have been actually published, are those of the French astronomer Picard. In the *Histoire Céleste* of Lemonnier there is an account of observations of the meridional altitude of the sun, made by Picard in the garden of the Royal Library of Paris, with a quadrant of nine feet seven inches, and a sextant of six feet, both furnished with telescopes. The earliest of these observations is dated October 2, 1667 [**]. In his treatise on the "Measure of the Earth" the same admirable astronomer has described the different modes of verification to be employed in using instruments fitted with telescopic sights [††].

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[*] Sprat's *Hist. Roy. Soc.*, p. 241. [†] Ibid., p. 250. [‡] Ibid., p. 314.

[§] "Animadversions on the first part of the Machina Celestis," p. 42. London, 4to, 1674.

[‖] Hevelius in the same year replied to Hooke in a letter addressed to the Royal Society, for the contents of which see Hooke's *Animadversions*, &c., p. 5.

[¶] Birch, *History of the Royal Society*, vol. ii. p. 208.

[**] Histoire Céleste, p. 11.

[††] See also *Anc. Mém. Acad. des Sciences*, tome vii., p. 133.

Hooke, without seeming to be aware of what Picard had done, has also given an account of similar methods of adjustment in his "Animadversions upon the *Machina Celestis* of Hevelius."*

The establishment of the national observatories of Paris and Greenwich immediately followed the remarkable improvements in practical astronomy above alluded to. The Royal Observatory of Paris was commenced in 1667, but was not finished before the year 1671. It was a building of great magnificence, designed by Claude Perrault, the famous architect of the Louvre; but it was constructed without due regard to the purpose for which it was intended. It consisted of a quadrangular pile flanked by two massive towers. No means were provided in the construction of the building for enabling the astronomer to observe the celestial bodies at all altitudes, by means of instruments sheltered under its roof, nor was it possible to repair the omission, on account of the enormous thickness of the walls. Dominique Cassini, who had arrived in France in the year 1669, in compliance with an invitation from Louis XIV. to reside in his capital, commenced his labours at the observatory on the 14th of September, 1671. It is important to remark, however, that no special duties were assigned to him in connexion with that establishment, nor did he derive any emoluments from the French Government, in consideration of his services as an astronomical observer. A liberal pension was, indeed, granted to him upon his arrival in Paris, but this was on account of the sacrifice he had made in leaving his native country to adorn France by his talents †. His superintendence of the Royal Observatory was naturally suggested by the position which he occupied as first astronomer of France ‡, but his duties were altogether discretionary. The other astronomers who had access to the Observatory, occasionally received allowances from the Government for their services, but there was no fixed sum set apart to provide for the annual maintenance of the establishment §. The consequence of this oversight was, that no definite plan of observation was projected, by an undeviating adherence to which the advantages of a national observatory can alone be realised. This circumstance long continued to exercise an injurious influence on the progress of practical astronomy in France.

The inherent vice in the construction of the Royal Observatory of Paris has been already alluded to. It might be expected that Cassini, who arrived in France when the building was in the course of erection, and in whose opinion upon any subject relating to astronomy the French Government reposed unbounded confidence, would have effectually used his influence in preventing so deplorable a sacrifice of the interests of science to the fancy of the architect. It is true that he did not fail to remonstrate on account of the defective plan of the building, but the modifications proposed by him, shewed that his ideas were not in accordance with the existing requirements of practical astronomy. Among the alterations recommended by him, was the construction of an apartment in which the celestial bodies might be observed from east to west throughout the whole of their diurnal courses *. Such a mode of observation might have been very serviceable for the discovery of satellites, and for the examination of the physical constitution of the planets, but in so far as the determinations of the *positions* of the celestial bodies was concerned, the tendency of the age was towards observations made exclusively in the plane of the meridian with fixed instruments. The alteration proposed by Cassini was, in fact, such a one as Tycho Brahé would have recommended for the purpose of making ordinary observations of the celestial bodies with his equatorial armillæ. Another alteration suggested by him was the admission of the solar light through an aperture, so as to trace upon the floor the daily course of the sun throughout the year. By this contrivance he contemplated the formation of a vast sun-dial: he also expected to derive from such observations a more accurate knowledge of the variations of refraction. It is not difficult to perceive that such an alteration, if it had been carried into effect, would have been in reality a step backwards rather than a positive improvement. The remarkable success which attended Cassini's early efforts of a similar kind at Bologna, seems to have given a bias to his views on practical astronomy, from which he was unable completely to emancipate his mind throughout the whole course of his life. It does not seem to have occurred to him that the use of the telescope and the pendulum, introduced into the practice of observation a degree of delicacy and precision, which left all such clumsy contrivances immeasurably behind. It must be acknowledged, therefore, that notwithstanding the distinguished talents of Cassini, and the many brilliant discoveries which he made in celestial physics, the position which he occupied in France at the time of the establishment of the Royal Observatory of Paris, was unfavourable to the advancement of practical astronomy in that country.

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* One of the most remarkable methods of adjustment practised by these astronomers consisted in reversing the instrument so as to determine the error of collimation. It is a curious fact that the Arabian astronomer, Ibyn Jounis had already suggested a similar method of adjustment with respect to the gnomon. Thus, among several modes of verifying the perpendicularity of the instrument, he recommends to turn it round and make observations with it in two opposite directions. Delambre, *Hist. Ast. Moyen Age*, p. 102.

† Cassini IV., *Mémoires pour servir à l'Histoire des Sciences*, p. 186, 4to, Paris, 1810.

‡ First astronomer, at least, in the estimation of the Court of Louis XIV.

§ "Mémoires pour servir à l'Histoire des Sciences," p. 186.

Picard commenced his observations at the Royal Observatory of Paris on the 9th of July, 1673. This admirable astronomer was one of the first to perceive that the improvement effected by Huyghens in the construction of clocks completely removed the difficulty experienced by Tycho Brahé and his contemporaries in their attempts to employ the element of time in astronomical observations. It was now clearly seen that by determining the altitudes of the celestial bodies as they passed the meridian, and noting the times of observation, their declinations and right ascensions would be obtained with a degree of accuracy commensurate to the existing state of theoretical astronomy. The simplicity of this method gave it an incalculable advantage, as soon as it was found to be practicable, over any other that had been hitherto employed for a similar purpose. With the view of carrying it into effect, Picard solicited the establishment of a quadrant in the plane of the meridian, but although he continued to labour several years at the observatory, the object of his request was not accorded to him. Not having such an instrument available to him, he determined the instant of the passage of a celestial body across the meridian, by noting the times at which the body attained the same altitude on each side of that circle †. It is manifest that the time equidistant from the two observations, indicated the instant of the passage of the body over the meridian. In applying this method to the sun, Picard did not fail to take into account the correction depending upon the change of declination during the interval included between the two observations. We have seen that this method of observation, which is termed *the method of corresponding altitudes*, had been already employed by the astronomers of India as well as by the Arabian astronomers, in tracing the direction of a meridian line by means of observations of the sun. It does not appear, however, that they paid any attention to the correction above referred to. The application of the method, however, to the celestial bodies generally, and the necessity of employing a correction in the particular case of the sun, were distinctly pointed out by Thomas Digges in his "*Alæ seu Scalæ Mathematicæ*," published at London in 1573 *.

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* "Mémoires pour servir à l'Histoire des Sciences," p. 294.

† Sometimes the same object was effected by means of observations made with a telescope attached to a wall whose face was in the plane of the meridian. This instrument, termed *la lunette murale*, was also occasionally employed in determining the apparent altitudes of the stars included within a small range of observation.

Picard's labours at the Royal Observatory of Paris extend to 1682, in the autumn of which year he died. The name of this astronomer is imperishably associated with the improvements effected in practical astronomy in the seventeenth century. It cannot but be a matter of regret to every person who takes an interest in the progress of astronomical science, that he was not selected to direct the national observatory of his country. Unfortunately his labours were not calculated to attract the attention of that class of persons whose attachment to astronomy is founded solely on the pleasure to be derived from gazing at celestial phenomena. He appears, moreover, to have been a man of a retiring disposition, who devoted his talents to the cultivation of science for its own sake, regardless alike of the applause of the multitude or the patronage of the great. Hence it happened that although he was, of all the astronomers of his age, perhaps the one most qualified to superintend the duties of an observatory, he was set aside by the government of his own country, and a foreigner appointed over the national observatory, whose labours, indeed, were of a more brilliant character, but were of infinitely less importance to the progress of astronomical science than were those of Picard. It is deplorable to see the interests of a great country thus sacrificed to the caprices of a court. The circumstance may well excite the indignation not merely of Frenchmen, but of every person who is moved by the spectacle of true merit thus contemptuously overlooked, while the individual who exhibits qualities of a more meretricious nature is caressed and favoured.

La Hire commenced his observations at the Royal Observatory of Paris, in the year 1677. A mural quadrant of 5 feet radius, which Picard had long solicited in vain, was finally adjusted, and permanently fixed in the plane of the meridian on the 25th of April, 1683. La Hire continued to make meridional observations with this instrument for a period of more than thirty years; but unfortunately none of those later than the year 1686 have ever been published.

The establishment of the Royal Observatory of Greenwich was only a few years posterior to that of the similar institution of Paris. The following account of its origin is given by Flamsteed:—In 1675, a Frenchman, who styled himself Le Sieur de St. Pierre, represented to the English government that he was in possession of a method of finding the longitude *from*

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* The words of Digges are:—" . . . . Non solum per solem orientem aut occidentem, seu sub æqualibus altitudinibus deprehensum (Prosthaphærœsi elapso inter observationes tempore convenienti pro solis motû non neglectâ), sed etiam per stellas fixas omnes." (Supplementa. *Capitulum Septimum, Canon Qu*

easy celestial observations, and claimed the reward offered for such a discovery. The method which he was desirous of communicating, was founded on a comparison of the observed and calculated distances of the moon from the fixed stars. A committee, consisting of Lord Brouncker, the Bishop of Salisbury, and several other individuals, was appointed to take the subject into consideration. Flamsteed, who, through the influence of his patron Sir Jonas Moore, was nominated one of the members of the committee, was requested to provide the observations which the Frenchman demanded for the purpose of illustrating the practicability of his method. Flamsteed speedily supplied the necessary observations, but he took occasion to remark that, however accurate they might be, the method was still defective, inasmuch as the best astronomical tables sometimes erred to the extent of 12′ in the moon's place. Moreover, he stated that the method tacitly implied that the places of the fixed stars in Tycho Brahé's catalogue were absolutely correct, whereas he had found by his own observations that they were generally 5′ or 6′ in error, and in some instances even more. The commissioners agreed unanimously in the justness of Flamsteed's remarks, and, at the suggestion of Sir Jonas Moore, it was resolved to memorialise the king on the expediency of erecting an observatory for the purpose of making observations of the celestial bodies which might serve for the discovery of the longitude, since the solution of that important problem appeared evidently unattainable by any other means *.

The king gave his cordial assent to the views of the commissioners, and steps were immediately taken to carry them into effect. Flamsteed was appointed Astronomer Royal, with a salary of £100 a year. The warrant of Charles II. for the payment of his salary is dated March 4, 1674–5. He is therein styled "our Astronomical Observator," and it is declared that the duty of his office is "forthwith to apply himself with the most exact care and diligence to the rectifying the tables of the motions of the heavens, and the places of the fixed stars, so as to find out the so much desired longitude of places for the perfecting the art of navigation." The warrant for the building of the Observatory is dated June 2nd, 1675. It modestly announces the royal resolution to build a small observatory in the park at Greenwich "in order to the finding out of the longitude for perfecting navigation and astronomy." Sir Christopher Wren is charged to prepare the plan of the building, and to select a proper site for it; and the Master General of the Ordnance is instructed to advance the funds necessary for its completion, upon the condition that the expense of erection do not exceed five hundred pounds †.

The foundation stone of the observatory was laid on the 10th of August, 1675, and the building was finished in less than a year. Flamsteed took up his residence at it on the 10th of July, 1676, and shortly afterwards commenced his duties as an observer. Before proceeding further with the history of this famous observatory, it will be desirable to allude briefly to the important labours of Roemer on the Continent.

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* Baily's Life of Flamsteed, p. 125.

† Flamsteed states that, besides £500 in money, the king allowed bricks from Tilbury Fort, and some wood, iron, and lead from a gate-house demolished in the Tower; and that he also encouraged them further with a promise of what more would be requisite. According to Mr. Baily, the actual cost of erection amounted to £520 9*s*. 1*d*. (Baily's Life of Flamsteed, p. 39.)

Olaus Roemer was born at Copenhagen on the 25th of September, 1644. When Picard proceeded to Denmark in 1671, for the purpose of determining the exact position of Uranibourg, the scene of Tycho Brahé's labours, he found Roemer engaged in studying mathematics and astronomy under Erasmus Bartholinus. Having discovered that the young man was possessed of no ordinary talents, the French astronomer employed him as an assistant in his astronomical observations, and, at their conclusion in 1672, he brought him along with him into France. In 1675 Roemer communicated to the Academy of Sciences of Paris a memoir, in which he announced his discovery of the gradual propagation of light. This important communication obtained for him a seat in the Academy. He had also apartments assigned to him at the Royal Observatory, where he continued, during his residence in France, to make occasional observations of the celestial bodies. In 1681 he returned to Denmark, having been appointed Royal Professor of Mathematics in the University of Copenhagen. The great reputation which he had already acquired in France, pointed him out as a person eminently qualified to undertake the duties attached to the observatory of that city. The peculiar construction of this edifice has already been briefly alluded to. Roemer did not fail soon to perceive that it could not be advantageously employed for the purposes of astronomical observation. In consequence of the instruments being placed at the very top of the building, they were constantly liable to be disturbed in their positions by the violence of tempestuous winds. It was also found to be injurious to the health of the observer to prosecute his labours in so exposed a situation. Roemer, therefore, resolved to obtain the erection of a more suitable observatory in some open district of the country; but having been unable to realise his views so soon as he desired, he was induced in the meantime to convert his private dwelling-house into a temporary observatory.

The most remarkable novelty of the *Observatorium Domesticum* of Roemer was the *Transit Instrument* of which that distinguished astronomer was the inventor[*]. It was placed so as to make observations with it at a window. In consequence of the interposition of the neighbouring houses, it was impossible to command the view of a very large arc of the meridian. The range of the instrument extended from 20° south declination to 40° north declination. The axis, which was of iron, was five feet long, and an inch and a half thick. Its extremities rested upon iron supports inserted in the sides of the window, and the direction of its position was east and west, so as to allow a telescope attached to it to revolve freely in the plane of the meridian. The telescope was placed near one of the extremities of the axis. In order to obviate the effects of flexure, the tube of the telescope was composed of two cones attached to each other at their bases. At the focus there were inserted three horrizontal and ten vertical threads, but of the latter only three were used in practice. The illumination of the field of view was effected by means of a lantern placed above the centre of the telescope which threw its light upon a perforated concave speculum inserted a little behind the object-glass, the speculum again reflecting the light along the interior of the tube. In order to admit the passage of the light to the speculum, the upper portion of the tube of the telescope near the object-glass was altogether removed. Towards the other end of the axis was a projecting arm, carrying at its extremity a microscope by means of which the declinations of the celestial bodies were read upon a graduated arc fixed opposite to it. The arc, which extended to 75°, was divided to every ten minutes. In the common focus of the object-glass and eye-glass of the microscope there were inserted eleven parallel threads, by means of which the interval between two consecutive points of the graduated arc was divided into ten equal parts. The interval between two threads was consequently equal to a minute, and this again, according to Horrebow, the assistant of Roemer, might be subdivided by estimation so as not to commit an error of more than $2\frac{1}{2}''$ or $3''$ in determining the declination of a celestial body *. The pressure of the telescope and projecting arm upon the axis of the instrument, was counterpoised by a weight tending to pull it upwards at the centre. This contrivance was devised by Roemer some time after the instrument had been in use, in consequence of certain irregularities in the observations, which he could only account for by the flexure of the axis. The error of collimation of the telescope was ascertained by reversing the instrument. For this purpose a mark was carefully observed in the open fields before the instrument was placed in its position. The adjustment of the instrument so as to make the telescope revolve exactly in the plane of the meridian was effected by means of observations of corresponding altitudes of the stars †. Horrebow states that the horizontality of the axis of rotation was carefully ascertained, but he does not mention by what means this object was effected.

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[*] Horrebow's

Roemer continued to make observations with this instrument until a suitable observatory was erected for him in the country, when he obtained another of a similar kind but of a more perfect construction. Horrebow, his successor, states that in 1715, he prepared a suitable place for the original instrument in the ancient observatory, and having transported it thither, made many thousand observations with it, which were registered in fourteen folio volumes, when on the 21st of October, 1728, these, as well as all Roemer's observations, and also the others which he himself had made, were destroyed by the fatal conflagration which consumed so large a portion of the city on that occasion ‡.

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* Roemer's instruments were in general divided in the first instance to every ten minutes, and afterwards subdivided to every minute by means of the microscope, the seconds being estimated as mentioned in the text. From the circumstance of *eleven* threads being required to divide the space between two consecutive points on the instrument into *ten* equal parts, Delambre has hastily concluded that the threads performed the function of a vernier, but this is obviously a mistake.

† Delambre, who can see in Roemer only a person who carried out the ideas of Picard, endeavours, in every possible manner, to diminish the credit due to the Danish astronomer for the invention of this invaluable instrument; and, in order to attain his end, he has not scrupled to throw out an insinuation no less at variance with truth than it is unworthy of his high reputation as an historian of science. Thus, while trying to discover the reasons which induced Roemer to construct his instrument, he expresses himself in the following terms:—" . . . . . Peutêtre Roemer n'a-t-il été induit à la faire construire que pour la raison unique que le mur de sa chambre était oblique au méridien qu'il ne pouvait y placer un quart de cercle et pas même la lunette murale de Picard." (*Hist. Ast. Mod.*, tome ii., p. 639.) Now so far from it being true that Roemer fell upon the construction of the transit telescope by mere accident, as Delambre has suggested in the foregoing passage, it appears from a letter which the Danish astronomer wrote to Leibnitz in 1700, that as early as the year 1675 he had discovered the peculiar advantages of such an instrument, but was hitherto unable to obtain a suitable building wherein to place it. Thus he commences his description of the instrument in the following terms:— ". . . . . De instrumento, cui uni aptum ædificium, jam per 25 annos exoptavi, sed nunquam obtinere licuit." (*Miscellanea Beroliniensia*, tome iii., p. 277.)

‡ Basis Astronomiæ, p. 55.

Roemer, in his views of practical astronomy, seems to have been far in advance of the age in which he lived. He makes some admirable remarks on this subject, in a letter dated December 15, 1700, addressed by him to Leibnitz, who had solicited his opinion upon the most suitable plan of an observatory. "I differ very widely," says he, "from those who have hitherto decked out observatories more for show than for use, accommodating the instruments to the buildings rather than the buildings to the instruments."[*] The illustrious Dane, while writing these lines, had doubtless a vivid recollection of the magnificent "*Observatoire de parade*[†]" on the banks of the Seine, and of the expedients which Picard and himself were compelled to adopt, while endeavouring to fix a quadrant in the plane of the meridian, in the year 1678[‡]. The superiority of circular instruments, when compared with the quadrant and sextant, did not escape his sagacity: "I do not concur with others," says he, "in their opinion respecting the construction of instruments, since I consider that the use of the quadrant and sextant should be altogether abandoned, for I would place more confidence in a circle of four feet, than I would in a quadrant of ten feet."[§] He then proceeds to describe the plan of an observatory, which he had long desired for himself, and the character of the instruments which he designed to employ in it. In both these respects his wishes were fully realised a few years afterwards, as appears from the following description of his Rural Observatory, given by Horrebow:—

"The 'Observatorium Tusculaneum' of Roemer was built in an open situation upon a gentle eminence, a little to the west of the ancient observatory. It was 17 Danish feet in length, the walls were 5½ feet high outside and 6½ feet inside. The observations in the meridian were made with an instrument termed the *Rota Meridiana*, or Meridian circle. It consisted of a circle of 5½ feet in diameter, revolving in the plane of the meridian at one of the extremities of a horizontal axis of equal length. A telescope 5 feet long was attached to the eastern face of the circle. The intrument rested upon two solid supports of fir-wood, fitted with bars of metal into which the extremities of the axis were inserted. In order to prevent flexure, the axis was composed of two hollow cones of iron attached at their bases. The focus of the telescope was traversed by seven vertical and three horizontal threads. The divisions of the instrument were read by means of two microscopes fixed opposite the face of the circle at a distance of 10° from each other. The mode of reading off was similar to that practised in the case of the instrument already referred to. In order that the instrument might command the whole of the meridian from the northern to the southern horizon, a continuous fissure about 4 inches broad, was formed in the walls and roof of the building, the parts being united by iron bars only in those places where no stars were apparent.

Roemer did not fail to verify his instrument before commencing his observations with it. The deviation of the optical axis of the telescope from a line perpendicular to the axis of rotation, or in other words, the error of collimation, was ascertained by reversing the instrument. As this mode of verification was troublesome, he subsequently effected the same object by observing two distant marks in the horizon, diametrically opposite to each other. The horizontality of the axis of rotation was ascertained by means of the plumb-line. The adjustment in the plane of the meridian was effected by the aid of observations of Capella and α Lyræ, during their passages above and beneath the pole.

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[*] Miscellanea Beroliniensia, tome iii., p. 277.

[†] M. Arago, *Annuaire*, 1845.

[‡] Horrebow, in his *Basis Astronomiæ*, cites a statement of Roemer's to the effect that he, in conjunction with Picard, fixed a mural quadrant in the plane of the meridian, at the Royal Observatory of Paris, in the year 1678. This seems to be contradicted by La Hire, who asserts that the mural quadrant was adjusted permanently in its position only in the year 1683. It is probable that the expedient employed by Picard and Roemer was merely of a temporary nature.

The other instrument employed by Roemer in his Observatory was a telescope revolving in a plane perpendicular to the meridian. It was furnished with a system of vertical threads at its focus, similarly to the telescope of the meridian circle, being designed to be used in observing the passage of the celestial bodies across the prime vertical *.

In order to exhibit an illustration of his mode of observing, Roemer drew up a list of the declinations and right ascensions of certain stars, determined by him with the meridian circle on three consecutive nights in the month of October, 1706. This small collection received from him, in

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* It is to be regretted that Delambre, who was so eminently qualified to appreciate the value of the improvements in practical astronomy effected by Roemer, has allowed his mind to be so overruled by preconceived notions as to have been betrayed into a complete misrepresentation of the labours of that astronomer on the present occasion. Thus, in his description of the *Observatorium Tusculaneum*, the following extraordinary passage occurs:—" On y voit deux portes, quatre fenêtres, un lit, une cheminée, quatre horloges, et deux lunettes, l'une meridienne, l'autre, plus petite, qui tournait dans le premier vertical. Mais ce qu'il y a de singulier c'est que de quatre fenêtres, deux sont dans la direction de l'axe de rotation, et qu' une seule tout au plus parait être dans la direction de la lunette principale (Planche VIII.), et que la Planche III. destinée principalement à faire connaître l'instrument, et qui nous montre Roemer occupé à observer, parait prouver l'impossibilité absolue de diriger la lunette au nord; et quand tout serait ouvert, et la lunette tournée au nord, on ne concevrait pas mieux comment l'observateur pourrait passer du côté de l'oculaire." (*Hist. Ast. Mod.* tom. ii., p. 654). It will be evident to any person who bestows even a passing glance over the pages of Horrebow's *Basis Astronomiæ*, that Delambre, in the foregoing passage, confounds the *Observatorium Domesticum* fitted up by Roemer in a corner of his house in the year 1690, with the more complete edifice erected by him in 1704, which is denominated by Horrebow the *Observatorium Tusculaneum*. In fact, Plate III. refers to the former of these observatories, and to the original transit instrument (termed by Horrebow the *Machina Domestica*), while, on the other hand, Plate VIII. is intended to represent the Tusculan Observatory with the *Rota Meridiana* and the prime vertical telescope. It has been mentioned in the text that the observations with the *Rota Meridiana*, instead of being made at a window *barely opposite to the telescope*, as Delambre seems oddly enough to imagine, were in reality made through a fissure in the building, situate *exactly* in the plane of the meridian; and that so far from its being impossible to direct the telescope towards the north, the extent of the fissure allowed its being directed to any point in the meridian included between the northern and southern horizons. The following words of Horrebow are so explicit upon this point that it seems incredible that they should have escaped the notice of Delambre:—" Ut autem tubo cum machina primaria M (Rota Meridiana) circumvoluto totus meridianus ab horizonte australi per zenith usque ad horizontem borealem pateret, parietes ædificii cum tecto acuminato rimam continuam habebant quatuor pollices latam, . ." (*Basis Astronomiæ*, p. 141). The following remark of Delambre's in reference to the same subject is, perhaps, still more calculated to excite astonishment than anything that has yet been cited:—" Horrebow regrette *qu'on n'eut pas la faculté d'observer les étoiles circumpolaires;* ses planches paraissent démontrer cette impossibilité et cependant la polaire; la tête du Dragon, la Lyre, la tête et la queue du Cygne, deux étoiles de la queue de la Grande Ourse, ont été observées au dessous comme au dessus du pôle." (*Hist. Ast. Mod.*, tom. ii., p. 654). The words in Italics, which are so inserted in the original, with the evident intention of impressing their significance more strongly upon the reader, afford a remarkable illustration of the facility with which a person may be

consequence, the appellation of the *Triduum*. Unfortunately, it contains the only results of his labours that have been preserved, all his other observations having been destroyed during the fire of 1728. In accuracy they are surpassed only in a slight degree by the observations of the present day. The passages across the meridian are generally observed at the three vertical threads, or *wires* of the focus, as they are now technically termed. The time is in several instances assigned to fractions of a second. It is no mean tribute to the memory of Roemer, that the observations of the *Triduum* constitute the earliest collection of facts that have been deemed worthy of being employed as data in the solution of the great problem of modern astronomy—the motion of the Solar System in space.

The Tusculan Observatory was built in the year 1704, and in the month of December of the same year, Roemer commenced his labours in it, which he continued to prosecute till his death in 1710*. Horrebow states that the observations made by Roemer during this period, filled three large folio volumes, and equalled in number those of Tycho Brahé. The observations were continued after the death of Roemer, till the autumn of 1711, when the Observatory, by some unexplained casualty, was destroyed†. The instruments, which sustained great injury on this occasion, were transported to the ancient Astronomical Tower, where, with all the observations, except the *Triduum* of Roemer, a copy of which was safely deposited in the hall of the Academy, they were finally consumed by the dreadful fire of 1728.

Roemer was the first person who constructed an altitude and azimuth circle. Tycho Brahé and Hevelius had made use of quadrants moveable about a vertical axis, but Roemer did not fail to perceive that such instruments were essentially defective. The diameter of the vertical circle of Roemer's instrument was three feet five inches; the diameter of the azimuth circle was three feet eight inches. The instrument was chiefly used

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betrayed into error upon any subject, when, instead of carefully examining facts, he allows his mind to be so wholly engrossed by his own favourite ideas as to look for nothing else except what may appear to corroborate them. We venture to assert, that the words above cited are not to be found anywhere in the book which Delambre professes to be analyzing. Indeed, it is obvious that they are utterly inconsistent with Horrebow's description of the Tusculan Observatory, the edifice to which they relate. Delambre himself has not failed, in the above passage, to remark that the regret expressed by Horrebow is at variance with the fact of several circumpolar stars having been observed by Roemer; but it seems wholly unaccountable that he was not thereby induced to look more narrowly into the subject, so as to discover the origin of this apparent contradiction. It is not difficult to trace the passage in the *Basis Astronomiæ* which Delambre has so grievously misinterpreted (see p. 151 of the work just cited), but it is manifest that only the most unwary explorer of that work, seduced by some mental hallucination, could have been ensnared by such a passage.

* Delambre remarks, that as the observations which form the basis of Roemer's fragment on the annual parallax of the terrestrial orbit, inserted by Horrebow in his *Basis Astronomiæ*, were made by Roemer in the year 1692, they were anterior to the erection of the Tusculan Observatory, and he therefore concludes that we are totally ignorant of the kind of instrument employed by Roemer in these observations. (*Ainsi nous ne savons pas quel instrument il a employé dans ces observations.* Hist. Ast. Mod., tom. ii., p. 640.) This is another striking instance of the mistakes into which Delambre has fallen by confounding the *Observatorium Domesticum* of Roemer with the *Observatorium Tusculaneum*. The observations referred to were, in fact, made at the former of these edifices, (which was fitted up as early as 1690,) with the original transit instrument, or *Machina Domestica*, already described in the text.

† Horrebow simply says—"*Postea desolatum est observatorium.*" From this expression, and also that which follows, "*atque instrumenta jam magnis ictibus fracta,*" it would seem that the building was overthrown by the violence of some dreadful storm.

in observing corresponding altitudes of the sun and stars, for the purpose of regulating the clock, and adjusting the positions of the meridional instruments.

Roemer also constructed an equatorial instrument, by means of which he was enabled to follow a celestial body throughout the whole of its course above the horizon, and also to measure the differences of the right ascensions and declinations of two stars situate very near to each other. Horrebow states that the object for which Roemer expressly constructed this instrument, was to determine the parallaxes of the planets, by measuring their distances from the neighbouring stars at different altitudes above the horizon. We have seen that in 1665 Hooke proposed to effect the same object, by making observations of a similar kind in the meridian under different latitudes.

There are few individuals, of ancient or modern times, who effected improvements in practical astronomy equal in importance to those due to Roemer. It is a remarkable fact that the Meridian Circle, the Altitude and Azimuth Circle*, and the Transit Telescope, all of which were invented by this astronomer, form the capital instruments of the Central Observatory of Russia, erected recently at Pulkowa, the most magnificent institution that has been established in ancient or modern times for the purposes of astronomical observation.

Horrebow, who in 1710 succeeded Roemer as Astronomer Royal of Denmark, has given a detailed account of the catastrophe which resulted in the destruction of the astronomical observations of Roemer, as well as all those made by himself. He had been engaged at his own house on the evening of October 20, 1728, in writing a commentary upon a fragment of Roemer's on the Annual Parallax of the Terrestrial Orbit, which he had a short time previously discovered among the manuscripts of that astronomer, when his attention was aroused by a confused noise of drums and bells, announcing that a serious conflagration had broken out in the city †. As the place where the fire was raging, was at a considerable distance from his own house, he was for some time in hopes that it would be extinguished without occasioning him any inconvenience. The wind, however, having unfortunately shifted about eight o'clock in the evening, the fire now began to approach his own dwelling-house. Presently fragments of burning material fell into his court, while other portions, alighting on the roof of his house, threatened it with instant danger. There was now no time to be lost. His maid-servants were hastily conducted out of his house, along with eight children, four of whom were in a state of nudity, while the nurse carried in her arms an infant which had been only recently born. His wife and his eldest son, a youth of sixteen years of age, remained with him, and, with other friends, assisted in transporting his effects to the Hall of the Academy. This new place of refuge being ere long threatened with conflagration also, Horrebow was compelled to remove his effects to the Astronomical Tower. His wife, worn out with fatigue and anxiety, went in quest of her children, while he, with his eldest son, stationed himself in the Tower, whence he could perceive his house falling a prey to the fury of the flames. About four o'clock in the morning of the 21st of October, the wind having again shifted, the fire began to threaten the Astronomical Tower. By this time the avenue to the superior part of the Tower, where Horrebow had stationed himself, was almost completely blocked up with a multitude of effects which had been deposited for safety by various persons, so as nearly to prevent his descent. The near approach of the fire now suggested the certain conviction that the Tower would speedily become a prey to the flames. The occasion for taking some decisive step was therefore urgent. Some bedding which he had thrown out at a window, was immediately taken away by thieves. He had now to consider only his personal safety. Before leaving his own house he had searched out some manuscripts and a few engravings, which he locked up in a desk, and brought along with him to the Tower. Being anxious to carry away as much of what was really valuable as he possibly could, he was desirous of leaving the desk behind him, but amid so much confusion he lost the key, so that he was unable to obtain access to the manuscripts which he had locked up in it, and he was in consequence reduced to the necessity of carrying it bodily along with him. This unfortunate circumstance prevented him from carrying away other things of greater value. The descent to the bottom of the Tower had now become extremely hazardous. Abandoning, therefore, everything except the desk, he hastened down with it at the risk of his life, and escaped at one of the doors in a state of mind bordering upon complete distraction. Shortly afterwards the Tower was enveloped in flames, and all that was contained in it irrecoverably perished!

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* The use of the Vertical Circle of Pulkowa is indeed different from that of Roemer's instrument, being employed mainly in observing the celestial bodies a few minutes before and after their culmination.

† The mode in which poor Horrebow endeavours to bring the dreadful catastrophe before the mind of the reader is quite dramatic. He supposes himself to be engaged in writing his commentary, and to be interrupted in the midst of his labours (as was really the case) in the following manner :—" Namque ex observationibus ab anno 1692 usque ad 1702 ipsum beatum authorem parallaxia orbis annui deduxisse ex tota dissertatione, imprimis, § 160, clarum est. *Sed quid audio! Tumultus hominum in plateis cursitantium et clamantium! Sonitus fistularum, tympanorum, campanarum, &c. Eheu! incendium ultra modum vires et incrementa sumens.* Die 20 Octob. 1728, hor. 7, vespert. (*Basis Astronomiæ*, p. 70.)

Horrebow states that he was anxious to record these particulars, lest it should be thought by posterity that the astronomical observations made by Roemer and himself had perished through any fault on his part. Doubtless no one who reads his sad tale will be disposed to harbour such an accusation against him, or to cherish towards him any other feeling than that of deep commiseration for his irreparable misfortune. Notwithstanding the many valuable articles of his own which perished on that occasion, he asserts that there was nothing which he regretted at the time, or which he still continued to regret, except the loss of these observations. He congratulates himself, however, on having rescued from the flames the fragment of Roemer's on the Annual Parallax of the Terrestrial Orbit.

We now resume the history of the Royal Observatory of Greenwich. Flamsteed, who was appointed Astronomer Royal, was born at Denby, near Derby, on the 19th of August, 1646. His early predilection for astronomy is evinced by an observation of an eclipse of the sun made by him in the year 1662, the record of which appears among his MSS. preserved at Greenwich. He continued to make observations of celestial phenomena at his native place, till the year 1674, when he was induced by a circumstance, to which allusion has already been made, to transfer his residence to London. When he entered upon his duties at Greenwich, he found that the Government had made no provision to enable him to prosecute his observations, by furnishing the establishment with a collection of suitable instruments. The only appliances which he possessed for this purpose, were an iron sextant of seven feet radius, and two clocks given him by his kind patron, Sir Jonas Moore, and a quadrant and two telescopes which he brought with him from Derby. The angles were measured with the sextant by the aid of a contrivance which Hooke had suggested in his "Animadversions on the *Machina Celestis* of Hevelius." A screw working upon the edge of the instrument (which was racked for this purpose) imparted a slow motion to the moveable index, upon the end of which it was fixed, the angles being measured by counting the revolutions and parts of a revolution of the screw. The clocks were made by Tompion, the most celebrated artist of the day. The pendulums were thirteen feet long, and made each a single vibration in two seconds of time. The clocks required to be wound up only once in twelve months. The quadrant which Flamsteed brought with him from Derby had a radius of three feet, and was employed by him in regulating his clocks by means of the observed altitudes of the sun or stars *.

Flamsteed commenced his labours at the Royal Observatory of Greenwich on the 29th of October, 1676. The plan of observation at first pursued by him was the same as that practised by Tycho Brahé and the earlier astronomers. The intermutual distances of the stars were determined by observation, and their positions relative to two great circles in the celestial sphere were deduced from these results by trigonometrical calculation. Flamsteed had not long continued his observations, when he found that Hooke's contrivance for measuring angles with the sextant could not be implicitly confided in. He, therefore, drew diagonals upon the limb towards the end of the year 1677, and subsequently expressed each angular distance determined with the instrument not only, as hitherto, in terms of the revolutions and parts of a revolution of the screw, but also in degrees, minutes, and seconds. This practice was found by him to be very useful in checking any inaccurate reading of the observations.

In order that he might be enabled to deduce useful results from his observations, Flamsteed was desirous of ascertaining the latitude of his observatory, the place of the equinox among the stars, and other points of essential importance; but these objects could not be effected by means of the sextant, which served only to determine the apparent positions of the celestial bodies relative to each other. He perceived that for this purpose a mural quadrant was necessary, and therefore as soon as he entered the observatory he endeavoured to obtain one. Shortly afterwards, Hooke was charged by Sir Jonas Moore, to construct a mural quadrant of ten feet for the observatory; but the instrument, when completed, was found to be totally unmanageable. Flamsteed, therefore, now began to think how he might take meridional altitudes with his sextant, and he so far succeeded in his object as to be enabled, in 1677, to determine by its aid the latitude of the observatory, which he found to be 51° 28′ 10″.

Flamsteed now proceeded to correct the elements of the solar theory by means of his own observations. He devised on this occasion an ingenious method for determining the absolute right ascension of the sun.

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* Flamsteed states that he continued to use this quadrant till June, 1678, when he obtained a neater one from the Royal Society, which, however, he was compelled to restore to its owners in the month of October, of the following year. This circumstance induced him to construct a new quadrant of 50 inches radius, fitted with peculiar contrivances, by means of which he was enabled to ascertain the time from observation within three seconds.—(Baily's *Life of Flamsteed*, p. 45.)

He remarked that by observing the meridional altitude of that body near each equinox, two times might be found when he had the same declination; and by the tables, the intermediate arc described by the sun might be found, half of whose excess above a semicircle (if it were more than one), or half of whose defect (if it were less), would be his true distance from either equinox at those times*. This idea forms the germ of a fundamental method of great importance which astronomy owes to Flamsteed, and which we shall presently have occasion to allude to more particularly.

Finding by experience that the sextant was very ill adapted for determining the meridional altitudes of the celestial bodies, and that the Government was not disposed to supply him with a mural arc, Flamsteed resolved to construct an instrument of the latter description at his own expense. This design was carried into effect by him in the year 1681; but the instrument was not fixed in the meridian till 1683. It was of the same radius as the sextant. The arc was made equal to 140° in order to allow observations being made of all the stars which passed the meridian between the north pole and the southern horizon. Being very weakly constructed, the instrument was found to have warped when placed in the meridian. Flamsteed, however, was of opinion that with due care it might be usefully employed notwithstanding this defect. Accordingly, during the period included between the years 1683 and 1686 he continued to determine the altitudes of the celestial bodies with it, and in the following year he constructed a small catalogue of stars to serve as points of reference in his future observations, founded upon the results obtained by the aid of this slight arc and the sextant†.

Having acquired possession of a small property by the death of his father in 1688, and seeing no prospect of being furnished with any instruments by the Government, Flamsteed now resolved to construct a strong mural arc at his own expense. He had already commenced operations for this purpose when, his assistant Stafford dying in the month of May of the year above-mentioned, he engaged in his service the celebrated Abraham Sharp‡. According to his plan of the instrument, the edge was to be racked so as to measure the angles by Hooke's method, while at the same time, as in the case of the sextant, diagonals were to be drawn upon the limb. Sharp, who was at once an ingenious mathematician, an expert calculator, and a skilful mechanic, racked the edge of the limb, prepared the index, and fixed the arc on the wall. Afterwards he divided and engraved the limb "so exquisitely," says Flamsteed, "as to excite the admiration of every beholder." The instrument was not completely ready for use till the month of October, 1689. Fourteen months were occupied in its erection, and the total cost to Flamsteed was no less than one hundred and twenty pounds, not one farthing of which was ever refunded to him by the Government.

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* Baily's *Life of Flamsteed*, p. 50.

† This was a catalogue of 130 stars reduced to the year 1686, which he continued to use in computing from his observations the places of the moon and planets, even for some time after he had commenced his observations with the mural arc in 1689.

‡ Abraham Sharp was born at Little Horton, near Bradford, in Yorkshire, about the year 1651. After receiving the elements of a sound education at his native place, he was put as an apprentice to a merchant in Manchester; but having no natural inclination towards commerce, he quitted his situation with his master's consent, and subsequently opened a school in Liverpool, at the same time cultivating an acquaintance with mathematics and astronomy. Shortly afterwards he removed to London, where he became known to Flamsteed, who was then about to assume the superintendence of the Royal Observatory of Greenwich. In 1688 he was appointed by Flamsteed to be his assistant, on which occasion he constructed the famous mural arc with which that astronomer made the most valuable part of his observations. Finding that the fatigue of night observation did not suit his weakly constitution, he retired to his native place, where he spent the remainder of his days, living upon the proceeds of a small patrimonial estate. Here he erected an observatory, which he fitted up with instruments all of his own construction. He also made his own telescopes, ground his own lenses, and constructed all sorts of tools used by artificers, besides contriving and executing a great variety of ingenious pieces of mechanism. Moreover he is the author of a mathematical work containing a collection of most elaborate tables relating chiefly to logarithms. Although he lived in great retirement, he carried on an extensive epistolary correspondence with Newton, Halley, and all the most celebrated mathematicians of his time. In private life he was most exem-

The mural arc erected by Flamsteed on this occasion was made equal to 140°, as in the case of the former instrument of the same description. Its radius was 79 inches in length. The limb was, in the first instance, divided to every 5′, and was then subdivided into smaller parts by means of diagonals. For this purpose the fiducial edge of the moveable index, included between the outer and inner arcs of the instrument, was divided into five parts so as to indicate minutes *, and each minute was subdivided into six equal parts, each equal to 10″, which again might be bisected by estimation so as to obtain a final subdivision to every 5″.

Before commencing his observations with the mural arc, Flamsteed took the precaution of ascertaining whether the zero point of division had undergone any alteration with respect to the zenith, during the interval that had elapsed since it was marked upon the limb. This was effected by observing certain stars near the zenith, first with the instrument itself attached to the *eastern* face of the meridian wall, and then observing the same stars with the telescopic index of the instrument transported to the *western* face of the wall. Half the difference of the zenith distances of each star determined in this manner, gave the corresponding error of collimation of the instrument, and the mean of all the results gave the mean error of collimation. From observations of this kind, Flamsteed found that the zenith distances of those stars which passed the meridian towards the north, were all too great as determined by the instrument. The error on the 11th of September, 1689, amounted to 45″. In the course of his subsequent observations, he found that the error increased slowly in the same direction every year, a circumstance which he attributed to the gradual sinking of the southern extremity of the wall to which the instrument was attached. He did not fail, therefore, to determine from time to time, by means of suitable observations, the amount of deviation arising from this cause. He remarked that the stars in the feet of the constellation *Gemini* afforded peculiar advantages for this purpose; for being situate near the solstitial colure, they were liable to change very slowly from the retrograde motion of the equinoctial points, and, being at

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plary. He died on the 18th of July, 1742, in the ninety-first year of his age. The delicacy of Sharp's hand for manipulative operations was exquisite. He is generally admitted to be the first person who cut divisions on astronomical instruments with any pretension to accuracy. He is also the first of those remarkable men by the aid of whose mechanical talents the Astronomers of the Royal Observatory of Greenwich were enabled to execute the series of observations which have won for that establishment its unrivalled reputation.

* The intervals in this case, being the results of a strict mathematical process, were not exactly equal, but the difference between two consecutive divisions could scarcely be considered sensible.

the same time not far removed from the ecliptic, they were subject to only a very small error in the same direction from the parallax of the earth's orbit *.

Flamsteed's next object was to ascertain the errors produced by the instrument on the right ascensions of the stars. He remarked that such errors might arise either from the plane of the instrument not being exactly in the meridian, or from the object-glass of the telescope not being properly adjusted in its cell. He proposed to determine by one process the combined effect due to these two causes. This object he accomplished for all altitudes included between the equator and the northern tropic, by comparing the times of the sun's passage over the meridian, as deduced from observations of equal altitudes on each side of that circle, with the corresponding times obtained directly by the aid of the instrument. In this manner he found that at 40° of zenith distance, the transit of a celestial body, when observed with the instrument, took place 33 seconds earlier than the true time. The errors for the parts of the arc, comprised between the tropic and the north pole, and between the equator and the southern horizon, were found by comparing the differences of the right ascensions of several bright stars, derived from observations of their intermutual distances with the sextant, with the corresponding results deduced from observations with the instrument itself. Having thus ascertained the errors of the plane of the instrument at various zenith distances throughout the whole extent of the arc, Flamsteed arranged the results in a table, which he continued to use from the year 1690, in determining by observation the right ascensions of the stars †.

Flamsteed commenced his course of regular observations with the mural arc on the 11th of September, 1689. But before he could deduce any useful results from them, it was necessary for him to determine the latitude of his observatory with greater accuracy than he had hitherto done. From observations of the least and greatest altitudes of circumpolar stars, he finally concluded its true value to be 51° 28′ 34″. He next proceeded to derive from his observations, the obliquity of the ecliptic, and the other fundamental points of astronomical science. His labours on this occasion are distinguished by considerable originality and address. The most important part of them is that which relates to the determination of the absolute right ascension of a celestial object. It has been already mentioned that he succeeded in determining the place of the sun with respect to the solstice, by observing that body at equal altitudes near the equinoxes, and halving the intermediate arc. The magnitude of this arc, however, could only be ascertained by computation, and consequently the accuracy of the result depended in a great degree upon the solar tables. Flamsteed eluded this source of error by a process which enabled him to determine not merely the place of the sun with respect to the equinox, but also the places of the stars relative to the same point. For this purpose the interval between the times of the sun's passage over the meridian and that of a bright star near the equator, was noted by the aid of the pendulum clock, on the occasions when the sun attained the same meridional altitude near the two equinoxes. Now since the sun has been continually

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* Historia Cælestis, vol. iii., p. 112.

† Hist. Cæl., vol. iii., p. 133. This table does not appear in the *Historia Cælestis*, but Mr. Baily found it among the manuscripts of Flamsteed, and has inserted it in his interesting collection of memorials respecting the astronomer.—(See *Life of Flamsteed*, p. 374.)

travelling in his orbit, during the period included between the two observations, while on the other hand the position of the star is invariable, it is manifest that the excess of the second of these intervals over the first, when converted into space by supposing every 15° equivalent to an hour, will indicate the arc of right ascension described by the sun during the intermediate period. The half of this arc then gives the distance of the sun from the solstice at either observation, and since the difference of the right ascensions of the sun and star is known by the clock, the distance of the star from the solstice is also at once ascertained.

It is obvious that the practice of observing a star in connexion with the sun according to the above method, is attended with a twofold advantage. In the first place it serves to determine the right ascension of the sun independently of the solar tables, by affording an accurate measurement of the arc described by that body, during the interval of time that elapses between the two observations. Secondly, it leads at the same time to a knowledge of the absolute right ascension of the star. According to every method hitherto practised by astronomers, the ascertainment of the absolute right ascension of the sun was effected by noting the *time* of his passing through the equinox, and calculating his subsequent motion by the aid of the solar tables. The places of the stars with respect to the equinox were then determined by measuring the distance between the sun and a bright star, employing for this purpose the moon or Venus, as the intermediate object of observation. The method of Flamsteed had the advantage of assigning an easy and direct process by means of which the right ascensions of the sun and star might be simultaneously determined. Moreover, since the altitude of the sun is supposed, according to this method, to be the same at each of the two extreme observations, the final results are not affected by any uncertainty in the values of parallax or refraction. They are also, independent of a knowledge of the latitude of the place of observation, an element which entered essentially into all the ancient methods for determining the place of a celestial body with respect to the equinox.

The absolute right ascension of any one star being accurately determined, the right ascensions of all the others may be readily found by a comparison with the fundamental star. In the year 1690 Flamsteed determined the absolute right ascensions of no less than forty stars by comparing them directly with the sun in the mode above described, and employed the results in establishing a series of points of reference to aid him in his future observations. By his labours on this occasion his name has become imperishably associated with one of the fundamental methods of astronomical science.

Having established the elements of the solar theory and other points of primary importance, Flamsteed was now enabled to derive from his observations the absolute positions of the celestial bodies. He therefore proceeded to compute the observed positions of the moon and planets, with the view of making the results subservient to the rectification of the theory of their movements. He also commenced the formation of a catalogue of the fixed stars upon a more extensive scale than any which had been hitherto executed. It must be acknowledged that in the prosecution of these arduous labours, he received very little assistance from the Government. They had not hitherto furnished the observatory with a single instrument, nor did they allow him even the smallest sum for the occasional repair of such instruments as he was actually using. When he commenced his observations, they assigned to him "a surly labourer" to manage the sextant, but they seemed to imagine that the salary of a hundred pounds a year which he received was amply sufficient to ensure his own performance of all the other duties connected with the observatory*. To execute such a task, however, was manifestly impossible. Under these circumstances he was compelled to procure an assistant, by paying him a small salary out of his own pocket. To meet the expenses which the illiberal treatment of the Government thus entailed upon him, he was for some time reduced to the necessity of adding the profession of *teacher* to his usual avocation. Well might he remark that he earned the salary doled out to him *by labour harder than thrashing*†. The difficulties of his situation were further aggravated by the possession of a feeble frame of body, and the periodical attacks of a painful malady to which he was constitutionally subject.

The illustrious Newton was the first person who availed himself of the observations made at the Royal establishment of Greenwich. In the years 1694-5 he obtained from Flamsteed a considerable number of observed places of the moon, which proved very serviceable in guiding him to a knowledge of some of the more hidden inequalities in the motion of that body, and thereby affording a seasonable confirmation of the theory of universal gravitation ‡.

Notwithstanding the numerous disadvantages against which he had to struggle, Flamsteed continued with unremitting assiduity to labour in the discharge of his duties, and he confidently looked forward to the prospect of being one day enabled to present the results of his observations to the world in a satisfactory form, although he clearly saw that, if left wholly to his own resources, it would be impossible for him to obtain a speedy realisation of his wishes in this respect. Unlike many astronomers, he did not content himself with merely accumulating a mass of observations, leaving to others the task of reducing them to a form which might render them available for any useful purpose. Much of his time was occupied with the formation of his great catalogue of the stars, and with calculating from his observations, the places of the moon and planets; and it was his desire not to commence the publication of the observations until both these objects were fully accomplished, so as at the same time to be enabled to place within the reach of the public not merely the observations themselves, but also the results directly deducible from them §.

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* The paltry salary which he received was subject to a tax of £10, so that in reality it did not amount to more than £90 a year. Even this pittance, however, seemed to the Government to be more than an adequate remuneration for his labours, for at one time in the early part of his career they annexed to his duties the task of instructing in nautical astronomy two boys of Christ Church Hospital.

† Baily's *Life of Flamsteed*, p. 117.

‡ For the correspondence which took place between Newton and Flamsteed on this occasion, see Baily's *Life of Flamsteed*, pp. 133-160.

§ That Flamsteed seriously contemplated the publication of his observations at some future period, even although he should obtain no assistance from any quarter, is evident from the following passage of a letter dated October 26, 1700, addressed by him to Dr. Smith of Oxford:—"Briefly, sir, I am ready to put the observations into the press, as soon as they that are concerned shall afford me assistants to copy them and finish the calculations. But if none be afforded, both they and I must sit down contented, till I can finish them with such hands as I have; when I doubt not but to publish them, as they ought to be, handsomely and in good order, and to satisfy the world, whilst I have been barbarously traduced by base and silly people, that I have spent my time much better than I should have done if

While Flamsteed was thus strenuously endeavouring, by his own unaided efforts, to give a definite shape to his labours, several of his scientific contemporaries began to express a feeling of impatience that the publication of the observations made at the Royal Observatory of Greenwich should be so long delayed. He was induced by this circumstance to draw up an estimate of the extent of the work which he was preparing for the press, and of the plan according to which he was desirous that it should be published*. This estimate was read at a meeting of the Royal Society held on the 15th of November, 1704, and received the unanimous commendation of the members. Prince George of Denmark, the Queen's consort, who was shortly afterwards elected a member of the Society, having been informed of the labours of Flamsteed as announced in this exposition, generously undertook to defray the expense of their publication, and, with a view to this object, a committee, consisting of Newton, Wren, and two or three other individuals, was appointed to inspect the manuscripts. The committee recommended that all the observations should be published, and Flamsteed was instructed forthwith to prepare them for the press. Flamsteed complied with the demands of the committee, surrendering into their hands a copy of his observations, and also a catalogue of the fixed stars which he had deduced from them. As the catalogue was not complete, he merely deposited it with the committee, as a guarantee for his furnishing them on a future occasion with a more perfect copy, expressly stipulating that in the meantime no steps would be taken towards its publication. In the month of May, 1706, the printing of the observations was commenced, but it proceeded very slowly, and was at length interrupted entirely for some time by the death of Prince George of Denmark in 1708.

On the 12th of December, 1710, the Queen issued a warrant appointing the President of the Royal Society, and such other members of the Council of the said Society as he should think fit, to be Visitors of the Royal Observatory of Greenwich. They were authorized to demand of the Astronomer Royal to deliver up to them within six months after the close of each successive year, a true and fair copy of his annual observations. They had also full powers to direct him to make such observations as they should deem desirable, and to inspect the instruments from time to time, so that they might be constantly maintained in a proper condition.

The origin of the Board of Visitors is clearly traceable to the unfortunate misunderstanding that prevailed between Flamsteed, on the one hand, and his scientific countrymen generally, on the other. It has continued to exercise its functions to the present day, but at the accession of William IV. a new warrant was issued, according to which its constitution underwent a slight modification†. The salutary influence of such a board of inspection is indisputable, for while on the one hand it serves to prevent the application of the resources of the observatory to any unwarrantable purposes, on the other hand it has the effect of periodically relieving the conscientious astronomer from the responsibility attached to the discharge of his onerous duties, and thereby operates as an encouragement to future exertion. It is gratifying to reflect that during the last hundred years, at least, it is only in the latter respect, that the advantages resulting from the establishment of the Board of Visitors, have been apparent.

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* It appears from Flamsteed's journal, that he had been only a few years occupied with his duties at Greenwich when persons began to importune him about publishing his observations. "Some people," says he, "to make me uneasy, others out of a sincere desire to see the happy progress of my studies, not understanding amid what hard circumstances I lived, called hard upon me to print my observations."—(Baily's *Life of Flamsteed*, p. 54.)

† By virtue of the new warrant, the President of the Royal Society for the time being, and five other Fellows of that Society nominated by him, together with the President of the Royal Astronomical Society for the time being, and five other Fellows of that Society nominated by him, and the Savilian Professor of Astronomy at Oxford, and the Plumian Professor of Astronomy at Cambridge, are appointed to be Visitors of the Royal Observatory.

In the spring of 1711 the printing of Flamsteed's observations was resumed by order of the Queen, and in the following year they were published in one large folio volume. The preface, which was written by Halley, contained some very ungenerous reflections on Flamsteed. Moreover, the form in which the results of his labours were presented to the world on this occasion was far from being in accordance with his own views. With respect to the stars observed with the mural arc, the places only of those were inserted in the work, which passed the meridian about the same times, and nearly on the same parallels, with the moon and planets. The catalogue of stars was also no other than the avowedly imperfect one which Flamsteed originally deposited with the committee appointed to superintend the printing of the observations in 1704, upon the express condition that it should not be published, but merely that it be retained as a pledge for his subsequent delivery into their hands of a more complete copy. It would appear that the parties charged by the Queen with the publication of the observations, grew impatient at what they conceived to be Flamsteed's tardiness in fulfilling his promise, and adopted the resolution of printing the catalogue in their possession, without his concurrence, or even his knowledge. This was manifestly an act deserving of severe reprobation. It may be urged, indeed, as an excuse for such an extreme proceeding, that the observations were public property, and that a due regard for the objects contemplated in the establishment of the observatory, suggested the expediency of publishing them within a reasonable period of time; and, perhaps, according to the strict letter of the law, the act was justifiable upon that ground. But, on the other hand, a more generous allowance ought to have been made for the difficulties of Flamsteed's position, arising mainly from the circumstance of the Government having failed to afford him such support in the prosecution of his arduous labours as he had a right to expect in virtue of the office for which he was held responsible. At all events, it looks like a wanton display of harshness on the part of the committee in whose hands the manuscripts of Flamsteed were deposited, to have resorted to their publication without beforehand giving him due notice of their intention [*].

Justly indignant that the results of his long and toilsome labours should be presented to the world under circumstances so injurious to his personal character, and in a form so little calculated to advance his reputation as an astronomer, Flamsteed resolved to publish a complete edition of his observations at his own expense [†]. He accordingly proceeded to carry his design into effect, with all the energy which the consciousness of a just cause, and a laudable desire to set himself right in the eyes of posterity, were capable of inspiring. His constitution, however, already shattered by the incessant inroads of ill health, and now sinking under the frailties of old age, was inadequate to the complete execution of a task of such magnitude. It was contemplated by him to publish the results of his labours in three volumes; but he had barely succeeded in completing the second volume when he died, on the 31st of December, 1719. The preparation of the third volume for the press, was effected by the voluntary exertions of Crosthwait, his assistant at the time of his death, and the famous Abraham Sharp, who had also at one time served him in the same capacity, and had ever since continued his devoted friend[*]. The whole work was finally published in the year 1725, in three volumes folio, under the title of "Historia Cælestis Britannica." The first volume contains extracts from the observations of Gascoigne and Crabtree from 1638 to 1643, also Flamsteed's own observations at Derby down to 1675, and his subsequent observations at Greenwich with the sextant from 1675 to 1689. It comprehends, moreover, various subsidiary tables to be used in calculation, and also the places of the moon and planets deduced from the observations. The second volume contains the observations made with the mural arc from 1689 to 1719[†]. At the end of this volume, also, there is a collection of useful tables, and a list of the places of the moon and planets computed from the observations. The last-mentioned results, as well as the corresponding results in the first volume, exhibit not only the right ascensions and polar distances of the several bodies, but also their longitudes and latitudes. The third volume commences with the Prolegomena, in which Flamsteed takes a rapid survey of the progress of astronomy, concluding with a description of his own instruments and methods of observation. There are next inserted copies of all the catalogues of the fixed stars that had been executed by astronomers previous to the time of Flamsteed. These are succeeded by Flamsteed's own catalogue, which exhibits the right ascensions and polar distances, as well as the longitudes and latitudes of 2935 stars, reduced to the beginning of the year 1689, together with the annual variations in right ascension and polar distance, arising from the regression of the equinoctial points. At the end of the volume there are various subsidiary tables designed for abbreviating the labours of calculation.

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[*] It is right to state that this charge rests solely upon the authority of Flamsteed. In all probability the parties misunderstood each other. The whole affair, as disclosed by Mr. Baily's work, is of a very unpleasant nature.

[†] There were 400 copies printed of Halley's edition of 1712. In 1715 Flamsteed succeeded in procuring from the Government all the copies that remained in their possession, amounting to 300, and immediately committed them to the flames, with the exception of the sheets on which his observations with the sextant were printed.

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[*] Sharp resided at this time in Yorkshire. He also assisted Crosthwait in the preparation of the maps of Flamsteed's Atlas, which was published in 1730. The following passage of a letter from Crosthwait to Sharp, dated January 27, 1721-22, reflects so much credit upon all the parties alluded to, that it cannot fail to prove acceptable to the reader:—"I am much concerned to find, by yours of the 2nd instant, that you had entertained the least suspicion of being forgotten or slighted by me, though there had been nothing more for you to do. I can assure you, with the greatest sincerity, that I shall for ever (though I am sure I shall have no share in the profits) retain a grateful remembrance of the generous and kind assistance you have given towards completing Mr. Flamsteed's works, and shall be ready at all times hereafter, so long as life endures, when in my power, to return you gratitude; and the memory of the ingenuous and disinterested Mr. Sharp will always, by me, be had in the greatest esteem, next to that of my deceased and good friend Mr. Flamsteed." (*Baily's Life of Flamsteed*, p. 346.) Mrs. Flamsteed died in 1730, without leaving a single farthing either to Crosthwait or Sharp, notwithstanding their disinterested exertions in her behalf during a period of ten years!

[†] At the top of each page of observations in this volume the error of collimation of the instrument is inserted.

Flamsteed is universally admitted to have been one of the most eminent practical astronomers of the age in which he lived. His merits do not, indeed, appear at first sight so conspicuous as those of some of his illustrious contemporaries with whom he may be compared, although at the same time they are no less substantial. He does not generally exhibit the enlightened discernment which Picard displays in his researches, nor was he endowed with the fertility of invention by which Roemer was so eminently distinguished; but in carrying out views of practical utility, with a scrupulous attention to accuracy in the most minute details, in fortitude of resolution under adverse circumstances, and persevering adherence to continuity and regularity of observation throughout a long career, he has few rivals in any age or country. Without the possession of these invaluable qualities, the most splendid genius may fail to exercise any durable influence: by means of them Flamsteed was enabled to establish the fundamental points of practical astronomy upon a new basis, and to rear a superstructure which, for many years afterwards, served as a landmark of vast importance to astronomers. In so far as the accuracy of his observations is concerned, his labours must be considered with reference to the times in which he lived, and the difficulties which he had to encounter. When judged according to this equitable standard, they will not suffer from the scrutinies of the candid enquirer. It forms a noble attestation to the merits of Flamsteed in this respect, that his observations are the earliest from which the phenomenon of aberration clearly and unequivocally emerges, its maximum value being assigned by them with a degree of precision almost equal to that deducible from the most trustworthy observations of the present day *. As first Astronomer of the Royal Observatory of Greenwich, he set an example to his successors the beneficial influence of which cannot for a moment be doubted; nor, while that noble establishment continues to maintain its proud pre-eminence among the institutions devoted to practical astronomy, will the labours of its original Director, prosecuted with such unwearied perseverance throughout a long career, despite the depressing influence of constitutional ill health, and the unrelenting hostility of a powerful faction, cease to be held in respectful remembrance by his countrymen.

Flamsteed was succeeded at the Royal Observatory by the celebrated Edmund Halley. This great astronomer was born at London, on the 29th of October, 1656, and had already distinguished himself by a brilliant career of research on various subjects of physical science. It seems somewhat surprising, when we consider that he was now in the 64th year of his age, that he should have undertaken the discharge of duties of so onerous a nature as those attached to the situation of Astronomer Royal. Unlike his predecessor, however, he had the good fortune to possess a robust constitution, and, in reliance upon its vigour, he was desirous of prosecuting a series of observations of the moon, in regard to which he had a special object in view, as will be presently mentioned.

It has been already mentioned that the instruments used by Flamsteed

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* See page 340 of this work, where the value of aberration deduced by M. Peters from Flamsteed's observations is given. The same astronomer makes the latitude of Greenwich, according to Flamsteed's observations, to be 51° 28′ 41″.9 ± 0″.9.—(*Recherches sur la Parallaxe des Etoiles Fixes*, p. 11.) Mr. Airy, from recent observations, has determined the true value of the latitude to be 51° 28′ 38″.2.—(*Greenwich Observations* 1842, p. xliv.)

were all his own, the Government not having even defrayed the expense of occasionally repairing them. We need not wonder, therefore, that when Halley entered the observatory he found it completely stripped of everything available for the prosecution of celestial observations. The mural arc, the clocks, the telescopes, and, in short, every instrument in the observatory, had been carried away by Flamsteed's executors immediately after his death. In consequence of this circumstance, some time elapsed before Halley was enabled to enter upon a regular course of observation. In 1721 he procured a transit instrument, with which he commenced observations of the passages of the celestial bodies over the meridian. This instrument was five feet six inches long, and is stated by Lalande to have been constructed by Hooke. Halley's first observation with it is dated October 1, 1721. In 1726, a mural quadrant, for the observation of zenith distances as well as differences of right ascension, was at length erected. This instrument was constructed by the celebrated artist Graham*. It was made of iron, and had a radius of eight feet. The limb was composed of two arcs, upon each of which a different mode of division was executed. The inner arc was divided to every 5′, according to the ordinary sexagesimal scale, and was afterwards subdivided, by means of a vernier, to every 30″, which again might be subdivided, by estimation, to one-fourth of this quantity, or 7½″. The outer arc was divided, first, into ninety-six parts, and then each of these into sixteen smaller parts, each of which again was subdivided, by means of a vernier, to every 13″, and each of these parts might finally be subdivided, by estimation, to 5″ or 6″. In making observations with the instrument, both modes of reading off were employed, so that the one might serve as a check upon the other; but in cases of discordance, greater reliance was usually placed on the indications of the outer arc. Halley commenced his observations with this instrument on the 20th of October, 1725. Henceforward he continued to observe not only the transits of the celestial bodies over the meridian, but also their zenith distances. It does not appear, however, that he adopted such an extensive plan of observation as that pursued by his predecessor, Flamsteed. His main object was to procure materials for the improvement of the lunar and planetary tables; and, for this purpose, he deemed that it would be unnecessary to observe any other stars than those that were situate within the limits of the zodiac. His labours were more especially directed to the observation of the moon. He was stimulated to this object by the large reward which the Government had offered relative to the longitude, being convinced that the method of lunar distances was better adapted than any other to the solution of that important problem. This subject seems to have so far engrossed his attention as to have caused him in some degree to neglect the ordinary duties of his office. It has been already mentioned that the

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* George Graham was born at Gratwick, in Cumberland, in the year 1675. He was not only the most skilful artist of his time, but was also distinguished by a profound acquaintance with various subjects of natural philosophy. Besides the mural quadrant alluded to in the text, he constructed the famous zenith sector with which Bradley discovered the phenomena of aberration and nutation. It was also with a zenith sector executed by the same artist that the French academicians, who measured the length of an arc of the meridian in Lapland, determined the latitudes of their stations. Graham contributed various papers to the *Philosophical Transactions* of the Royal Society, of which he was a member. One of these related to the mercurial compensation pendulum, the

Board of Visitors, which was composed of members of the Royal Society, were authorized to demand of the Astronomer Royal, within six months after the lapse of each successive year, "a true and fair copy of the annual observations he had made." At a meeting of the Royal Society, held on the 2nd of March, 1727, Sir Isaac Newton, who was then President, took occasion to remark that this regulation had not been recently observed; and stated at the same time, that, as the continued neglect of it might be attended with detriment to the public interest, he thought it proper to remind the Astronomer Royal of the circumstance of such a regulation being in force. Halley, who was present, replied to Newton by asserting that he had already executed a great number of observations, especially of the moon; but that on account of the utility of such observations towards ascertaining the longitude, for which a large reward had been offered by the Government, he had hitherto kept them in his own custody, that he might have time to finish the theory he designed to build upon them, before others might take the advantage of reaping the benefit of his labours*.

Halley died on the 14th of January, 1742, in the 86th year of his age. It appears that for some time previous, he had ceased to attend to the duties of the observatory, a circumstance which is not to be wondered at when his extreme old age is taken into consideration. His last observation is dated December 31, 1739. The only part of the observations made by him at Greenwich that has ever been published, is a collection of observed places of the moon, which are to be found at the end of his Astronomical Tables, which were published in 1749†. These tables were printed off in 1719, with a view to their immediate publication; but their author being in the meantime appointed Astronomer Royal, they were retained by him for his own exclusive advantage, since there was now an increased probability of his bringing the lunar theory to such a state of perfection as would lead to a practical solution of the problem of the longitude. With a view to this object, he had no sooner entered upon his duties at Greenwich, than he commenced instituting a comparison between the places of the moon deduced from his observations, and the corresponding places calculated from the tables. These results were printed off at successive intervals during Halley's lifetime, and the whole collection was finally published along with his Astronomical Tables, as has been already mentioned. It exhibits a comparison between the observed and calculated places of the moon, extending from 1722 to 1739. The error of the tables in longitude is frequently less than 1′, but in some instances it rises to 5′ or 6′. In general, it may be said to range between 2′ and 4′. It does not appear that Halley employed these residual errors in any researches for the purpose of obtaining an improvement of his tables of 1719.

The original records of Halley's observations are deposited at Greenwich in four small quarto volumes. Upon the recommendation of Mr. Baily, a manuscript copy of them was taken by the order of the Lords of the Admiralty, and presented to the Astronomical Society, in the year

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* Mem. Ast. Soc., vol. viii., p. 188, cited by Mr. Baily, from the Minute Book of the Royal Society.

† In the preface to these tables it is stated that they were mainly constructed by Halley, upon the observations of Flamsteed.

1832. Mr. Baily has concluded, from a careful inspection of these observations, that they do not possess sufficient value to render it desirable that they should be printed*. Maskelyne had already intimated to Delambre, that they were hardly preferable to those of Flamsteed†. Halley, indeed, was endowed with a mind of vast compass as well as extraordinary sagacity and power; but he seems to have undervalued those habits of minute attention which are indispensable to the attainment of a high degree of excellence in the practice of astronomical observation‡.

The absence of any definite plan of operations in connexion with the Royal Observatory of Paris has been already alluded to. The evils arising from this cause did not fail soon to present themselves. The astronomers engaged in prosecuting observations at that establishment, were frequently compelled to discontinue their labours, at the instance of the Government, for the purpose of executing scientific operations in the provinces of France. This circumstance occasioned numerous interruptions in the records of the observations, which manifestly tended in a great degree to depreciate their value. Moreover there was an incompleteness generally apparent in the observations actually made, arising from the want of a presiding power to direct the labours of the establishment. These defects in the constitution of the observatory are described in the following graphic terms, by Cassini IV.:—"*Chacun venait observer dans les cabinets comme il l'entendait, quand et selon que cela lui plaisait, les astronomes novices pour s'exercer, les académiciens pour leur propre compte. Il n'y avait ni plan général suivi, ni chef pour diriger; de là, ni accord, ni ensemble, ni suite dans les travaux.*"§

But although the Royal Observatory of Paris failed to attain such a degree of efficiency as was consistent with the flourishing condition of science in France, there was one important department of practical astronomy in which that country took the lead of all other nations. It was in France that the length of an arc of the meridian was first measured, upon principles so unexceptionable, and with so scrupulous a regard to accuracy in all the details of the operation, as to command the confidence of astronomers generally in the result, and to induce Newton to resume the ever memorable investigation which conducted him to his grand discovery of the principle of universal gravitation. It is to France also that astronomy owes the establishment of the ellipticity of the earth, one of the most conclusive facts that can be adduced in favour of the Newtonian theory. This object was effected by a comparison of geodesical measurements, executed not merely on the soil of France, but also in the arctic regions and under the equator. The operations connected with the establishment of these results extended over nearly three-quarters of a century, and exercised the talents of the most eminent astronomers of France who flourished during that period. The name of Picard is imperishably associated with the origin of these operations which have reflected so much honour on France. Their close was illustrated by the exertions of Lacaille, one of the most distinguished astronomers of the eighteenth century*.

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* See a paper by Mr. Baily on this subject in vol. viii. of the *Memoirs of the Astronomical Society*.

† Histoire de l'Astronomie au Dixhuitième Siècle, p. 134.

‡ It would appear, from the following expression, that Halley did not contemplate the possibility of determining the apparent position of a celestial body within even 10″ of the truth:—"Ut verum fatear, minuta secunda vel etiam dena secunda, instrumentis quantumvis affabre factis certo distinguere vix homini datum est."—(*Phil. Trans.*, 1716, p. 456.)

§ Mémoires pour servir à l'Histoire des Sciences, etc., p. 191.

In connexion with the Royal Observatory of Paris may be mentioned Louville's application of the micrometer to divided instruments. This was effected by directing the instrument approximatively to the object, and then adjusting it, so that the plumb-line fell exactly upon the nearest point of division. The moveable thread of the micrometer was then brought into exact coincidence with the object, and its distance from the fixed thread, as determined by the revolutions of the screw, gave the fraction of an angle which was to be added to or subtracted from the altitude indicated by the point of division over which the plumb-line passed. Louville first described this mode of applying the micrometer in the "Memoirs of the Academy of Sciences" for 1714†. A sector, furnished with a micrometer of this kind, was shortly afterwards employed by the astronomers engaged in verifying the arc of the meridian that had been measured in France in the seventeenth century. It would appear that a similar application of his instrument did not escape the attention of Gascoigne, the original inventor of the micrometer. Thus, Crabtree, writing to him in a letter dated December 6, 1641, expresses his astonishment that he should be enabled not only to project the diameter of Jupiter into prodigious measures, but also to take distances, altitudes, inclinations, and azimuths *without the limb* of an instrument, and each to any required number of parts‡.

The difficulty of making accurate observations from the deck of a vessel, arising from the unsteady position of the observer, was found to operate as an insuperable obstacle to the success of any method for finding the longitude at sea. This difficulty was entirely removed by the invention of the reflecting quadrant. The germ of this invaluable instrument is to be found in Sprat's "History of the Royal Society." Thus, in an account of the inventions of members of the Society, he alludes to "a new instrument for taking angles by reflection, by which means the eye at the same time sees the two objects, both as touching in the same point, though distant almost to a semicircle, which is of great use for making exact observations at sea."§ The author of this instrument was Hooke, in whose "Posthumous Works" there is to be found a description of it‖; but it does not seem that he attempted to follow out his ingenious idea by a further improvement of the instrument. Newton was the first person who devised the construction of the reflecting quadrant, resembling the sextant of the present day. He communicated an account of it to Halley, who, however, omitted to impart a knowledge of it to the world; but soon after his death, in 1742, a description of the instrument in Newton's handwriting was found among his papers*. In the meantime the instrument had been re-invented by two individuals, independently of each other. The reflecting quadrant of Hadley was first described at a meeting of the Royal Society, held on the 13th of May, 1731†. It was subsequently found that his invention of the instrument could be traced back at least as early as the summer of 1730. The other inventor was Thomas Godfrey, of Philadelphia, in America. He appears to have devised an instrument similar to Hadley's towards the close of the year 1730. The Royal Society, which took into consideration the claims of the two individuals, decided that both were independent inventors.

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* Picard commenced his operations for the measurement of an arc of the meridian in 1669. The arc of the meridian in Lapland was measured by Maupertius and his companions in 1735-6. The ellipticity of the earth's figure was finally established beyond all doubt by the verification of the French arc of the meridian in 1741. The operations connected with the measurement of the arc in Peru were completed a few years afterwards.

† Mém. Acad. des Sciences, 1714, p. 73.

‡ Phil. Trans., 1717, p. 609. In 1669 Hooke determined the zenith distances of *γ Draconis*, by the aid of a micrometrical contrivance similar to that of Louville's, inasmuch as the intersection of cross wires of the telescope was not brought into exact coincidence with the star; but in this case the deviations of the star from the optical axis of the telescope were measured by means of a graduated scale.

§ History of the Royal Society, p. 246, 4to. Lond., 1667.

The idea of enlarging Hadley's reflecting quadrant, or rather octant, so as to measure a distance of 120°, was first proposed in 1757, by Captain Campbell, who suggested to the Board of Longitude, that a sextant of large radius, divided by Bird, would serve for making observations at sea, equally well with the reflecting circle as described by Mayer‡. The sextant is the form in which the reflecting instrument has since been generally used for nautical purposes.

Accurate celestial observations being now practicable on board a ship by the use of the sextant, it only remained to construct trustworthy lunar tables, in order to perfect the method of finding the longitude at sea, by means of a comparison between the observed and computed distances of the moon from the neighbouring stars. Many years were not allowed to elapse ere this important object was accomplished, as we shall presently have occasion to mention.

James Bradley, the third Astronomer Royal of Greenwich, was born at Sherbourn, in Gloucestershire, in the year 1692. In 1711 he entered Baliol College, Oxford, where he completed his education. Having qualified himself for the church, he was presented to a living in the year 1719. His predilection for astronomical pursuits, which evinced itself at an early age, was fostered by his uncle, the Rev. James Pound, with whom he resided for several years at Wanstead, in Essex. In 1721 he was appointed to the Savilian chair of astronomy in the University of Oxford, which had then become vacant by the death of Keill. His nomination by the Government, as successor to Halley in the Observatory of Greenwich, is dated February 2, 1742. His reputation as an astronomer was already well established in Europe by observations of a miscellaneous nature, and more especially by his immortal discovery of the Aberration of Light, and at the time of his appointment he was actually engaged in those researches which resulted in his discovery of the Nutation of the Earth's axis.

Bradley's first object after his removal to Greenwich was to repair the instruments, and carefully to adjust their positions. Halley had confined himself to the use of the mural quadrant soon after its erection in 1725 *. Bradley, however, resolved to employ both the quadrant and the transit instrument in his observations. The former of these instruments was repaired by Graham, and the latter by Sisson, several important improvements being at the same time effected in their construction. Bradley's first observation with the quadrant was made on the 15th of June, 1742. His earliest transit observation is dated the 20th of July in the same year.

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* A drawing and description of the instrument, both taken from Newton's paper, are inserted in the *Phil. Trans.* for 1742, p. 155.

† For a description of Hadley's reflecting octant see *Phil. Trans.*, 1731, p. 147. An examination of the question relative to the invention of this instrument will be found in the *Nautical Magazine*, vol. i., p. 351.

‡ Bradley's "Miscellaneous Correspondence," p. 89.

The mode in which Graham's iron quadrant was divided has been already mentioned. Halley generally adopted the nearest reading of the vernier without regard to the fractional parts, but occasionally he marked the half of the interval between two divisions of the vernier. The error of his observations might, consequently, amount from this cause to 5″ or 6″. Bradley pursued a similar plan of observation till July 18, 1745, when, having applied a new micrometer screw to his instrument, the threads of which were much finer than those of the old one, he employed it to measure the fractional parts of the angles. One revolution of the screw altered the angle upon the instrument 53″; and, consequently, the fifty-third part of a revolution was equivalent to 1″. By this application of the micrometer, Bradley was now enabled to make observations to seconds with as great accuracy as he had formerly done to divisions of the vernier †.

The iron quadrant of Graham continued to be attached to the eastern face of the wall erected for its support in 1725, and was employed both by Halley and Bradley in observing the celestial bodies which passed the meridian to the south of the zenith. Bradley, however, was desirous of extending the plan of his observations, and with a view to this object he presented a memorial to the Government in the year 1749, soliciting another quadrant, by means of which the stars that passed the meridian to the north of the zenith might be observed. The Government, with a promptitude hitherto unusual, at once acceded to his views, and in the following year the observatory was furnished with a magnificent brass quadrant of eight feet radius constructed by Bird ‡, who now took the place of Graham, as the most skilful divider of instruments in his day. As in the case of the iron quadrant, there were two different modes of division executed upon the limb. By the inner arc the quadrant was

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* This course was imperatively prescribed by the necessities of his situation, for there was only one vertical wire in his transit, and he had no assistant by whose co-operation he might be enabled to make simultaneous observations with two instruments. As soon as Bradley was appointed Astronomer Royal he succeeded in procuring a regular assistant, who was paid by the Government. His first assistant was his nephew, John Bradley, who, according to Lalande, resided in that capacity at the Royal Observatory for a period of fifteen years.

† Rigaud's *Miscellaneous Works, &c., of Bradley*, p. lv.

‡ John Bird was born in 1709; he was brought up a cloth-weaver in the county of Durham. It is said that his attention was first drawn to the dividing of instruments, by observing the imperfect graduation of a clock dial. Having acquired considerable expertness in his new vocation, he proceeded to London, where he was employed by Graham and other artists of the day. After residing in London for a few years, he commenced business on his own account, and soon attained an unrivalled reputation as a divider of instruments. In 1767 he received £500 from the Government, on condition that he should divulge his method of graduation. This he did in a work which he published on the subject. (*The Art of Dividing Instruments*, London, 1767.) He died on the 31st of March, 1776.

divided to every 5′; then the intervals between two consecutive divisions were subdivided by the vernier to every 30″, and these were finally subdivided by the micrometer screw to every 1″. By the outer arc the quadrant was divided into ninety-six parts; then these were subdivided by the vernier into sixteen smaller parts, each equal to 13″.1836; and these were finally subdivided by the micrometer screw to every 1″. The Government at the same time furnished the observatory with a new transit telescope by Bird, eight feet long, besides an excellent clock by Shelton, and several other instruments of minor importance. They also purchased of Bradley the famous zenith sector with which he discovered the phenomena of aberration and nutation, and appropriated it to the use of the observatory. This noble instrument was designed by Bradley to be henceforward employed in determining the errors of collimation of the quadrants, by making observations with it when its face was turned alternately east and west.

Bradley being now in possession of instruments of unequalled perfection, proceeded to execute a series of preliminary observations for the purpose of ascertaining with greater accuracy the latitude of his observatory, the place of the equinox, the quantity and laws of refraction, and other fundamental points of astronomy. In pursuance of this design the brass quadrant was turned towards the north, so as to be employed in making observations of circumpolar stars. These observations were prosecuted from August 10, 1750, to July 31, 1753. From them Bradley deduced the latitude of the observatory to be 51° 28′ 38½″. He also succeeded by means of them in constructing the elegant rule, so long used by astronomers, for finding the quantity of refraction corresponding to any assigned zenith distance, and any observed readings of the barometer and thermometer. He determined the absolute right ascensions of a few of the principal stars, by means of observations made near the equinoxes, according to the method of Flamsteed.

As soon as Bradley had established these fundamental points he removed the brass quadrant from the western face of the meridian wall, and permanently attached it to the eastern face, where it was afterwards employed in observing the stars that passed the meridian to the south of the zenith. At the same time the iron quadrant was removed from the eastern face of the wall, and, after being re-divided by Bird, was attached to the western face, for the purpose of making observations with the telescope turned towards the north. Bradley now commenced the series of admirable observations which have formed the groundwork of so much valuable research to future enquirers, and which would have assured to him an immortal reputation, even independently of those great discoveries with which his name is inseparably associated. The sun, moon, and principal stars, and the planets when situate in favourable positions, were regularly observed with the transit instrument and the mural quadrants. Moreover, a multitude of small stars, chiefly those of Flamsteed's catalogue, were included in the plan of observation. From the year 1750 may be dated the commencement of a series of observations which in point of accuracy may bear a comparison with those of modern times. Henceforward the records of Greenwich Observatory embody a collection of materials, which have almost exclusively formed the groundwork of every investigation undertaken in modern times, for the purpose of improving the solar, lunar, or planetary tables.

duity to prosecute his labours at the Royal Observatory, until at length, in the autumn of 1761, he was compelled by the increasing frailties of old age, to retire from the duties of active life. "Our eye," says his learned biographer, the late Professor Rigaud, "looks into the Greenwich registers with feelings of interest for the traces of that hand which conveyed so much instruction to mankind, and catches occasionally the sight of it till September 1, 1761, when the sun's transit was the last observation that Bradley ever entered, most probably that he ever made. His existence continued for a few months longer, but his scientific career was closed."* In fact, he shortly afterwards withdrew to Chalford, in Gloucestershire, where he continued to reside among his wife's relations till his death, which took place on the 13th of July, 1762, having attained the age of 70 years†.

The registers of Bradley's observations occupied thirteen folio volumes, and at his death were taken possession of by his executors. In 1767 the Government, under the impression of their being public property, commenced a law-suit with a view to their recovery, which, however, they abandoned in 1776. As soon as it was ascertained that the Government had relinquished their claim, the manuscripts were transmitted to Lord North, who was then Chancellor of the University of Oxford, to be presented by him to the University. They were finally printed, at the expense of the University, in two folio volumes. The first volume was published in 1798, under the superintendence of Dr. Hornsby. The second volume was prepared for the press by Dr. Robertson, and appeared in 1805. These two volumes contain the observations made by Bradley from 1750 to 1762. The original manuscripts, as well as the registers of the observations made by Bradley, at Greenwich, previous to 1750, are deposited in the Bodleian Library, Oxford.

The vast mass of observations made by Bradley, which were published in two volumes, as above mentioned‡, continued inapplicable to any useful purpose, in consequence of their not being reduced, until, at length, Bessel undertook to execute this task, in so far as regarded the observations of the stars. The results of his labours were published in 1818, at Königsberg, in one folio volume, entitled, "*Fundamenta Astronomiæ pro anno* 1755, *deducta ex Observationibus viri incomparabilis James Bradley, in specula Astronomica Grenovicensi per annos* 1750–1762 *institutis.*" In this work Bessel has determined, by a series of elaborate investigations, the quantity and laws of refraction, the maximum value of aberration, and other fundamental points of astronomy, as deducible from Bradley's observations. He estimates the error in declination of these observations to be gener[illegible] less than 4″, and the error in right ascension less than 1″ of time.

The observations made with the zenith sector at Kew and Wanstead, by the intercomparison of which Bradley was conducted to the discovery of aberration, were at one time supposed to be lost; but having been found among the papers of the late Dr. Hornsby, they were published by Professor Rigaud, in 1832, at the expense of the University, of which the immortal author of the discovery forms one of the brightest ornaments*.

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* Bradley's *Miscellaneous Works* (Memoirs, p. 100).

† In 1751 a pension was granted to Bradley by George II., not as an augmentation of salary, but as a gratuitous acknowledgment of his personal merit.

‡ Rigaud considers that their number cannot be less than 60,000.

While Bradley was prosecuting his admirable observations at Greenwich, the Continent was adorned by two astronomers who are justly entitled to be associated in the history of astronomy with their illustrious contemporary. It will be desirable, before proceeding further, to present the reader with a brief notice of their labours.

Nicholas Louis de Lacaille was born at Rumigny, near Rosoy, in France, on the 15th of March, 1713. He was destined by his parents for the church, but his inclination towards astronomical studies effectually frustrated their design. In 1713 he proceeded to Paris, where he was kindly received by J. Cassini, who assigned to him a situation in the Royal Observatory. In 1738 he was employed, in conjunction with the younger Maraldi, in surveying the western provinces of France. In the two following years he took an active part in the operations connected with the verification of the French arc of the meridian, and it is to his energy and skill that the successful prosecution of that important undertaking is mainly to be ascribed. While engaged in these labours he was appointed Professor of Mathematics in the Mazarine College of Paris, whither he retired at their conclusion. A small observatory having been fitted up for his use at the college, he continued to labour there for several years, dividing his time between his professional duties and astronomical pursuits. In 1750 he proceeded to the Cape of Good Hope, for the purpose of making observations in the southern hemisphere. During his residence there, he amassed materials for determining the parallaxes of the Moon, Venus, and Mars, and for investigating the laws of refraction. He also measured an arc of the meridian, and the length of the second's pendulum, besides observing ten thousand stars in the southern hemisphere. Upon his return to Europe, in 1752, he established himself in his former place of residence, where he proceeded to deduce a number of important results from his observations. In 1757 he published a fundamental catalogue of stars, forming one of the most valuable contributions made to astronomical science during the eighteenth century. In the following year he published his solar tables, the elements of which were determined with such precision as to leave little further to be desired. These are the earliest solar tables in which the effects of planetary perturbation are taken into account. He is also the author of a catalogue of about 500 zodiacal stars, which was published after his death.

The advantages which Lacaille enjoyed in the prosecution of his labours were extremely moderate. He had no assistant to relieve him from a portion of the drudgery incidental to astronomical pursuits, nor were his instruments by any means so perfect as those of some of his contemporaries. By his talents and perseverance, however, he succeeded in overcoming these difficulties; but, unfortunately, he fell a victim to incessant application, when he had only attained an age at which the human constitution usually retains its full vigour. He died on the 21st of March, 1762, in the 49th year of his age.

When the admirable labours of Lacaille are taken into consideration, it is impossible to repress a feeling of regret that he was not placed in a position wherein he might have been enabled to exercise his talents with still greater benefit to astronomical science. Upon this point his distinguished countryman, M. Biot, very naturally makes the following remark:—"*Quel éclat aurait jeté l'astronomie observatrice en France, quelle hauteur aurait-elle pu atteindre, dès ce temps-là même, si un tel homme avait succédé à Picard et à Roemer, dans un observatoire vraiment royale, créé avec l'intelligence de la science, disposé pour ses besoins, et pourvu des instruments propres à l'avancer!*" *

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* "Miscellaneous Works and Correspondence of the Rev. James Bradley, D.D., F.R.S.," 4to., Oxford, 1832.

Tobias Mayer was born on the 17th of February, 1723, at Marbach, a town of Wurtemberg, in Germany. His father having died while he was yet young, he was thrown upon his own resources at an earlier age than usual, and was compelled to support himself by teaching mathematics, a knowledge of which he had acquired solely by the perusal of such books as he could occasionally procure. He was only twenty-two years old when he published a treatise on the theory of curve lines. About the same time he began to apply himself with great ardour to the prosecution of astronomical inquiries. In 1750 he communicated to the Cosmographical Society of Nuremberg, a paper on the libration of the moon, in which he determined the elements of that curious phenomenon with greater precision than had been hitherto done by any astronomer. This paper is remarkable for containing the earliest example of the method of *equations of condition*, the use of which has contributed so much to the improvement of modern astronomy. In 1751 he was appointed to the superintendence of the Observatory of Gottingen, having been at the same time nominated to one of the chairs in the University of that city †. The observatory had been recently furnished by George II. of England with a magnificent mural quadrant of six feet radius, constructed by Bird. Mayer proceeded to submit his instrument to a most scrupulous examination, and in the execution of this task he displayed a combination of sagacity and address which would alone suffice to stamp him as an observer of the highest order. He now commenced a course of regular observation, with the view of procuring materials to serve as a groundwork for establishing the fundamental points of astronomy. In 1753 he published new solar and lunar tables. The latter will be for ever memorable, from being the first which afforded a practical solution of the problem of the longitude. The observations employed in their formation consisted chiefly of eclipses of the sun and occultations of the stars. They did not amount to more than 200, and yet, with such skill were they discussed by Mayer, that his tables seldom erred so much as $1\frac{1}{2}'$. In 1755 he transmitted them to England, where they were compared by Bradley with the Greenwich observations, and were deemed by that astronomer to be so accurate that, in so far as the calculated places of the moon were concerned, they might serve in finding the longitude at sea within half a degree of the truth, and generally much nearer ‡. In 1756 Mayer communicated a paper to the Royal Society of Gottingen, in which he explained the principle of an instrument, since well known to astronomers as the repeating circle. He continued to prosecute his astronomical researches with unremitting assiduity, until at length his constitution became impaired from incessant application, and he fell into a languishing illness, of which he expired on the 20th of

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* Journal des Savans, 1847, p. 524.

† This was the chair of political economy. It does not appear that Mayer delivered any lectures upon that subject.

‡ Mayer's Solar and Lunar Tables, p. cxii.

February, 1762[*]. An improved set of lunar tables, which he left behind him at his death, were transmitted to England by his widow, in consideration of which she obtained from the British Government a grant of money amounting to £3000[†]. In 1770, his Theory of the Moon[‡], which served as the basis of his tables, was published at London. It is substantially the same as Euler's, but the values of the coefficients were determined by the aid of his own observations. In the same year there was published, also at London, his improved solar and lunar tables[§]. There is appended to them, a short dissertation entitled *Methodus Longitudinum Promota*, in which he recommends the method of lunar distances for finding the longitude at sea, and suggests for this purpose the use of a reflecting circle, with the repeating principle applied to it. This work also contains his formula for refraction, embodying the result of his researches on that important subject[||]. The two works above mentioned were published at the expense of this country, under the superintendence of the Board of Longitude, the task of editing them being executed by Dr. Maskelyne. In 1775 the first volume of Mayer's "Posthumous Works" was published at Gottingen, which, among other subjects, contained a catalogue of 998 zodiacal stars. The remaining portion still exists in manuscript, with the exception of the observations from which the catalogue was formed, which were printed at London, in 1826.

Mayer has earned for himself an imperishable reputation by his lunar tables. Some astronomers may have achieved results of greater brilliancy, but few have been privileged to contribute so directly by their labours to the well-being of their fellow-men. Even independently of the improvement which he effected in this instance, Mayer possesses claims to be ranked with the greatest astronomers of ancient or modern times; but, as in the case of either of his illustrious contemporaries, Bradley or Lacaille, his labours are not of a nature to be generally appreciated, and therefore his reputation is less widely diffused than that of many an individual whose contributions to science, though more showy, are infinitely less substantial.

Bradley was succeeded at the Royal Observatory of Greenwich by Dr. Bliss, Savilian Professor of Geometry in the University of Oxford. This astronomer adopted the same plan of observation as that pursued by his predecessor, but his death in 1765 prevented his labours from acquiring any degree of importance in the annals of the observatory.

Neville Maskelyne, the fifth Astronomer Royal of Greenwich, was born in London in the year 1732. He completed his education at Trinity College, Cambridge, and subsequently qualified himself for entering the church. It is said that his attention was first drawn to the science of astronomy by the great solar eclipse of 1748. He contracted an early intimacy with Bradley, whom he assisted in the computations connected with his researches on refraction. At the recommendation of that astronomer, he proceeded to the island of St. Helena, to observe the transit of Venus, in 1761. He commenced his labours at the Observatory of Greenwich on the 7th of May, 1765. His instruments were the same as those used by Bradley and Bliss, but his plan of observation was more limited than that pursued by his predecessors. He confined his attention to the sun, moon, and planets, and a select number of stars. The errors of collimation of his quadrants were determined from time to time by observations of γ *Draconis*, with the zenith sector having its face turned alternately east and west. During the intermediate intervals, the errors were deduced by means of the quadrants themselves, from observations of the stars in the feet of the constellation *Gemini*. We have seen that Flamsteed employed observations of the same stars, for the purpose of determining the error of collimation of his mural arc. In order that he might be enabled to deduce from his observations the absolute right ascensions of the sun, moon, and planets, it was necessary to establish with great precision a definite number of reference points along the contour of the zodiac. With this view he selected thirty-six stars situate near the equator, each of which was sufficiently bright to be observed in the daytime, even when not very far removed from the sun. In order to render the observations of these stars available for ulterior purposes, it was necessary to ascertain their places with respect to the equinox. This was effected by comparing one of them, α *Aquilæ*, with each of the remaining thirty-five, and then comparing α Aquilæ directly with the sun. The mode of investigation pursued by Maskelyne on this occasion was founded mainly upon Flamsteed's method for determining the absolute right ascension of a star. He continued to observe these stars on every practicable occasion, and employed the results in the formation of a small catalogue which, in point of accuracy, far exceeded anything of the kind hitherto executed. By comparing the observed transits of these standard stars with the corresponding results deduced from the catalogue, it was possible to ascertain the exact error of the clock, and hence to determine the right ascensions of the sun, moon, and planets.

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[*] It is somewhat singular that Bradley, Lacaille, and Mayer, the three greatest astronomers of the eighteenth century (supposing Halley to have been of the seventeenth), died within a few months of each other.

[†] A copy of the resolution of the Board of Longitude, recommending Parliament to grant Mayer's widow the sum of £5000, will be found at page cxxiii. of the Solar and Lunar Tables of that astronomer. It appears from Maskelyne's preface to these Tables that the sum actually awarded by Parliament amounted to £3000.

[‡] Theoriæ Lunæ juxta Systema Newtonianum, &c., 4to., Lond., 1767. Although the title-page of this work bears the date of 1767, it was only published in 1770, as appears from Maskelyne's preface to Mayer's Solar and Lunar Tables, published in the last-mentioned year.

[§] "Tabulæ Motuum Solis et Lunæ, novæ et correctæ," &c., 4to., Lond., 1770.

[||] See page 117 of the work referred to in the text.

Maskelyne first introduced the practice of observing a star at the five vertical wires of the telescope. This mode of observation was pursued by him with undeviating regularity throughout his long career. He was also the first who noted the instant of transit in terms of the tenths of a second *. He continued to prosecute his labours without interruption, —adhering constantly to the same plan of observation which he had traced out in the beginning of his career—till his death, which happened on the 9th of February, 1811, having occupied the situation of Astronomer Royal for a period of forty-six years.

It is to Maskelyne that the original establishment of the "Nautical Almanac" is due. In 1763 he published a work, entitled "The British

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* In noting the passage of a star over the wires in the transit, Bradley in general took the nearest second, except when the interval was perceptible, in which case he marked the observation * or —, to indicate that the true time exceeded or fell short of the time recorded. In some cases the time of transit is estimated in terms of the half or third of a second. Maskelyne, in the beginning of his career, divided the second into eight parts. In September, 1772, he first commenced the practice of recording the times of transit to tenths of a second.—(*Miscellaneous Works of Bradley*, p. liv.)

Mariner's Guide," in which he pointed out the advantage, for finding the longitude at sea, of calculating beforehand for each year the distances of the moon from the sun, and the principal stars lying near her path, and inserting the results in an annual ephemeris. This suggestion was approved of by the Government, and he was forthwith appointed to carry it into effect. The "Nautical Almanac" first appeared in 1767. During the remainder of his life Dr. Maskelyne continued to superintend the publication of this ephemeris, which has proved so valuable to the mariner.

Maskelyne had not long prosecuted his labours at the Observatory of Greenwich, when he succeeded in inducing the Government to print his observations annually. The whole results have been published in four folio volumes. From their uniformity, their continuity, and their general accuracy, they have proved of inestimable value in many important investigations relating to astronomical science.

It has been already mentioned that Roemer did not fail to perceive the superior advantage of circular instruments in making astronomical observations, when compared with quadrants, or any other sections of a circle, and that, in accordance with this view, he procured the construction of a meridian circle. The example of the Danish astronomer does not seem to have been imitated by any of his contemporaries, nor was the idea of circular instruments revived, until Mayer proposed the construction of the repeating circle. In 1757 a reflecting circular instrument, constructed according to the description which Mayer had transmitted to England along with his lunar tables, was actually employed by Captain Campbell in making observations at sea, but it was speedily replaced by a sextant of twice the radius, as being more easily manageable *.

Towards the close of the last century the Danish astronomer, Bugge, made observations with an altitude and azimuth circle. The diameter of the vertical circle was four feet †. In 1789 a vertical circle of five feet in diameter was constructed by Ramsden ‡ for the celebrated astronomer Piazzi, who employed it in determining the declinations of his great catalogue of stars. Circular instruments of various forms soon afterwards came into general use.

About the same time that circular instruments were again applied to the purposes of astronomical observation, the method of reading off the angles by means of microscopes fixed in piers opposite to the divisions of the limb was also restored. In the present instance, however, an important improvement of Roemer's method was effected, for, instead of subdividing the interval between two consecutive points of division, by means of parallel threads placed in the focus of the microscope, and then *estimating* the fractional parts of these subdivisions, the exact distance of

* Mayer's Tables, p. cxi.

† Lalande, *Astronomie*, art. 2333.

‡ Jesse Ramsden was born at Salterhebble, near Halifax, Yorkshire, in the year 1735. Having obtained a sound elementary education, including some knowledge of geometry and algebra, he was subsequently apprenticed to a clothworker at Halifax. In 1755 he proceeded to London, where he found employment as a clerk in a warehouse, but having a strong inclination towards scientific pursuits, he bound himself as an apprentice for four years to a mathematical instrument maker. Soon after he completed his engagement, he commenced business on his own account, and eventually attained unrivalled excellence as an artist. Several of the finest astronomical instruments used in the public observatories of Europe towards the close of the eighteenth century were the productions of his ingenuity and skill. He also constructed a great number of achromatic telescopes, and other philosophical instruments. He was a Fellow of the Royal Society, and also one of the Foreign Members of the Imperial Academy of St. Petersburg. He died on the 5th of

the index from the nearest point of division was ascertained by means of a micrometer screw. This mode of reading off angles was first proposed by the Duc de Chaulnes, in a small work which he published at Paris in the year 1768. Ramsden did not fail to see the advantages of it, and accordingly he applied it to the altitude and azimuth circle which he constructed for Piazzi.

Towards the close of his career at Greenwich, Maskelyne began to suspect, from certain irregularities in the declinations of the stars, as determined with Bird's mural quadrant, that the instrument had become defective from long use. This fact having been established beyond doubt by evidence derived from other sources, he represented to the Government the expediency of replacing it by a circular instrument. This request was acceded to, and a mural circle of six feet in diameter was constructed for the Royal Observatory by Troughton[*], but it was not actually finished when Maskelyne died.

John Pond, the successor of Maskelyne at the Royal Observatory of Greenwich, was born in London in the year 1767. At the age of sixteen he was placed at Trinity College, Cambridge. While engaged in prosecuting his studies there, he displayed an interesting proof of his inclination towards those pursuits which subsequently formed the occupation of his life, by uniting with two of his fellow-students in soliciting Prof. Vince to give a course of lectures on practical astronomy. After leaving Cambridge, he proceeded to the Continent, where he continued to travel for some time. Upon his return to England he established his residence at Westbury, in Somersetshire, and devoted his whole attention to practical astronomy. Having procured an altitude and azimuth circle of three feet diameter, constructed by Troughton, he commenced a series of observations with it, and so successfully did he prosecute his labours, that in 1806 he was enabled to demonstrate by irrefragable evidence founded upon the results, that the brass quadrant of Greenwich had undergone a change of form since its erection in Bradley's time[†]. His earliest observation at Greenwich is dated the 11th of January, 1811. In the month of June of the same year the mural circle which Troughton had been charged to construct for the Observatory was ready for use. This magnificent instrument demanded a method of observation totally different from that hitherto pursued with the quadrants. In his mode of using it Pond shewed that he possessed talents adequate to the occasion. The observations of a celestial object were now made, not by determining the distance from the zenith agreeably to the practice hitherto pursued with the quadrants, but by referring the object to the pole as the zero of measurement, the position of the latter being ascertained by observations of circumpolar stars. The instrument, according to this practice, served to determine the polar distance of an object, and therefore the results were totally independent of the place of observation. The error of the instrument was ascertained by comparing the observed places of certain standard stars with their places as deduced from a fundamental catalogue. The difference between each observed and calculated result gave a corresponding value of the error of the instrument for the day of observation, and the mean of all such differences gave the mean error of the instrument, or, as Pond termed it, the *index error*. This quantity, being then applied to all the observations, gave the polar distances of the various objects, freed from the effects of instrumental error. In the case of the standard stars, the application of the index error supplied the means of obtaining a correction of their places as assigned by the catalogue, and it is manifest that by frequently repeating the same process the catalogue would be rendered more and more perfect*. This method of observation obviated the necessity of employing the plumb-line, the use of which was not, indeed, considered by Troughton in his original design of the instrument; while at the same time it dispensed with an exact knowledge of the latitude of the Observatory †.

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[*] Edward Troughton was born in 1753 in the parish of Corney in Cumberlandshire. He was brought up to farming pursuits till he was seventeen years of age, but he then proceeded to London, and acquired instruction as an artist under his brother John. In 1782, the two brothers commenced business on their own account, and by their industry and skill soon attained a prosperous condition. Upon the death of his brother, Edward continued to prosecute the business of an artist for many years, and ultimately acquired a European reputation by his excellence as a constructor of astronomical instruments. In 1830, he received an honorary gold medal from the King of Denmark. He died on the 12th of June, 1835. A marble bust of this distinguished artist adorns the Observatory of Greenwich. He was the last of a succession of English artists, each of whom was without a rival in his day. It is somewhat singular that Sharp, Graham, Bird, Ramsden, and Troughton should have succeeded each other at almost equal intervals; and still more so, that they should all have been born in a tract of country comprehended within the limits of three adjacent counties of the north of England.

[†] Phil. Trans., 1806, p. 420, et seq.

In 1816 the Royal Observatory was enriched by the accession of a new transit instrument ten feet long, constructed by Troughton. Henceforward the Greenwich determinations, both in polar distance and right ascension, acquired a degree of precision which has hardly been surpassed by the most accurate observations of the present day.

In the course of his famous controversy with Brinkley on the parallax of the fixed stars, Pond was led to employ occasionally a new method of observation which he subsequently practised with great success. It is essentially founded upon the principle of optics that when a ray of light is reflected from any surface, the angles of incidence and reflexion are equal to each other. In virtue of this principle, it is clear that when a celestial object is observed with the mural circle by direct vision, and also by reflexion from the surface of a fluid, the half of the angle contained between the two readings of the circle will assign the altitude of the object, and the reading which corresponds to the point of bisection will indicate the *horizontal point.* Hence, knowing the latitude of the place of observation, the polar distance of the object may at once be determined. Pond first applied this principle by observing the object at one of its transits by direct vision, and then at the next transit by reflexion from the surface of a trough of mercury. This method was attended, however, with inconvenience, inasmuch as, in consequence of unfavourable weather, several days might intervene before a complete observation could be obtained. Moreover the result was liable to some degree of uncertainty, arising from the different effects of refraction at the beginning and end of the interval, and from causes tending to produce a slight derangement in the adjustment of the different parts of the instrument. Impressed with a sense of these defects, Pond contrived a method of observing a celestial object at one transit both by direct and reflected vision, founded upon a combination of simultaneous observations made with two mural circles*. A mural circle by Jones, that had been destined for the Cape of Good Hope, was employed, along with Troughton's circle, in testing the practicability of this method. The experiment having been attended with complete success, Pond solicited from the Government the permanent use of the second circle, and his request being complied with, he now introduced the method into general practice.

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* The process thus indicated would not free the polar distances of the catalogue of a common error, but Pond shewed how this object might be effected by observations of circumpolar stars.—(*Phil. Trans.*, 1818, p. 410.)

† A knowledge of the latitude was necessary for transforming the polar distances into altitudes, with a view to compute the effects of parallax and refraction, but it is manifest that, for such a purpose, extreme precision was not required.

Pond commenced his regular observations by direct and reflected vision in the month of September, 1825. This method of determining the zenith point continues still to be practised at Greenwich, except in so far as relates to the use of two circles, the present Astronomer Royal having devised a mode of effecting the same object by means of only one circle.

Pond did not fail to make regular observations of the sun, moon, and principal stars, but he devoted less attention to the planets than the importance of improving their respective theories demanded. In sidereal astronomy generally, his contribution to the existing stock of knowledge was much more extensive than that of his predecessor. His peculiar methods for eliminating the effects of instrumental error, were founded upon a skilful combination of masses of observations, and he was led in consequence to determine the places of a considerable number of stars. In 1833 he published a catalogue of 1112 stars, which proved a valuable acquisition to the practical astronomer. It is generally admitted to be one of the most accurate productions of the kind that has ever been given to the world.

In the autumn of 1835, Pond was compelled by ill health to resign his appointment. The Government manifested a just appreciation of his merits, by bestowing upon him a retiring pension, but he was not destined long to enjoy the fruits of their liberality. He died on the 7th of September, 1836, and was buried at Lee in Kent, in the same tomb with Halley.

George Biddel Airy, Esq., the present Astronomer Royal, commenced his labours at Greenwich as the successor of Pond, on the 2nd of October, 1835. He had already earned a distinguished reputation by his researches on physical astronomy, and by his directorship of the Observatory of Cambridge, to which he was appointed in the year 1828. During the period of his connexion with the latter establishment, he introduced a practice which did not fail to exercise an important influence on the future character of the labour performed at public observatories in this country. It had been hitherto usual for the directors of such establishments, to confine their attention exclusively to observations of the celestial bodies, leaving to others the task of their reduction. Flamsteed, indeed, deserves to be cited as an honourable exception to this remark; and with respect to his successors, the circumstance of their having omitted to reduce their observations, must be attributed to the scantiness of the resources which the Government placed at their disposal, rather than to any inattention or apathy on their own part. There can be no doubt, however, that the restriction of their labours to mere observation, tended seriously to frustrate the very object for which public observatories have been established, namely, the attainment of a more accurate knowledge of the movements of the celestial bodies; for few individuals could muster sufficient courage to undertake the investigation of any subject relating to astronomical science, when, as a preliminary operation, it was found indispensable to execute an enormous mass of tedious calculations. Indeed it is not to be expected, that any one can take such an interest in the reduction of observations as the person who actually makes them, and hence it is reasonable to presume that he of all others is best qualified for the execution of so laborious a task. Under a strong impression of the imperfect state of all observations that were not immediately available for purposes of research, Mr. Airy, as soon as he was appointed director of the Cambridge Observatory, introduced a system of reduction as a normal part of his duties. The volume of results which henceforward issued annually from that establishment, formed a perfect model to the practical astronomer. Not only were the observations regularly reduced, but in so far as the planets were concerned, the results were compared in every instance with the corresponding numbers calculated from the most esteemed tables, so that the errors of the latter being thus exhibited in bold relief, served to invite rather than to repel the researches of the theoretical enquirer. This admirable system was shortly afterwards introduced at Greenwich.

Another practice of great utility, which Mr. Airy adopted during his directorship of the Cambridge Observatory, and which he followed up more effectually upon his removal to Greenwich, was that of making regular observations of the planets. This was a department of practical astronomy, which for a long time had been very much neglected at the Royal Observatory. The sun, moon, and principal stars were regularly observed as they passed the meridian; but, except during opposition, the places of the planets were very rarely determined. Mr. Airy's attention was strongly directed to the expediency of remedying this defect, by the difficulty which he experienced in procuring suitable observations of the planets, while engaged in researches on physical astronomy. The planets are now invariably observed at Greenwich on every practicable occasion, at whatever hour they pass the meridian.

From the intimate connexion which was found to subsist between the lunar theory and the problem of the longitude, the moon has formed an object of unremitting attention at the Observatory of Greenwich, ever since its establishment in 1675. This constancy of purpose has not failed to produce its due fruits, for it is universally acknowledged that, in so far as observation is concerned, the present advanced state of the lunar theory is almost exclusively attributable to the labours of the Greenwich astronomers. It is therefore an object involving in an especial degree the honour of the Royal Observatory, to render the observations of the moon as accurate and as complete as possible. In pursuance of this object Mr. Airy has recently introduced the use of an altitude and azimuth instrument at Greenwich, for the purpose of making observations of the moon in any part of her course above the horizon. Two distinct circumstances suggested the expediency of deviating in this respect, from the plan of observation hitherto pursued at the Royal Observatory. In the first place, it frequently happens, from the unfavourable state of the weather, that the moon cannot be seen when she is on the meridian. Secondly, although the heavens should be perfectly serene, it is found to be impossible to discern the moon on the meridian for several days before and after her conjunction with the sun, on account of the overpowering effulgence of the solar rays. It happened from this circumstance, that throughout an had never been observed at all. The altitude and azimuth instrument, devised by Mr. Airy for remedying this defect, was constructed by the joint exertions of Messrs. Ransome and May, engineers of Ipswich, and Mr. Simms of London, the successor of the celebrated Troughton. Both the vertical and azimuthal circles are 3 feet in diameter. The telescope attached to the instrument is 5 feet long, and is furnished with an object-glass of 3¾ inches aperture *. The observations with it were commenced on the 16th of May, 1847. In practice it has been found to work admirably. Mr. Airy considers the results to be hardly, if at all, inferior to those obtained by the use of the mural circle. The utility of the instrument in promoting the object for which it was designed, soon became apparent. It has been found that the moon may be observed with it in the morning and the evening, when she is only an hour distant from the sun. The consequence has been, that the number of lunar observations, contained in the volume that issues annually from the Greenwich Observatory is now about three times greater than it formerly was †, while, at the same time, the observations with the new instrument, possess this peculiar advantage, that many of them have been made in a part of the moon's orbit, where no observations at all had hitherto been supposed to be practicable ‡. It is manifest that this circumstance cannot fail to be attended with advantage in contributing towards the further improvement of the lunar theory.

The complete reduction of the observations of the moon and planets made at Greenwich since the middle of the last century, is a work with which the name of the present Astronomer Royal is imperishably associated. At the meeting of the British Association, held at Cambridge in 1833, Mr. Airy suggested the expediency of effecting a uniform reduction of all the observations of the planets made at Greenwich since 1750, the year when Bradley commenced his observations with the new instruments constructed by Bird §. He proposed, on that occasion, that if the Government should agree to defray the necessary expense, he would undertake gratuitously to superintend the whole operation. At the instance of the Association, the Government acceded to this proposal, and the work was forthwith commenced. The results were finally published in 1846, in one large quarto volume. The whole work is divided into five sections. In the first section the errors of the clock are deduced from the observations. In the second the errors of the instrument are similarly investigated. In the third the geocentric places are computed from the observations. In

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* A detailed description of this instrument will be found in the volume of the *Greenwich Observations* for 1847.

† During the first year which elapsed after the instrument was in operation, the number of lunar observations made with it amounted to 203. The number of similar observations made on the meridian during the same period was only 111.

‡ This must be understood only with respect to the sun. The moon had been observed from the earliest ages in every part of her *absolute* orbit; but as the perturbations of her motion depend mainly on her angular distance from the sun, it is obviously a matter of primary importance that she should be observed in every part of her course relative to that body.

§ Previous to the epoch referred to in the text, astronomers were not in the habit of recording their observations so fully as to admit of their being reduced in the present day with all desirable accuracy. Thus the indications of the barometer and thermometer had not been hitherto noted by any observer, not even excepting Bradley. Apart, therefore, from all consideration of the superiority of the instruments constructed by Bird, this circumstance alone would render it inexpedient to attempt the reduction of any anterior ob-the fourth the corresponding places are calculated from the best existing tables. In the fifth the observed and tabular places are compared together, and the resulting errors exhibited.

When we consider that the observations which form the subject of this immense volume, were made by various individuals, with instruments very different from each other, and by methods totally dissimilar, we may form some conception of the difficulty of eliminating the systematic errors to which they were liable, and assimilating them by legitimate principles so as to exhibit them in the character of one homogeneous collection of results. The execution of this great undertaking renders the Greenwich observations of the planets from 1750 to 1830 immediately available for purposes of theoretical research, leaving in this respect nothing further to be desired.

The reduction of the Greenwich lunar observations from 1750 to 1830 was undertaken at the public expense in consequence of a representation to that effect having been made to the Government by the British Association, agreeably to a suggestion of Sir John Lubbock's on the occasion of the meeting of the Association, which was held at Liverpool in 1837. The organization of the plan of reduction, as well as the superintendence of its execution, was in this instance also confided to Mr. Airy. In consequence of the multitude of inequalities by which the moon's motion is affected, the undertaking was one of stupendous magnitude. In order to form some idea of the labour expended in its execution, it may be mentioned that upwards of 8000 places of the moon were deduced from the observations, and compared with the corresponding places calculated from the tables. An extraordinary force of computers was employed for several years at the Royal Observatory in these calculations. The results were finally published in the year 1848, in two quarto volumes *. Mr. Airy, who is distinguished no less as a theoretical than as a practical astronomer, has made them the subject of profound discussion, with a view to obtain a more accurate knowledge of the moon's motion, and has announced the conclusions at which he arrived in a paper which appears in the seventeenth volume of the "Memoirs of the Astronomical Society."†

Allusion has already been made to Pond's method of determining the zenith distance of a celestial body at one transit by a combination of

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* The calculations connected with this great undertaking were superintended by Mr. Hugh Breen, who had the melancholy satisfaction of just living to see its completion. Mr. Airy, who bears honourable testimony to his skill and accuracy, states, at the beginning of the first volume of the published results, that after a short illness he expired on the morning of April 1, 1848, only a few hours having elapsed after the last supplementary tables had been sent to the press.

† Allusion has already been made to some of the more important results of these researches in a former part of this work. Another very interesting confirmation of them is mentioned by Prof. Hansen, in a letter to the Astronomer Royal, dated June 27, 1850. (*Mo. Proc. Ast. Soc.*, June, 1850.) Mr. Airy had deduced $+ 1'.721$ as the correction of Damoiseau's value of the secular motion of the lunar node. Prof. Hansen, having caused one of his assistants to compute, by the modern tables, the nineteen ancient eclipses recorded by Ptolemy in the Almagest, found by a comparison of the results with the recorded observations, that the correction of the same element should be $+ 1'.643$. The close agreement of these two values will appear very remarkable, when the complexity of the moon's motion is taken into account. Professor Hansen states that they both agree as nearly as could be desired, with the corresponding value deduced by him from the theory of gravitation. The result of these different researches completely refutes the statement recently put forth by Prof. Seiffarth of Leipsic, to the effect that the ancient eclipses indicated the necessity of a considerable correction of the motion of the lunar

the results of two mural circles. During Mr. Airy's directorship of the Observatory of Cambridge, he devised a mode of accomplishing the same object by the use of only one circle. This practice was adopted by him at Greenwich, soon after his appointment as Astronomer Royal; and, as only one mural circle was henceforward necessary for that establishment, the mural circle by Jones was dismounted in the year 1839, and was sent to the Cape of Good Hope, for the use of the Royal Observatory established in that colony.

In practical astronomy, as in every other department of human knowledge, circumstances are constantly arising which create the necessity for further improvement. The meridional instruments constructed by Troughton, for the Royal Observatory of Greenwich, were unrivalled specimens of artistic excellence, and in the hands of Pond and his successor, served amply to uphold the reputation of that establishment. In recent times, however, it has been found that they are no longer adequate to the exigencies of the present state of astronomical science. The difficulty which has been experienced in observing on the meridian the small planets that have been recently discovered, and also occasionally faint stars, whose positions it is necessary to determine in connexion with extra-meridional observations, has suggested the expediency of replacing the telescope of the mural circle by one of larger aperture*. The mural circle, however, not being adapted for carrying a large telescope, it became necessary to devise a different mode of construction; and as the form of the transit seemed to be most suitable for this purpose, it was proposed that an instrument should be constructed, which should combine the functions of both the mural circle and the transit. The advantage of an instrument capable of assigning *with due precision* the two elements of the position of a celestial body, must be obvious to every reader. The transit circle devised by Mr. Airy, with a view to the accomplishment of this object, was constructed by Messrs. Ransome and May, of Ipswich, and Mr. Simms, of London. The circle is 6 feet in diameter. The telescope has an aperture of 8 inches, and a focal length of 11½ feet. The adjustment in azimuth and collimation is effected by means of two collimating telescopes, placed one to the north and the other to the south of the instrument†.

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* The telescope of Troughton's mural circle had an aperture of 4 inches, and a magnifying power of 150.

† It has been already mentioned (see p. 464) that Roemer determined the error of collimation of his transit circle by means of two distinct marks diametrically opposite to each other. In fixing two objects at the exact distance of 180° apart, he employed a telescope of a peculiar construction, termed the *amphioptra*, or reciprocal tube, which he contrived for that express purpose (*Basis Astronomiæ*, p. 97). It is necessary, when a fixed object is employed directly as a meridian mark, that it be situate at a considerable distance from the telescope, so that the rays of light from it may enter the object glass in sensibly parallel directions, otherwise they will not converge at the focus. The intervention of a lens, however, serves to render a near mark equally suitable for this purpose; for since parallel rays of light falling upon the lens converge at its focus, so on the other hand, if an object be placed at the focus of the lens, the rays of light diverging from it will be refracted by the lens, and, emerging in parallel directions, will produce the same effect as if they came from an object at an infinite distance. Hence, if an object be viewed with a common astronomical telescope, through a lens adjusted to it in this manner, it will appear quite distinct in the focus of the telescope, at whatever distance it be situate from the observer. This mode of obtaining the advantage of a distant mark, was originally devised in 1785, by Dr. Rittenhouse, an American philosopher (*Trans. Amer. Phil. Soc.*, vol. ii., p. 181). The collimating telescope is an application of the same principle. It consists of a small telescope, having its object glass turned towards the object glass of the telescope to be adjusted. In this case it is clear that the cross wires

The observations are made by direct and reflected vision, in the same way as formerly practised with the mural circle. The instrument has been in constant use since the commencement of the year 1851*.

Although meridional determinations form the principal object of attention at Greenwich, observations of a miscellaneous nature are not altogether neglected. In one of the domes of the more ancient part of the building, there is fitted up an equatorial of 5 feet in diameter, which was originally constructed by Ramsden for Sir George Shuckburgh, and which in the year 1811, was presented to the Royal Observatory by the Hon. Mr. Jenkinson. This instrument is occasionally used in observing comets. It has recently been employed in determining the distances between Mars and the neighbouring stars, in connexion with the corresponding observations of the American expedition to Chili†. In the south-eastern dome of the modern part of the observatory, a powerful telescope, mounted equatorially, has been in constant use since the year 1838. The object glass, which is by Cauchoix, of Paris, is the munificent gift to the observatory of the Rev. R. Sheepshanks. It has an aperture of 6.7 inches, and a focal length of 8 feet 2 inches. This telescope is fitted with a double-image micrometer, contrived by Mr. Airy. It has been employed in measuring the diameters of the planets, and in determining the distances and angles of position of double stars. An interesting result obtained by the use of this telescope, is the recent determination of the true figure of the planet Saturn, by the Rev. Mr. Main, the chief assistant at the Royal Observatory,

Besides the equatorials above referred to, there are also detached telescopes for the observation of eclipses, occultations, and other phenomena of a similar kind. Among the instruments of this description is a reflecting telescope of 10 feet, constructed by Sir William Herschel.

In Pond's time an instrument termed the zenith tube was constructed by Troughton for the observatory, the object of which was to determine small distances from the zenith. It consisted of a telescope 25 feet long, adjusted to the zenith by the aid of a plumb-line. The small variations in the zenith distance of a star were ascertained by measuring with an internal micrometer, the deviations of the star from the optical axis of the telescope. In practice the use of this instrument was limited to observations of γ *Draconis*, which passes within about 2′ of the zenith of Greenwich.

The zenith tube was first erected in 1833, and strong hopes were entertained that observations with it would lead to a more accurate determination of the constant of aberration, and might also serve to throw light upon several other interesting points of astronomy. These hopes, how-

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in the focus of the former telescope will form a distinct image at the focus of the latter. Hence, if the collimating telescope have a fixed position in the meridian, the transit may be adjusted to it by bringing its cross wires to coincide with the cross wires of the collimator. The principle of the collimating telescope has been applied to various astronomical purposes in modern times by Gauss, Bessel, Kater, and others. We have already had occasion to mention a singular mode of using it, by Dr. Robinson, in his researches on Irradiation (see p. 353).

* One of the peculiarities of this instrument, consists in its being made of as few separate pieces as possible, so as to obviate the effects of unequal expansion, and to secure more effectually the adjustment of the different parts. For this purpose the material of construction used is cast iron, the pivots of the axis being hardened by the process which engineers term *chilling*.

† See at p. 213 a brief allusion to the object of this expedition.

ever, have not been realised. Notwithstanding a great amount of attention and skill bestowed upon it for many years, the results have not been found to exceed in accuracy those obtained by the aid of the mural circle. The Astronomer Royal, in consequence, has recently contrived a new instrument, termed the reflex zenith telescope, which appears to be adapted for giving results of great accuracy. The adjustment of this ingenious instrument, with respect to the zenith, is effected not by means of the plumbline, which Mr. Airy considers to have been the main cause of the failure of the former instrument, but by reflexion from a surface of mercury. This instrument is now being constructed, and will soon be ready for use.

The volume which issues annually from the Greenwich Observatory, exhibits an interesting picture of the activity which reigns in that establishment. The sun, moon, and planets, and the stars of the Nautical Almanac, are observed as they pass the meridian on every practicable occasion. A considerable number of other stars is also observed on the meridian for particular purposes. Then there are the extra-meridional observations with the equatorials, comprehending occasional observations of comets, determinations of the diameters of the planets, and micrometrical measurements of double stars; besides observations from time to time of eclipses, occultations, and other such phenomena, with detached telescopes. The observations of the planets for the year are all reduced, and a comparison is instituted between the results thus obtained, and the corresponding results calculated from the most esteemed tables of the several bodies. Not only are the errors in the geocentric longitude and ecliptic polar distance of each planet thus regularly tabulated, but the equations between the errors in geocentric longitude and the *heliocentric* errors of the earth and planet are also regularly formed. The observations, in fact, are so exhibited as to be immediately available for correcting the elements of the planets, or otherwise perfecting the theory of their movements*. The stars observed during each year are also reduced to their mean places, forming the materials for occasional catalogues, which prove of inestimable value, both as a guide to the practical astronomer and as a groundwork of research to the theorist. The same system of complete reduction is applied to all the other observations. In the case of the occultations of stars by the moon, the results in every instance are employed in forming an equation for the rectification of the lunar theory. So with respect to the occultations and transits of Jupiter's satellites, the times of actual occurrence are compared with the times indicated by the Nautical Almanac, and the errors of the ephemeris, or rather of the tables of the satellites, thereby clearly exhibited to the eye.

It will be obvious to any person who bestows even a passing glance upon the annual volume of the *Greenwich Observations*, that the office of

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* The error in the geocentric longitude of a planet may arise from either of the following causes:—1st, an error in the earth's longitude; 2nd, an error in the radius vector of the earth's orbit; 3rd, an error in the heliocentric longitude of the planet; 4th, an error in the radius vector of the planet's orbit. Hence an equation is formed between each error of geocentric longitude and the four errors above mentioned, represented algebraically as unknown quantities. In the annual volume of the *Greenwich Observations*, the numerical values of the coefficients of the different terms in each equation are regularly calculated, but this is the farthest step that is practicable. The discussion of the equations can only be undertaken, with any hopes of success, when they have accumulated in large numbers, and when the observations to which they relate have extended over a

Astronomer Royal is no sinecure. One forms, however, a very imperfect idea of the heavy responsibility attached to the directorship of a national observatory, from a mere inspection of the current labours of the establishment. The rapid progress of astronomy in modern times, is constantly suggesting the necessity of modifying in some degree the instruments and methods of observation in actual use, and it is therefore desirable that the individual appointed to such an office should possess a natural aptitude for encountering every fresh difficulty that may occur in the practice of observation, while at the same time he should maintain a vigilant eye over the labours of his contemporaries, so as to avail himself of every really useful improvement that their researches may elicit. It is hardly necessary to remark that sound judgment and strong natural sagacity, combined with a profound knowledge of the various branches of physical science bearing upon practical astronomy, as well as indefatigable energy, are indispensable to the due discharge of such duties. It is universally admitted that the present Astronomer Royal has proved himself to be eminently qualified for his arduous office, and that he has upheld in the most satisfactory manner the ancient reputation of the Observatory of Greenwich. Moreover, while he has been actively engaged in the promotion of objects more immediately relating to practical astronomy, he has not failed to contribute to the advancement of astronomical science in general, by researches of a purely theoretical character. It ought to be remarked, also, that in virtue of his office he is frequently called upon by the Government to take an active part in scientific operations, which have only a remote connexion with astronomy, but which necessarily demand much careful attention. In all these labours, as well as in the multifarious details connected with the ordinary discharge of his duties, Mr. Airy has uniformly displayed the same consummate ability, the same untiring energy, and the same devotedness to the trust committed to his charge.

A retrospective view of the history of the Royal Observatory of Greenwich is calculated to excite feelings of agreeable satisfaction with respect to the mode in which that noble institution has fulfilled the end for which it was originally designed. From the circumstance of the observations extending with uninterrupted regularity over a long period of time, and from their undisputed excellence, they have formed almost the exclusive materials by means of which astronomers in modern times have succeeded in bringing the tables of the sun, moon, and planets to their present high state of perfection. The lunar theory, which so intimately concerns the interests of navigation, has been especially indebted for its successive improvements to the observations made at Greenwich. It is also mainly by a discussion of Greenwich observations that the constants of precession, and the other uranographical corrections, have been established with such precision in the present day. Delambre has justly remarked, that if by some great revolution the sciences had perished, while this collection of observations alone, with some methods of calculation, had survived the general wreck, there would still remain sufficient materials for reconstructing the whole edifice of astronomical science *. In the present day, when the attention of astronomers is being directed to questions of greater delicacy than any which had hitherto formed the subject of research, the materials annually accumulated at Greenwich have acquired new claims to the consideration of the theorist. It is thus that the recent determinations of the places of the stars at that observatory, have served to confirm beyond all doubt the interesting conclusion arrived at by various astronomers in modern times relative to the motion of the solar system in space *.

In the department of practical astronomy, which relates to the determination of the figure of the earth and the promotion of objects connected with geographical science, England, although not the first in the field, has in recent times assumed a distinguished position. The geodetical operations which have been carried on in the present century, both in the British Isles and in India, may fairly challenge comparison with the most unexceptionable undertakings of a similar kind that have been executed on the Continent. The Indian arc of the meridian is, moreover, especially remarkable for being the longest that has yet been measured upon the surface of the earth. With respect to those operations which have for their object the determination of the terrestrial ellipticity by means of experiments with the pendulum, it may be asserted with confidence, that no country has achieved so much as Britain has done. The experiments of the lamented Captain Foster, for completeness and delicacy of execution, may be said to leave nothing further to be desired.

It is impossible, within the limits of this work, to give an account of the numerous establishments that have been founded, in different parts of the British empire, for the promotion of practical astronomy. To the influence of the Astronomical Society is attributable in a great degree the activity which thus generally prevails. This society was founded in the year 1820, and was finally incorporated by Royal Charter in 1830. The first President of the Society was Sir William Herschel.

In France, practical astronomy continued to make slow progress during the whole of the eighteenth century; a circumstance mainly attributable to the imperfect organization of the Royal Observatory of Paris. Some brilliant exceptions, indeed, must be made to this remark. We have seen Lacaille, in spite of unfavourable circumstances, raise himself, by the mere force of his talents, to a high position among contemporary astronomers. It has been remarked, also, that France took the initiative in those important operations which relate to the determination of the magnitude and figure of the earth. In this department of practical astronomy, she still continued to maintain her pre-eminence. The measurement of the arc of the meridian comprehended between Dunkirk and Barcelona, was one of the most splendid scientific operations of the eighteenth century †. This magnificent undertaking was executed under the joint superintendence of Delambre and Méchain. Delambre, more especially, by his labours on this occasion, proved himself worthy to be associated with Picard, Bouguer, and Lacaille. The main peculiarity of the operation consisted in the observations having been made exclusively with the repeating circle. It has been already mentioned that Mayer first sug-

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* In recent years attention has been devoted at the Greenwich Observatory to the phenomena of Meteorology and Magnetism, and the observations relative to these departments of physical science are now published with the same regularity as the astronomical observations.

† It may be remarked that the object of this operation was, not to determine the figure of the earth, but to obtain an invariable standard of measurement, by adopting as the fundamental linear unit, the ten millionth part of a degree of the meridian. MM. Arago and Biot subsequently extended the arc beyond Barcelona to Formentera, one of the Balearic Isles.

gested the principle of repetition as a means of obviating the errors of division in astronomical instruments. The idea of that astronomer was not, however, realised till the year 1787, when it was at length applied successfully by Borda, a French officer of Marine, distinguished by philosophical acumen and strong practical talents.

In the construction of tables of the various planetary bodies, an object of research which may be said to form the connecting link between theoretical and practical astronomy, the labours of Delambre would suffice to assign to him a high place in the history of astronomy, even although he possessed no other claims to the recollection of posterity. The method of *equations of condition*, the utility of which Mayer originally exhibited in his admirable researches on the libration of the moon, was employed with great success by Delambre, in the year 1785, in correcting the elements of the solar orbit by means of Maskelyne's observations. His tables of the planets Jupiter, Saturn, and Uranus, and of the satellites of Jupiter, which shortly afterwards appeared, as well as his solar tables, which were published at a later period, were all admirable attestations of his perseverance and skill. He may be said to have been the first who submitted masses of observations to a systematic process of treatment, deducing in this manner, from their totality, an accuracy of result which was unattainable by a succession of partial operations*.

About the commencement of the present century, an improvement was effected in the organization of the Royal Observatory of Paris. At the same time, two excellent mural quadrants, one of which was constructed by the celebrated Bird, were erected in suitable apartments. Henceforward the observations assumed a character of regularity and trustworthiness hitherto unknown, and were destined, in consequence, to prove really serviceable in conducing towards the advancement of theoretical astronomy. In recent times, when the duties of its directorship have been confided to M. Arago, the Royal Observatory of Paris has attained a high degree of efficiency. Observations are now constantly made at that establishment, both by direct and reflected vision, with two mural circles constructed by French artists of acknowledged eminence†. The brilliant result which M. Le Verrier deduced from his researches on the perturbations of Uranus, based to a considerable extent upon data procured from the registers of the Royal Observatory of Paris, was a graceful homage to the illustrious philosopher, at whose suggestion he was induced to undertake an examination of the subject.

In no country has practical astronomy made more rapid progress during the present century than in Germany. The labours of Bessel and a host of eminent astronomers, afford ample confirmation of the truth of this remark. It must be allowed, also, that in the construction of astronomical instruments, the German artists have attained a degree of excellence which is unsurpassed in any other country of the world.

Denmark still continues to sustain her ancient reputation by her patronage of astronomical science. In M. Schumacher, whose recent death has been so deeply deplored by the astronomical world, practical astronomy, more especially, has lost a supporter whose place it will be difficult to supply. The *Astronomische Nachrichten*, with which the name of that astronomer is inseparably associated, has, perhaps, contributed more towards the improvement of astronomical science, in all its branches, than any similar publication has ever done. This celebrated periodical was established by M. Schumacher at Altona, in the year 1821, and has ever since formed the common medium of intercourse among astronomers over the whole civilised world. Its publication continued to be regularly superintended by him till his death *.

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* Jean Baptiste Joseph Delambre was born at Amiens, in the year 1749. He died at Paris in 1822.

† One of the mural circles was executed by Gambey, an artist who appears to have attained a very high degree of excellence in the art of graduation. According to M. Faye, the probable error of the interval between two consecutive points of division of the circle constructed by him for the Royal Observatory of Paris, is less than 0″·108

While England, France, and Germany have been contributing towards the advancement of practical astronomy, Italy has not remained an idle spectator of the scene. There is assuredly no cause of more cheering congratulation, to those who look forward to the regeneration of that noble country, than is afforded by the circumstance that, while trodden in every age under the hoof of the oppressor, she has never, even in the darkest hour of her distress, resigned herself to despair, but has invariably maintained her high position in the intellectual world, with a constancy of purpose and a brilliancy of success worthy of her deathless renown. While unrivalled in the productions of the imagination, Italy has been always ready to dispute the palm with the most favoured nations in the more severe pursuits relating to the mathematical and physical sciences. In Oriani and Piazzi, as well as many other eminent individuals of modern times, she has afforded abundant evidence that practical astronomers of the first order are not wanting to sustain her reputation.

The efforts which Russia has made in recent times to acquire a position in the civilised world conformable to the grandeur of her material resources, have in no instance been more signally illustrated than in her munificent patronage of astronomical science. The earliest observatory of that country was built at St. Petersburg, in 1725, under the auspices of Peter the Great. Delisle, a French astronomer of considerable experience, was its first director. As in the case of most of the early observatories of Europe, it was built in the form of a high tower, the observations having been made in an apartment at the top of the building. In 1747, this structure was totally consumed by fire, whereupon Delisle returned to France. Shortly afterwards it was rebuilt in its original style, and Heinsius of Leipsic, an astronomer well known for his observations of the great comet of 1747, was appointed director. The instruments with which the observatory had been hitherto furnished were only of moderate excellence, and consequently the observations could not be expected to possess a high degree of importance. In 1761, however, the Russian Government, with a view to extend the usefulness of the observatory, procured from England a mural quadrant of eight feet radius, by Bird, who was then in the zenith of his fame, and also a transit instrument of five feet focal length, constructed by the same artist. Grischow, who was then director of the establishment, justly considered that the practice of making the observations in an apartment at the top of the building was unfavourable to the stability of the instruments; and, moreover, that the situation of the observatory in the heart of a populous city was not by any means desirable. On both these grounds, he suggested that a new observatory should be built in a more suitable locality, and as this idea was for some time favourably entertained by the Academy of Sciences, no immediate steps were taken for mounting the instruments obtained from England. It was not till the year 1798, when the design of erecting a new observatory was temporarily abandoned, that the instruments constructed by Bird were at length adjusted in the meridian in two lateral apartments, situate upon the first floor of the observatory.

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* Heinrich Christian Schumacher was born at Bramstedt, in Holstein, on the 3rd of September, 1780. He died at Altona, on the 28th of December, 1850. The *Astrono-*

Although Russia had not hitherto contributed greatly to the advancement of astronomical science in so far as meridional observations were concerned, it is just to state that, in regard to the important observations connected with the transits of Venus in 1761 and 1769, the Government, as well as the astronomers of that country, took a very distinguished part. Another important object connected with the practice of observation, which exercised the talents of the Russian astronomers towards the close of the last century and the beginning of the present, was the determination of the geographical positions of the principal points comprehended within the vast territories of the empire. The most active in the prosecution of these labours was the well known astronomer Schubert. In 1803, M. Wisniewski*, an astronomer of great merit, was appointed director of the Observatory of St. Petersburg. Although some excellent observations in the meridian were now occasionally made at that establishment †, it was deemed advisable to apply its resources chiefly to observations of comets and other isolated phenomena. In 1827, the idea of a new central observatory began again to be entertained by the Academy, and in 1830 the Emperor at length declared by his minister, *that the honour of the country appeared to him to demand the establishment, near the capital, of a new astronomical observatory, conformable to the actual state of science, and capable of contributing to its ulterior advancement.*

The first thing to be done was to fix upon the most appropriate situation for the new observatory. After some deliberation it was at length resolved, at the suggestion of the Emperor, that it should be erected in the vicinity of Pulkowa, a small town situate about ten miles to the south-west of St. Petersburg, upon a gentle eminence commanding an extensive view of the horizon in all directions. The design of the establishment was upon a scale of unprecedented magnificence. The foundation stone of the building was laid on the 21st of June, 1835, and it was finally completed on the 19th of August, 1839.

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* This astronomer seems to have been gifted with a remarkable power of vision. According to M. Struve, he continued to observe the comet of 1807, in the month of March, when all other astronomers had lost sight of it for four weeks. In the case of the great comet of 1811 he gave a still more striking proof of his acuteness of vision. This comet first became visible in the month of March, 1811, and after reappearing on its return from the perihelion, was finally lost sight of on the 11th of January, 1812. Bessel, however, suggested that the earth might not improbably overtake the comet, and that the latter might in consequence become visible for the third time, in the month of July or August. Wisniewski actually detected it at Novo-Tcherkask, on the 31st of July, and kept sight of it till the 17th of August. No other European astronomer appears to have perceived the comet on this occasion; but it is worthy of remark that it was seen by the Spanish astronomer Ferrer, at Havannah, in the Island of Cuba (*Mem. Ast. Soc.*, vol. iii., p. 36). It may be mentioned that Ferrer and his companion determined the orbit of this celebrated comet with great accuracy from their own observations, made with a sextant of only seven inches radius, before Bessel or any of the great astronomers of Europe had published any results on the subject.

† M. Struve states that a series of observations of the newly discovered planets, Ceres and Juno, was made at the St. Petersburg Observatory about this time, which in point of accuracy might bear comparison with those made at any other observatory in Europ

The task of providing the instruments for the new establishment was confided to M. Struve, the director of the Observatory of Dorpat, so celebrated for his labours on various subjects relating to astronomical science, and more especially for his observations of double stars. The imperial instructions relative to this object were characterised by unbounded liberality. The principal instruments of the establishment have been constructed by German artists. For meridional observations there are four instruments, two of which have been supplied by Ertel of Munich, and two by Repsold of Hamburgh. The instruments by Ertel are a vertical circle 43 inches in diameter, and a transit instrument of 5.85 inches aperture, and 8 feet 6 inches focal length: those by Repsold are a meridian circle 48 inches in diameter, and a prime vertical telescope of 6.25 inches aperture and 91 inches focal length. For extra-meridional observations, the observatory is furnished with a magnificent equatorial telescope by Merz and Mahler of Munich, the object glass of which has a free aperture of 14.93 inches, and a focal length of 22.55 feet. Moreover, it contains several clocks constructed by the most esteemed artists of Germany and England, as well as an ample collection of other instruments, both astronomical and physical, destined for various purposes connected with the practice of observation. In short, the Observatory of Pulkowa may be regarded as one of the most complete in existence, of those institutions that have been founded for promoting the advancement of astronomical science. The expense attending the erection of the building, and its subsequent equipment, amounted in round numbers to 600,000 roubles of silver*.

The illustrious Struve has been appointed first director of the Observatory of Pulkowa. An ample staff of assistants, several of whom enjoy a European reputation, is allowed him for carrying on effectually the labours of the establishment. The sum allotted from the imperial treasury for its annual maintenance is no less than 62,200 roubles†.

M. Struve commenced his labours at the Observatory of Pulkowa in the month of September, 1839. The main object to which he has proposed to devote the resources of the establishment, is the advancement of sidereal astronomy. Results of great importance relative to precession, aberration, and other kindred subjects, have already been deduced from the observations‡.

In the United States of America, practical astronomy has recently been making rapid progress. A central observatory has been established at Washington, which is placed under the direction of Lieut. Maury. The expedition to Chili, undertaken with a view to determine the solar parallax by means of simultaneous observations of Venus and Mars, bears honourable testimony to the enlightened views and liberality of the American Government. Besides the national observatory above alluded to, there are similar institutions, upon a smaller scale, established in several of the states of the Union. Of these, the most celebrated is the observatory of Harvard University, Cambridge, Massachusetts, which boasts of one of the largest and most perfect refracting telescopes in the world. The discovery of the eighth satellite of Saturn, and the establishment of various facts of a highly interesting nature, relative to the physical constitution

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* £100,000.

† £10,366 13*s.* 4*d.*

‡ M. Struve has given a complete description of the Observatory of Pulkowa in a magnificent work entitled, "Description de l'Observatoire Astronomique Central de Pulkowa, par F. G. W. Struve," 2 vols. fol., St. Petersburg, 1845.

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of the celestial bodies, have resulted from the labours of Mr. Bond, the director of the observatory, achieved by the use of this powerful instrument.

One of the inventions which do most honour to the Americans, consists in the application of electro-magnetism to geographical and astronomical purposes. As early as the year 1844, the instantaneous transmission of time by the electric telegraph, was employed in determining the difference of the longitudes of Washington and Baltimore. The same method has since been practised for ascertaining the relative longitudes of various other transatlantic cities. Recently, attempts have been made, with complete success, to record transit observations of the celestial bodies, by means of the electro-magnetic principle. As soon as the star is seen bisected by the wire of the telescope, a slight pressure of the finger completes or breaks the galvanic circuit, and the effect is instantaneously transmitted, by the intervention of a magnet, upon a recording surface, to which a uniform motion is given by a peculiar mechanism. This ingenious contrivance for fixing the precise instant of a phenomenon, by calling into exercise the sense of touch to aid that of vision, promises, at no distant period, to supplant the usual mode of recording by the combined application of the eye and the ear *. We may remark, in concluding this chapter, that it ought to be borne in mind also, as an earnest of what may be expected from the future exertions of our transatlantic brethren, that to an American philosopher Practical Astronomy is indebted for the beautiful invention of the collimating telescope.

CHAPTER XIX.

Catalogues of the Fixed Stars.—Their importance as forming the Groundwork of Astronomical Science.—Earlier Catalogues.—Ptolemy.—Ulugh Beigh.—Tycho Brahé.—Halley.—Hevelius.—Flamsteed.—Modern Catalogues.—Bradley.—Lacaille.—Mayer.—Maskelyne.—Publication of the Histoire Céleste of Lalande.—Piazzi.—Groombridge.—Zone Catalogues of Stars.—Bessel.—Argelander Santini.—Catalogue of the Astronomical Society.—Catalogues of Southern Stars.—Fallows.—Brisbane.—Johnson.—Henderson.—Standard Catalogues of Stars.—Catalogue of the British Association.—Recent Standard Catalogues.

CATALOGUES of the fixed stars, considered as contributions to Astronomical Science, possess an importance arising from two distinct causes. In the first place, an accurate catalogue of such objects furnishes a series of points of reference by means of which the positions of the various bodies composing the planetary system may be determined from observation, and the laws of their motions investigated. Secondly, it constitutes the groundwork of stellar astronomy. A comparison of catalogues constructed at different dates, enables the astronomer to ascertain the proper motions of the stars, and to arrive at conclusions respecting the changes which may be taking place in the physical constitution of the sidereal heavens. More-

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* It would appear that several individuals in the United States are entitled to share in the merit of this ingenious invention, among whom may be more especially mentioned Mr. Bond, Mr. Sears Walker, Prof. Mitchell, and Dr. Locke. It has been stated in the text, that the instant of transit may be noted, either by *completing* the galvanic circuit, supposing that in the ordinary state of the apparatus the current of electricity is interrupted, or by making the electricity to flow in a continual current, and then suddenly *breaking* the circuit. Both of these methods have been practised in America. At the

over, it is by such a comparison alone, that he can discover whether there exist any real grounds for believing that the whole solar system is affected with a motion of translation in space.

The first individual who constructed a catalogue of the stars was Hipparchus, who may be considered as the true founder of Astronomical Science. It is said by Pliny, that he was induced to enter upon this undertaking, by the sudden apparition of a new star in his time. Be this as it may, it is certain that he actually determined by observation, the longitudes and latitudes of upwards of a thousand stars, and arranged the results in a catalogue. The earliest production of this kind now extant, is the catalogue inserted by Ptolemy in the *Syntaxis*. It contains the longitudes and latitudes of 1028 stars, arranged in 48 constellations. The epoch is the first year of the reign of the emperor Antoninus, which corresponds to the year 138 A.D. It has been suspected, on very probable grounds, that this catalogue is nothing more nor less than the catalogue of Hipparchus, transported to the time of its reputed author, by applying to the longitudes of all the stars a common additive quantity for the supposed effect of precession during the intermediate period.

The next catalogue of stars, in the order of time, is that of the Tartar prince, Ulugh Beigh. This individual, who was a grandson of the renowned Tamerlane, established an observatory at Samarcand, the capital of his father's dominions, and devoted himself with great ardour to astronomical pursuits. Having found that the positions of the stars, as assigned by Ptolemy in his catalogue, were in many instances considerably erroneous, he formed the resolution of constructing a new catalogue, founded exclusively upon his own observations. This design was actually realised by him. The number of stars in the catalogue of this prince is 1019, being only nine less than the number contained in Ptolemy's catalogue. The epoch is the year 1437 A.D. The mode of arrangement is the same as that employed by Ptolemy. Ulugh Beigh was treacherously slain by his son in the year 1449, shortly after his accession to the throne of his father.

The next catalogue recorded in the annals of astronomy, is that of the illustrious Tycho Brahé. It originally appeared in the work of that astronomer, entitled *Astronomiæ Instauratæ Progymnasmata*, which was first published in the year 1602. The number of stars is 777. The epoch is 1600 A.D. From the labour and skill employed by Tycho Brahé in its construction, this catalogue was vastly more accurate than any other that had been hitherto executed. Kepler subsequently enlarged it from Tycho Brahé's observations to 1005 stars, and published it in the year 1627, at the end of the Rudolphine Tables.

Halley's Catalogue of Southern Stars was the next in succession after

a contrivance for recording transits by electro-magnetism, which had been used for some time at the Cambridge Observatory, U. S. In this apparatus, the signal was made by interrupting the galvanic circuit, the recording paper upon which the magnet acted being applied closely upon a cylinder, to which a uniform motion was given by a mechanism devised at Cambridge, termed the spring governor. Mr. Airy proposes to introduce the mode of recording transits by electro-magnetism at the Royal Observatory, where the necessity of transmitting time from the meridian transit clock to the altitude and azimuth instrument (the observations with which are made by noting the transit of the object over a system of horizontal and vertical wires) renders such a method peculiarly desirable. It would seem that the Astronomer Royal contemplates applying the principle so that the instant of transit shall be indicated by *completing* the galvanic circuit, and that he purposes giving a uniform motion to the recording cylindrical surface by means of a centrifugal

Tycho Brahé's. The observations which form the basis of this catalogue were made at St. Helena, during the years 1676-7-8. The number of stars is 341. The epoch is 1677. Besides the circumstance of its relating exclusively to stars in the southern hemisphere, this catalogue is further remarkable for being the first that was constructed from observations made by the use of telescopic sights. Although not so accurate as was desirable, even for the astronomy of his time, Halley did not on any subsequent occasion attempt its improvement. It was submitted, however, to a careful revisal by Abraham Sharp, and was inserted in its amended form in the third volume of the *Historia Celestis* of Flamsteed.

The catalogue of Hevelius appeared in a posthumous work published in 1690*. It contained 1564 stars. The epoch is 1660. In consequence of the pertinacity with which its author adhered to the use of simple pinnules in making observations of the celestial bodies, this catalogue has not been attended with so much advantage to astronomical science as might have been expected from the labour bestowed upon its construction.

All the above-mentioned catalogues, after being subjected to a careful revisal by the late Mr. Baily, were then reprinted; and, in their improved state, they now form the thirteenth volume of the "Memoirs of the Astronomical Society."

It has been already mentioned that Flamsteed's "British Catalogue" was published in 1725, in the third volume of the "*Historia Celestis.*" Mr. Baily executed a scrupulous revisal of this catalogue also, adding to it several hundred stars, which he extracted from Flamsteed's original observations. In this form it was republished by him in 1835, at the end of his "Account of the Life and Correspondence of Flamsteed," a work to which we have repeatedly had occasion to allude.

No catalogue of stars was constructed from Bradley's observations during the lifetime of that great astronomer. In the *Nautical Almanac* for 1773, there finally appeared a catalogue of 389 principal stars, extracted by Mason from the records of the observations made by him at Greenwich. This catalogue was subsequently inserted by Dr. Hornsby in the first volume of Bradley's Observations, which was published at Oxford in 1798. Allusion has been made to Bessel's *Fundamenta Astronomiæ*, which he published in 1818, containing a catalogue of stars constructed from the totality of the observations made by Bradley between the years 1750 and 1762. The number of stars in this catalogue is 3112. The epoch is January 1, 1750.

The illustrious Lacaille is the author of three catalogues of stars. The first of these was published in 1757, in his *Fundamenta Astronomiæ*. It contains the places of 398 stars. The epoch is January 1, 1750. The place of the equinox was determined by Flamsteed's method, from observations of *α Lyræ* and *Sirius*, in connexion with corresponding observations of the sun. This may be considered as the first catalogue which, in point of accuracy, can bear a comparison with those of modern construction. In consequence of its having become exceedingly rare, the late Francis Baily undertook a careful revisal of it, and procured its insertion in the fifth volume of the "Memoirs of the Astronomical Society."

The second catalogue of Lacaille's is founded on observations of southern stars made by him during his residence at the Cape of Good Hope. Of these observations, comprehending the places of nearly ten thousand stars, a partial catalogue only was formed by the author, containing the reduced places of 1942 stars*. The remaining mass continued in their original condition, until at length, in 1838, the British Association, at the suggestion of Mr. Baily, undertook the reduction of the whole of the observations according to a uniform system. The computations were executed under the superintendence of the late Professor Henderson, of Edinburgh. The number of stars is 9766. The epoch is January 1, 1750. The catalogue was printed in 1845, at the expense of the Government.

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* Prodromus Astronomiæ, Gedan, 1690.

Lacaille's third catalogue is one of 515 zodiacal stars, which was published in 1763 as a posthumous work. He contemplated that this catalogue should contain the places of 800 stars; but he was prevented by death from realising his design. The observations upon which it is based were reduced, shortly after his death, by his personal friend Baily; who does not seem, however, to have executed his task with the scrupulous regard to accuracy which a work of so much importance demanded. The epoch of this catalogue is the year 1765.

Mayer is the author of a catalogue of 998 zodiacal stars, which appeared in a volume of posthumous works, published at Gottingen, in 1775. Through the influence of Francis Baily, the original observations were brought from Gottingen to this country, and were published in 1826 at the expense of the Board of Longitude. The same individual having carefully revised the catalogue, by collating it with the original observations, procured its insertion in the fourth volume of the "Memoirs of the Astronomical Society."

Maskelyne's fundamental catalogue of 36 stars, published by him, originally in the *Greenwich Observations*, was by far the most accurate production of the kind that had been hitherto given to the world. This small collection long continued to be an indispensable guide to the practical astronomer.

Observations of circumpolar stars present a peculiar interest, on account of their utility in researches on refraction and various other subjects of astronomical science. In 1800 the Rev. Francis Wollaston published a small catalogue of such stars in a work entitled "Fasciculus Astronomicus."

Towards the close of the last century, a vast number of observations of stars were made at the *Ecole Militaire* of Paris, by D'Agelet and Michael Lefrançais Lalande, nephew of the celebrated astronomer of that name. The observations were made in zones. It was contemplated in this manner to determine the positions of all the stars in the northern hemisphere down to the ninth magnitude. They were published in 1801, in a volume entitled "Histoire Céleste Française," but they were unreduced, and therefore they were, in point of fact, inaccessible to the astronomer. Partial reductions of these observations were shortly afterwards made; but the great mass remained for many years in their original condition. In 1825, Schumacher published tables for facilitating the reduction of the stars contained in the *Histoire Céleste*, computed according to a plan suggested by Bessel. In 1837, the British Association agreed to defray the expense of the reduction of all the observations, at the suggestion of Francis Baily, who undertook the superintendence of the whole operation. The Government, as in the case of Lacaille's observations of southern stars, agreed to defray the expense of printing.

This great catalogue contains the places of 47,390 stars, reduced to the beginning of the year 1800. Although the results are not distinguished by a high degree of accuracy, they have proved eminently useful as points of reference in extra-meridional observations, and have served to throw light upon several interesting points of astronomical science.

In 1803 Piazzi published, at Palermo, his first catalogue of 6748 stars. The fundamental points used in its construction were the thirty-six stars of Maskelyne's catalogue. As some doubt seemed to rest upon the accuracy of the right ascensions of these stars, the illustrious Italian resolved to establish his results solely upon his own observations, and, in accordance with this view, he formed a preliminary standard catalogue of 120 principal stars, which he published in 1807. These formed the basis of his more extensive catalogue of 7646 stars, which appeared in 1814 *. This great work is justly considered to be one of the most important that has ever been executed by a single individual. Every star was observed several times, and a mean of all the results taken as the final place of the star †. Moreover, the table of refractions employed by Piazzi in reducing the stars to their mean places, was deduced by him exclusively from his own observations. The epoch of the catalogue is the beginning of the year 1800. From the circumstance of its being at once so extensive, so accurate, and so independent in its construction, this catalogue has formed the groundwork of much valuable research to the theorist, while at the same time it has proved an inestimable boon to the practical astronomer ‡.

In 1806 De Zach published a catalogue containing the places of 1830 zodiacal stars, founded on observations made by him at the observatory of Seeberg in Saxe Gotha.

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* "Præcipuarum Stellarum Inerrantium Positiones Mediæ, ineunte seculo xix". Quarto, Panormi, 1814. The observations employed in the formation of this catalogue were made by Piazzi, at the Observatory of Palermo, between the years 1792 and 1813, with the famous altitude and azimuth instrument constructed for him by Ramsden.

† According to Captain Smyth, each star was observed from five or six to ten or twenty, or even occasionally to a greater number of times. The same author states that the whole number of observations amounted to nearly 150,000. (*Cycle of Celestial Objects*, vol. i., p. 433.)

‡ Joseph Piazzi was born on the 16th of July, 1746, at Ponte in the Valteline, a district of Northern Italy, which then formed part of the Helvetic Confederation. In early life, he became a member of the monastic order of Theatines. As soon as he had qualified himself for holy orders, he was appointed to teach philosophy in a convent of Genoa, but having incurred the displeasure of the theological party by too free an expression of his opinions, he shortly afterwards resigned his situation. Having now devoted his attention more especially to the exact sciences, towards which he always evinced a strong inclination, he was appointed Professor of Mathematics at Malta, and subsequently at Rome. In 1780, he was appointed Professor of Mathematics in the Academy of Palermo. Shortly afterwards Ferdinand IV., King of Naples, having founded an observatory at that city, Piazzi was appointed director of the establishment. Having visited France and England, in order to extend his knowledge of practical astronomy, he returned to Palermo, bringing with him a collection of astronomical instruments from the latter country, and commenced his labours at the new observatory in 1792. He now proceeded to carry into effect his resolution of executing a great catalogue of stars, an undertaking which may be said to have formed the principal object of his life. Captain Smyth, who was a personal friend of the celebrated astronomer, referring to this great work, says of its author:—"I cannot forget his emphatic expression on putting a final correction to the last proof sheet in 1814. '*Now*,' said he, '*my astronomical day is closed.*'" (*Cycle of Celestial Objects*, vol. i., p. 433.) Piazzi died on the 22nd of July, 1826. By his will he bequeathed his library and instruments to the Observatory of Palermo, and an annuity to be employed in educating young men who showed an inclination towards astronomical pursuits.

A remarkable instance of devotion to this department of astronomical science is exhibited in the case of Stephen Groombridge. In 1806, this individual commenced observations of all the stars, down to the eighth magnitude, situate within 50° of the North pole, and finally completed his task in the year 1816. These observations were made with a transit circle constructed by Troughton. During the period included between the years 1816 and 1827, he was engaged in reducing his observations, but, before he succeeded in bringing his labours to a close, he was compelled by the weak state of his health to desist from his undertaking. The task of completing the reductions was executed at the expense of the Government, under the superintendence of Mr. Airy, and a catalogue embodying the results was finally published in 1838. It exhibits the mean places of 4243 circumpolar stars reduced to the year 1810. This is universally admitted to be one of the most valuable contributions to practical astronomy made during the nineteenth century.

The astronomical career of the illustrious Bessel was distinguished by a remarkable series of sidereal observations. In 1821 he commenced observations of all the stars, down to the ninth magnitude, comprehended between the parallels of 15° south declination and 45° north declination. These observations were made in zones with a meridian circle by Reichenbach, the right ascension and declination of each star being determined at a single observation. This great undertaking was completed in 1833. The number of observations amounted to about 75,000. By means of subsidiary tables appended to them, it was easy to perform their reduction in any particular instance; but still it was desirable that they should be exhibited in a completely reduced form. This task was executed by Professor Weisse, of Cracow, for all the stars situate within the region extending 15° on each side of the equator, and a catalogue of the results was published in 1846, at the expense of the Academy of Sciences of St. Petersburg. The number of stars in this valuable catalogue is 31,895, reduced to the year 1825. The preface to it is written by M. Struve. Professor Weisse is at present engaged in completing the reduction of the remaining zones of Bessel's observations. Argelander has followed up Bessel's undertaking by a similar observation in zones of all the stars included between 45° and 80° of north declination. These observations were commenced in 1841, and were finished in 1844. The number of stars is about 22,000, arranged in 204 zones. Tables are appended to facilitate their reduction.

It ought to be mentioned that, previous to the publication of Weisse's catalogue, a similar catalogue of stars, although less extensive, had been published by Sig. Santini, of Padua, founded upon his own observations. The number of stars in this catalogue is 1677, comprehended between the equator and 10° of north declination. The observations were made during the years 1838–39–40. The epoch is January 1, 1840. The object of this catalogue was to establish a series of reference points which might be useful in extra-meridional observations of new planets or comets. For this purpose the catalogue was so planned that, on each parallel of declination, some well-determined star should be found at every eight or ten minutes of time. The catalogue is inserted in the twelfth volume of the "Memoirs of the Astronomical Society."

In 1827 the Astronomical Society of London rendered an important service to practical astronomy, by the publication of a general catalogue of stars, founded upon the most celebrated catalogues that have been executed since the middle of the eighteenth century. This catalogue contains the places of 2881 stars, reduced upon a uniform system to the 1st of January, 1830. In addition to the mean place of each star, the constants for computing its apparent place, according to Bessel's method, are also inserted in the catalogue. The introductory explanation is by Francis Baily, to whom was assigned the important task of selecting the most trustworthy values of the uranographical corrections. The calculations were executed under the superintendence of Lieut. Stratford.

Hitherto the stars in the southern hemisphere had been almost entirely neglected by astronomers since the time of Lacaille. With the view of remedying this defect, the Government, in 1820, established an observatory at the Cape of Good Hope, appointing the Rev. F. Fallows to be its director. In the *Philosophical Transactions* for 1824 there is inserted a small catalogue by that astronomer, containing the mean places of nearly all the principal stars between the zenith of the Cape of Good Hope and the South Pole.

When Sir Thomas Brisbane was appointed Governor of New South Wales in 1821, he erected an observatory at Paramatta, which he furnished with excellent instruments. In the same noble spirit of disinterested liberality, he employed, at his own expense, two qualified assistants, Messrs. Rümker and Dunlop, to aid him in his astronomical labours. In 1832 M. Rümker published, at Hamburg, a small catalogue of stars observed in the southern hemisphere, designed as preliminary to one of greater extent. In 1835 Sir Thomas Brisbane published a catalogue of 7385 stars, chiefly in the southern hemisphere, founded also upon observations made at his establishment at Paramatta. M. Rümker is at present engaged in constructing from the same collection of observations a catalogue of 12,000 stars.

In 1830 an observatory was erected at St. Helena, and Lieut. Johnson, a talented officer who happened to be on duty on the island, was appointed its director. Two years only elapsed, when a stock of observations was amassed, which served for the formation of an admirable catalogue of 606 principal stars in the southern hemisphere. This catalogue was published in 1835, at the expense of the East India Company.

In 1844 Taylor's catalogue of 11,015 stars, founded on observations made at Madras during the years 1822–43, was published at the expense of the East India Company. This catalogue includes upwards of three thousand stars situate in the southern hemisphere, observed with especial reference to the same stars in Sir Thomas Brisbane's catalogue.

In the tenth volume of the "Memoirs of the Astronomical Society," there are inserted the mean declinations of 172 principal stars in the southern hemisphere, reduced by the late Professor Henderson of Edinburgh, from his own observations at the Cape of Good Hope. In vol. xv. he has given the mean right ascensions of the same stars. While on the subject of the southern stars, it may be mentioned, that the observations made by Fallows at the Cape of Good Hope, during the years 1829–30–31, have been recently reduced under the superintendence of Mr. Airy, and that from the results, a catalogue has been constructed, containing the mean places of 425 stars, situate chiefly in the southern hemisphere.

The only catalogues of stars distinguished by a high degree of accuracy which were accessible to astronomers in the year 1830, were those of Bradley and Piazzi. It was desirable, however, for purposes of research, as well as for facilitating the practice of observation, that a new catalogue of equal accuracy should be constructed from recent observations. In 1833, Pond, the Astronomer Royal, contributed towards supplying this desideratum by the publication of a catalogue containing the mean places of 1112 stars reduced to the beginning of the year 1830. In 1835, M. Argelander published a catalogue of 560 stars founded on observations made by him at Abo. The epoch is the same as that of Pond's catalogue. In the eleventh volume of the "Memoirs of the Astronomical Society," there is inserted a catalogue of 726 stars, formed by Mr. Airy from observations made by him at the Observatory of Cambridge during the interval included between the years 1828 and 1835. The epoch in this case also is January 1, 1830. In the *Greenwich Observations* for 1842, the same distinguished astronomer has published a catalogue of 1439 stars, founded on observations made at the Royal Observatory, between the years 1836 and 1841 inclusive. The epoch of reduction is January 1, 1840.

The publication of the catalogue of the Astronomical Society, in 1827, suggested the desirableness of re-observing the stars whose places were therein given, for the purpose of throwing light upon the question of their proper motions. In the tenth volume of the "Memoirs of the Astronomical Society" there is inserted a catalogue of the right ascensions of 1318 stars, determined by Mr. (now Lord) Wrottesley at a private observatory which he fitted up at Blackheath, the right ascension of each star being compared with the corresponding right ascension of the Astronomical Society's catalogue*. A similar catalogue of the right ascensions of 1248 stars has been recently formed by Lieut. Gilliss, of the United States Navy. It may be mentioned, that one of the objects proposed by the American expedition to Chili, of which that excellent astronomer has been appointed the superintendent, is the formation of a catalogue of all the stars down to the eighth magnitude, situate within 60° of the South Pole.

The great utility of the catalogue published by the Astronomical Society, suggested to astronomers the expediency of forming a more extensive catalogue upon the same plan, but accompanied with some improvements in the details. At the meeting of the British Association which was held at Liverpool in 1837, it was agreed to defray the expense of constructing such a catalogue. The superintendence of the operation was undertaken by Mr. Baily; but that individual, whose unparalleled exertions in this department of astronomical science will not fail to excite the gratitude and admiration of all future astronomers, had not the satisfaction to witness its completion, having died on the 30th of August, 1844, only a few months before the catalogue was ready for publication. The British Association appointed a committee consisting of Dr. Robinson, Lieut. Stratford, and Prof. Challis, to superintend the remaining part of the operation, and the catalogue was finally published in 1845. The preface is by Mr. Baily. The materials of its construction are derived from all the trustworthy catalogues that have been executed since the time of Bradley. The number of stars is 8377. The epoch of reduction is January 1, 1850. This catalogue is distinguished from the catalogue of the Astronomical Society in two important particulars. In the first place, the secular variation of precession, both in right ascension and declination, is given for each star. Secondly, the proper motions of the stars are assigned with as great a degree of nearness to the truth, as the existing state of sidereal astronomy allowed. This catalogue has proved of vast service to astronomers in every part of the civilised world.

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* Lord Wrottesley is at present engaged in prosecuting a series of observations with a view to the formation of a similar catalogue, which he purposes to compare with that of the British Association.

An important catalogue has recently issued from the Greenwich Observatory, founded on observations made during the twelve years commencing with 1836, and ending with 1847. On account of the increasing importance of the subject of the proper motions of the stars, it has been deemed expedient to employ two epochs in the formation of this catalogue. The observations made during the first six years, are reduced to the first of January, 1840; the epoch of the remaining observations is the commencement of 1845. The number of stars in the catalogue is 2156. One peculiarity of this catalogue consists in the constants of reduction being computed according to the modification of Bessel's method, which Mr. Airy had shortly before suggested.

Allusion has been made to Mr. Johnson's catalogue of southern stars. This distinguished astronomer, who was appointed Director of the Radcliffe Observatory, Oxford, upon the death of Prof. Rigaud in 1838, has been engaged for several years in re-observing the circumpolar stars of Groombridge's catalogue. This great undertaking is now all but completed. The number of stars observed will exceed the number in Groombridge's catalogue by about 2000. The epoch of reduction is the beginning of 1845. A comparison of this catalogue with Groombridge's cannot fail to lead to results of great importance.

In addition to the catalogues of stars alluded to in the foregoing pages, accurate catalogues of a select number of stars have been formed at every observatory, both in this country and on the Continent, where meridional observations form the chief object of attention. Apart from the special utility of these catalogues in so far as the practice of observation is concerned, they have in many instances been employed in some of the most delicate researches of modern astronomy*.

CHAPTER XX.

Early Notions of the Telescope.—Invention of the Telescope in Holland.—Galileo constructs a Telescope.—Kepler proposes the Telescope composed of Two Convex Lenses.—This Instrument first applied to Astronomical Purposes by Gascoigne.—Telescopic Observations of Huyghens and Cassini.—Reflecting Telescope proposed by Gregory.—Newton executes a Reflecting Telescope.—Efforts of his Successors to construct these Instruments.—Invention of the Achromatic Telescope by Dollond.—Reflecting Telescopes executed by Herschel.—Modern Improvements in the Refracting Telescope.—Improvements in the Construction of Reflecting Telescopes.—Lassell.—Lord Rosse.

THE telescope is justly considered to be one of the noblest inventions which the annals of human ingenuity can boast of. By its means the distant regions of space have been unveiled, and views of the material universe have been obtained, surpassing in splendour the most dazzling pictures of the imagination. Its penetrating power has revealed to us the astonishing fact, that far beyond the visible confines of the starry firmament, there exist countless myriads of suns and systems of suns, each of these glorious luminaries being in all probability the centre of a numerous cortége of revolving worlds. Nor has its influence been less apparent in conducting the mind to juster ideas respecting the system of which our own planet forms a part. The sun, which the ancient philosophers supposed to be merely a resplendent orb composed of a pure and immaculate substance, exhibits, when viewed with the telescope, unequivocal traces of a complex organization, accompanied with continual fluctuations in its physical condition, suggesting the sublime idea of its being the abode of innumerable objects of creative wisdom and beneficence. The planets, whose structure seemed to be equally mysterious with their movements, have been transformed from so many insignificant specks of light to an assemblage of magnificent worlds, presenting numerous points of analogy to the earth, and affording thereby irrefragable evidence in favour of the Pythagorean system of the universe. Even the loftier researches of physical astronomy are, to a great extent, dependent on the revelations of the telescope. Many of the beautiful conclusions that have been deduced from the theory of gravitation would fail to excite any interest in the mind of the enquirer, and the geometer would be deprived of some of his noblest triumphs, if the telescope were not available to trace their real counterpart in the heavens.

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* It may be stated in illustration of this remark, that a small catalogue of stars which Bessel constructed from his own observations at Königsberg, was employed by him in combination with the observations of Bradley and Piazzi, in his celebrated investigation of the quantity and laws of Precession. Again, M. Otto Struve has availed himself of a small catalogue of stars founded on observations made at Dorpat, in his recent researches on the same subject, in connexion with the great problem of the Motion of the Solar

The deep interest associated with the revelations of the telescope has called forth a variety of speculations respecting the probable epoch to which its origin may be referred. Some persons have pretended to discover in the writings of the ancient philosophers, indubitable proof that the telescope is not a modern invention. Thus it has been asserted that Democritus, who announced that the Milky Way is composed of a vast multitude of stars, could only have been led to form such an opinion by an actual examination of the heavens with the telescope. We are not warranted, however, in drawing so important a conclusion from a casual remark, however sagacious, any more than we should be justified in inferring that Seneca was in possession of the discoveries of Newton, because he predicted that comets would one day be found to revolve in periodic orbits. The same consideration applies to various other passages cited from the Greek and Latin writers, upon the strength of which attempts have been made to assign a high antiquity to the telescope.

When we come down to modern times, we find the invention of the telescope claimed for a still greater number of persons who flourished anterior to the epoch to which its origin is usually referred. Among these there is no individual who appears to have made so near an approach to the invention as our celebrated countryman, Roger Bacon. In the following passage, extracted from his *Opus Majus*, he describes the phenomena depending on the refraction of light by lenses with so much truth, that we should almost feel justified in ascribing to him some share in the invention both of the telescope and microscope. "Greater things than these may be performed by refracted vision. For it is easy to understand by the canons above mentioned that the greatest things may appear exceeding small, and the contrary. For we can give such figures to transparent bodies, and dispose them in such order with respect to the eye and the objects, that the rays shall be refracted and bent towards any place we please; so that we shall see the object near at hand, or at a distance under any angle we please. And thus from an incredible distance we may read the smallest letters, and may number the smallest particles of dust and sand, by reason of the greatness of the angle under which we may see them; and on the contrary, we may not be able to see the greatest bodies close to us, by reason of the smallness of the angle under which they may appear; for distance does not affect this kind of vision, except by accident, but the magnitude of the angle does so. And thus a boy may appear to be a giant, and a man as big as a mountain, for as much as we may see the man under as great an angle as the mountain, and as near as we please; and thus a small army may appear a very great one, and though very far off, yet very near to us, and the contrary. Thus also, the sun, moon, and stars may be made to descend hither in appearance, and to be visible over the heads of our enemies, and many things of the like sort, which persons unacquainted with such things would refuse to believe."*

The following passage, extracted from the preface, written by Thomas Digges, to the second edition of the *Pantometria* of his father, Leonard Digges, would also seem to imply that telescopes were not unknown in England previous to the seventeenth century;—"My father, by his continuell painfull practices, assisted by demonstrations mathematical, was able, and sundrie times hath, by proportional glasses, duly situate in convenient angles, not only discovered things farre off, read letters, numbered pieces of money, with the very coyne and superscription thereof, cast by some of his freends of purpose upon downs in the open fields, but also seven miles off declared what hath been doone in private places." Some remarkable passages to the same effect are to be found in Dee's preface to the edition of Euclid, published by him in 1570.

There are also two Italian philosophers who deserve to be mentioned in connection with the invention of the telescope. Battista Porta, in his *Magia Naturalis*, originally published in 1561, remarks that by combining together convex and concave lenses, objects may be seen enlarged. He then proceeds to describe an instrument capable of being constructed upon this principle, but his language is so utterly obscure as to defy interpretation. Again, Antonio de Dominis, Archbishop of Spalatro, in a posthumous work entitled *De Radiis Visus &c.*, which was published in 1611, traces very clearly the progress of rays in passing through convex and concave lenses, and remarks that by placing one of each kind at a certain distance apart, the direct and refracted rays will not interfere with each other. He adds that the proper distance between the two lenses must be found by experiment, and that the effect of their adjustment will be to magnify objects by increasing the angle under which they are seen. It is to be borne in mind, that this work was published at a time when the invention of the telescope was already notorious throughout Europe. Bartolo, the editor of the work, states, however, in the preface, that the manuscripts communicated to him by the author had been written twenty years previously, and that he received full authority from him to publish them *with the addition of one or two chapters.*

Notwithstanding the plausible statements contained in the various passages above cited, there does not exist clear proof that the speculations of any of the individuals alluded to, conduced to practical results, and therefore we cannot be justified in awarding to either of them the honour associated with the invention of the telescope.

It has long been generally admitted that telescopes were first made in Holland, about the beginning of the seventeenth century; but the question with respect to the real author of the invention has, until very recently, been involved in much obscurity. Three persons have been generally mentioned as possessing distinct claims to this much-contested honour. These are, Henry Lipperhey and Zacharias Jansen, spectacle makers in the town of Middleburg, and James Metius of Alkmaer, a son of the mathematician of that name, celebrated for having been the first who expressed the relation between the diameter and the circumference of a circle by the numbers 113 and 355.

Descartes, in his treatise on Dioptrics, published in the year 1637, thus alludes to the origin of the telescope:—"It is now about thirty years since a person named James Metius, who never studied at all, although his father and brother were both professors of mathematics, but who took pleasure in constructing mirrors and burning glasses, of which he had many of various forms in his possession, thought of looking through two lenses, one convex and the other concave. These happened on one occasion to be situated at the proper distance for magnifying objects, whereupon he applied them to the ends of a tube, and in this manner he constructed the first telescope."

Schyrlæus de Rheita, the Capucin friar, gives another account of the invention, in a work published a few years later*. He states that telescopes were first made by Lipperhey, whom he calls Lippensum, and he refers the date of the invention to the year 1609. Lipperhey, happening to place a concave before a convex glass, discovered by accident that the weathercock of a neighbouring church and other objects appeared nearer and larger. Having fitted the glasses in a tube, he placed the instrument in his shop, and amused his customers by showing them the magnified image of the weathercock. The Marquis of Spinola, who was then at the Hague, bought the instrument, and presented it to the Archduke Albert of Austria, by whom the great utility of the invention was first made known to the world.

During the early part of the seventeenth century numerous conflicting accounts circulated throughout Europe relative to the origin of the invention, which was in all cases alleged to be due to accident. It is singular that so much uncertainty should have prevailed respecting an event of so recent a date. In 1655, Pierre Borel, physician to the French king, published a work at the Hague †, professing to contain authentic particulars relative to this much-disputed question. He had taken considerable pains to collect all the evidence which might throw any light upon the matter, and he was warmly seconded in this object by William Boreel, the Dutch envoy at the Court of France. He searched out five individuals who were personally acquainted with the reputed inventors of the telescope, and induced them to make public declarations on the subject of the invention before the magistrates of Middleburg. The work above referred to contains the details of the examination of those persons, together with a letter of William Boreel's relative to the same subject. Two of the witnesses deposed in favour of Jansen, but they differ in the dates they assign to the invention, one of them, his son, fixing it in the year 1590; while the other witness, his sister, brings it down to 1611 or 1619. The other three witnesses declared that Lipperhey was the original inventor, one of them asserting that he made telescopes before the year 1605, while the other two fix the invention in the year 1609 or 1610. The depositions of these persons are so imperfect and contradictory, that little or no reliance can be placed on them.

The testimony of Boreel is more deserving of attention. He mentions, in his letter, that he knew Zacharias Jansen personally, and had often heard that he and his father were the real inventors of the microscope. When he was in England, in the year 1619, he saw in the hands of Cornelius Drebbel, his friend, the very microscope which had been given by them to the Archduke Albert of Austria. Boreel then continues:—"But long after, in 1610, after much research, these individuals invented, in Middleburg, the long sidereal telescope with which we gaze at the moon and planets, and presented one of them to Prince Maurice, who deemed it prudent to conceal the invention in order to make use of it in military operations. While rumours were abroad regarding the invention, a stranger came to Middleburg, and having asked for a spectacle maker, was shown by mistake into Lipperhey's shop. From the questions put by the stranger, Lipperhey, being a shrewd man, was enabled to detect the construction of the instrument, and having succeeded by this means in making telescopes, he was generally considered the real inventor of them. However, the mistake was soon discovered, for Drebbel, upon his return to Holland, proceeded with Adrian Metius to Jansen's shop, and purchased telescopes of him."

It follows, then, from Boreel's letter, that Jansen invented, first the microscope, and, after the lapse of several years, the telescope. This account tends in some degree to reconcile the conflicting statements of the two witnesses who deposed in favour of that individual: but indeed the whole evidence contained in Boreel's book is of so meagre and unsatisfactory a character, as to leave the question relative to the actual inventor of the telescope, still open to dispute.

In the first volume of the "Journal of the Royal Institution," published in 1831, there appeared an interesting communication from Dr. Moll, of Utrecht, relating to the invention of the telescope. The materials of this paper are due to the researches of Van Swinden, who had succeeded in discovering some authentic records, which serve to remove much of the obscurity that has hitherto prevailed respecting the real author of the invention.

In the library of the University of Leyden there exists, among the manuscripts of Huyghens, an original copy of a petition sent to the States General by Jacob Andrianzoon, the person whom Descartes calls James Metius. This petition is dated October 17, 1608. The object of it is to obtain the exclusive right of selling an instrument he had invented, by means of which distant bodies appeared larger and more distinct. He declares that the idea of the instrument occurred to him accidentally, while he was engaged in making other experiments; that he afterwards bestowed much attention on the subject, and had succeeded so far in bringing the invention to maturity, as to make objects appear as visible and distinct by his instrument, as could be done with *the one which had been lately offered to the States by a citizen and spectacle maker of Middleburg*. Metius was exhorted to bring his instrument to greater perfection, when his petition for a privilege would be taken into consideration.

Among the Acts of the States General preserved in the Government Archives at the Hague, Van Swinden found some interesting documents relative to the spectacle maker of Middleburg, alluded to in the petition of Metius. On the 2nd of October, 1608, the Assembly of the States took into consideration the petition of John Lipperhey, spectacle maker, a native of Wesel, and an inhabitant of Middleburg, inventor of an instrument for seeing at a distance. A committee was appointed to confer with Lipperhey, for the purpose of ascertaining whether it might not be possible to improve the instrument so as to enable one *to look through it with both eyes*. On the 4th of the same month it was resolved that certain of the members should test the instrument of Lipperhey, by observing with it from the turret of Prince Maurice's mansion. It was further resolved, that if the perspective should be found useful, an engagement should be entered into with the inventor, to execute three such instruments of rock crystal, and that he should be enjoined not to divulge the invention to anybody. On the 6th of the same month, the Assembly agreed to give Lipperhey 900 florins for such an instrument. On the 15th of December they examined the instrument invented by Lipperhey *to see with both eyes*, and approved of it; but as many others had a knowledge of this new invention to see at a distance, they did not deem it expedient to grant him an exclusive privilege to execute such instruments. However, they gave him orders to execute, for the use of the Government, two other instruments to see with both eyes, allowing him the same remuneration for his services as in the first instance.

The evidence afforded by the documents found among the manuscripts of Huyghens, restores to James Metius his right to the invention of the telescope, which had not been taken notice of in Borel's book, and was probably allowed, in consequence, to fall into oblivion. It would seem also, from the same document, that the telescopes of Metius could bear a comparison in point of efficiency with those constructed about the same time by another individual. However, as this statement rests entirely upon his own authority, and as he does not appear to have given the benefit of his invention to the world, we cannot consider his labours to be entitled to much consideration. The other documents prove that Lipperhey was in possession of the invention as early as October, 1608, and therefore that he could not have been indebted for his views on the subject to Jansen, who, according to William Boreel's letter, succeeded in constructing telescopes only in the year 1610. No mention of Jansen could be found in any of the State Records, and as his right to the original invention of the telescope is set aside by the authentic documents in favour of Lipperhey, there only remains to be claimed for him the invention of the microscope, upon the evidence set forth in Borel's book. We may conclude, then, that Lipperhey was the person who originally executed telescopes, and also that he was the first who made them known to the world; and, therefore, upon these grounds he possesses a just claim to the honour associated with the invention.

It is quite clear that Lipperhey must have been no common artizan, from his adapting his invention so readily to the views of the Assembly of the States General. The worthy individuals composing that Assembly were by no means satisfied with an instrument that was destined to penetrate into the immensity of space, and reveal the existence of unnumbered worlds, unless it was so executed as to enable them to see through it with both eyes. We have seen that a telescope of this construction was mitted to trial, it was cordially approved of by the Committee of their Highnesses. It appears, then, that this ingenious individual was the first person who executed the *binoculus*, an instrument the invention of which has been usually ascribed to De Rheita. Lipperhey doubtless fell upon the construction of the telescope by combining together lenses in various ways, as we may easily conceive that an optician might be induced to do. Attempts have been made to depreciate the merit of the Dutch inventor, on the ground that his discovery was made by chance. There exists, indeed, no reason to suppose that he was acquainted with the principles, whether mathematical or physical, upon which the construction of the telescope depends, or that he was indebted to a knowledge of those principles for the admirable result at which he arrived; but how few original discoveries or inventions, either in the arts or sciences, have been achieved by the aid of purely theoretical considerations; and, on the contrary, how many have been suggested to their authors by circumstances apparently unconnected with the object of their researches, and, therefore, to a certain extent, fortuitous! But, in fact, this circumstance of accidental discovery is no other than the result of that sagacious observation which distinguishes one man from a multitude of others, and enables him to select from the multitude of objects that claim his attention such relations as possess a character of generality that renders them subservient in extending the conquests of the human mind over the material world. Theory serves to develope principles that have been already established by induction, and in this manner acts the part of a valuable guide to the discovery of derivative truths; but in attempting to penetrate the secrets of nature, or in devising new combinations of principles with a view to the attainment of any specific end, the mind must rely mainly on its own innate sagacity.

Galileo has related the circumstances under which he first became acquainted with the invention of the telescope, and has also given an account of his early efforts to construct one of these instruments. Happening to be in Venice in the month of May, 1609*, he learned in that city that a Belgian had invented a perspective instrument, by means of which distant objects appeared nearer and larger. This report, which was soon confirmed by a letter he received from Paris, excited an intense interest in his mind, and upon his return to Padua he began to ponder upon the probable mode by which such an effect could be produced. He discovered, upon the very night of his arrival, that the enlargement of the object depended upon the doctrine of refraction, and on the following day he made his first attempt to execute an instrument upon this principle. Having procured a leaden tube, he fitted in one of its extremities a plano-convex lens, and in the other a plano-concave one, and, having applied his eye to the concave lens, he was delighted to perceive objects pretty much magnified. They appeared to be three times nearer and nine times larger. He immediately gave intimation of his success to his friends in Venice, with whom he had been conversing on the subject the previous day. Having succeeded a few days afterwards in making a better instrument, he joyfully proceeded to Venice, taking it with him. The perspective of the Paduan philosopher soon became an object of universal admiration in Venice. In order to afford the senators of the republic a practical proof of its efficiency, Galileo accompanied them to the more elevated parts of the city, whence, by looking through it, they surveyed the various objects of the surrounding landscape with inexpressible surprise and delight. During the whole period of his residence at Venice, which extended to a month, he was besieged from morning to night by crowds of the inhabitants, all eager to have a peep through the wonderful glass. He had not yet divulged the secret of his invention, but, with his wonted liberality, he now communicated it to the public, and, at the suggestion of one of his friends, he presented the instrument itself to the Doge Leonardi Deodati, who accepted it from him, sitting in full council. He accompanied it with a paper, in which he gave an explanation of its construction, and of the important purposes to which it might be applied both on land and at sea. The Senate indicated their appreciation of this novel compliment which Galileo paid to the chief magistrate, by increasing his salary as lecturer in the University of Padua to 1000 florins, and appointing him to his office for life.

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* This is inferred from the expression "mensibus abhinc decem fere," used by him

Galileo's object now was to construct a telescope of superior magnifying power. This he found to be a task of very difficult accomplishment, for the art of grinding and polishing lenses was then in its infancy. We have seen that at his first trial the instrument made objects appear three times nearer and nine times larger; in other words, it amplified three times in linear dimensions, and nine times in superficies. Soon afterwards he made another instrument which magnified sixty times in surface; and finally, sparing neither application nor expense, he succeeded in executing an instrument of such excellence as to represent objects almost 1000 times larger, and above 30 times nearer, than they appeared to be by the natural power of the eye *.

He points out a simple method of determining by experiment the magnifying power of the instrument. "Place," says he, "upon a wall at a certain distance, two disks, one of which you will observe with the telescope and the other with the naked eye. If the disk seen through the telescope appear equal to the external one, the magnifying power of the instrument is in the proportion of the two disks. If they do not appear equal, the external disk must be enlarged or diminished until this result takes place, and then the magnifying power will be, as already mentioned, in the proportion of the two disks."

Galileo's best telescopes did not magnify more than thirty-two or thirty-three times. Previous to the invention of achromatic object-glasses, this was the highest magnifying power that was attainable with telescopes of the form of construction employed by him and his contemporaries. The triumphs of genius are signally illustrated in the multitude of beautiful discoveries which Galileo achieved by the aid of his little perspective. Whatever claims other individuals may possess to the original invention of the telescope, the merit of applying it to the great purposes of physical science must be awarded, with acclamation, to the illustrious philosopher of Italy.

The telescopic discoveries of Galileo exercised such a withering influence upon the ancient philosophy, that many of its adherents, in the height of their mortification, took refuge in absolute incredulity, and, with absurd pertinacity, refused to assure themselves, by actual inspection, of the reality of the phenomena thus revealed for the first time to mortal eyes. "Oh! my dear Kepler," says Galileo, in one of his letters to that

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* Opere di Galileo, tome ii., pp. 5 et 267.

astronomer, "how I wish that we could have one hearty laugh together! Here, at Padua, is the principal professor of philosophy, whom I have repeatedly and earnestly requested to look at the moon and planets through my glass, which he pertinaciously refuses to do. Why are you not here? What shouts of laughter we should have at this glorious folly, and to hear the professor of philosophy in Pisa labouring before the Grand Duke with logical arguments, as with magical incantations, to charm the new planets out of the sky!"

Galileo can hardly be considered an independent inventor of the telescope. Admitting that he obtained no other account of the Dutch invention, than that a spectacle maker had succeeded in executing a perspective instrument by means of which distant objects appeared nearer, still the fact of his having received such a hint deprives his labours in a great degree of the character of originality. When once a problem is known to be resolvable, the difficulties which it involves may then be overcome with comparative ease by a strong concentration of the mental powers upon it. But, in justice to the original inventor, and the country which gave him birth, it is but right to mention, that more than one of the Dutch instruments had found their way into Italy about the time when Galileo was led to construct his first telescope. Sirturus, writing in 1618, makes the following statement:—"A Frenchman proceeded to Milan in the month of May, 1609, and offered a telescope for sale to the Count di Fuentes."* Again, in a letter from Padua, dated August 31, 1609, Lorenzo Pignoria announces to Paolo Gualdo that Galileo had been appointed lecturer at Padua for life on account of a perspective, *like the one which was sent from Flanders to Cardinal Borghese.* "We have seen some here," adds the writer, "and truly they succeed well."†

We dismiss, as an unfounded calumny, the assertion of Fuccarius, that Galileo had actually seen one of the Dutch telescopes, nor can we even admit, in the face of his own words to the contrary ‡, that he received any hint relative to the peculiarity of their construction; but the notoriety of the invention, as confirmed by the preceding extracts, could not fail to have operated as a powerful incentive to his efforts.

Some writers have been disposed to think that the Dutch telescope was constructed upon a different principle from Galileo's instrument. Proceeding upon the tradition of its having been originally exhibited as an amusing toy, by producing an inverted image of a weathercock, they have hence inferred that it was composed of two convex lenses, like the modern astronomical telescope. This conclusion, if true, would certainly go far to enhance Galileo's merit, but it rests on so narrow a foundation as to be totally inadmissible, being suggested solely by the vague tradition just mentioned. On the other hand, Descartes and De Rheita positively assert that the Dutch instrument was formed by a combination of a convex with a concave lens. The account of the invention as given by the latter of these two writers is the more worthy of confidence, inasmuch as he associates it with the story of the weathercock, but, of course, makes no allusion to an inverted image. Again, we find Lorenzo Pignoria writing to his friend, that Galileo had received a thousand florins annually for life, on account of a perspective *like* the one which had been sent from Flanders; but it is very obvious that if the Dutch telescopes had exhibited objects inverted, he would have expressed himself in different language.

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* Sirturus de Telescopio, Franc., 1618.

† "Di nuovo non abbiamo altro se non la reincidenza di S. Serenità, e ricondotti di Lettori, fra' quali il Sig. Galileo ha buscato 1000 fiorini in vita, e si dice col benefizio d'un occhiale simile a quello, che di Fiandra fu mandato al Card. Borghese. Se ne sono veduti di qua, e veramente fanno buona riuscita."—(*Lettere d' Uomini Illustri,* p. 112, Venez., 1744.

‡ The following are the terms in which he alludes to the information he received respecting the Dutch invention on the occasion of his visit to Venice, as mentioned in the text:—"Giunsero nuove, che al Sig. Conte Maurizio era stato presentato da un Olandese un occhiale, col quale le cose lontane si vedevano cosi perfettamente, come se fussero state molto vicine, *nè più fu aggiunto.*" (*Opere di Gal.*, Edit. Pad., 1744, tom. ii. p. 267.)

The Dutch telescopes were, doubtless, much less powerful than those of Galileo, but this is not to be wondered at, when we consider the consummate skill of the latter in matters relating to practical science, and the remarkable success which generally attended his experimental efforts. Still, we are by no means disposed to believe that they were such mean productions of art as they have been frequently represented to be. We have seen that the States General agreed to give 900 florins to Lipperhey for his instrument. The amount of remuneration tends to impress us with a favourable opinion of the instrument given in exchange, for, as Dr. Moll justly remarks, the Dutch people cannot be charged with a readiness to throw away their money upon a trifling toy, which possessed no other claim to recommendation than its novelty. It is true that the inventor was prohibited from making any telescopes except those ordered by the States General, and the latter would naturally consider themselves bound, on this account, to remunerate him more liberally for his labours than they would otherwise feel disposed to do. This prohibition, however, tends to confirm our belief of the value they set upon his telescopes. They refused him an exclusive privilege to make and sell them, on the alleged grounds that others were already in possession of the invention, but they reasonably believed that the instruments of the inventor would be superior to any others constructed, and they accordingly resolved to secure to themselves the monopoly of his skill. That the invention had already transpired, and that a restriction, such as we have mentioned, had been imposed upon the inventor, is clearly proved by the following extract from the correspondence of the celebrated president, Jeannin, who had been sent to Holland by his sovereign, Henry IV., to negotiate a truce between Spain and the States General. On the 28th of December, 1608, only a few days after the States General had refused the object of Lipperhey's petition, Jeannin thus writes to Henry's minister, the famous Sully:—"The bearer of this letter is a soldier from Sedan, who belongs to the Prince's company, and who is held very ingenious in many inventions and artifices of war. He has also made, a few days ago, an instrument, in imitation of that which has been made by a spectacle maker of Middleburg, to see at a distance. He will show it to you, and make you some for your sight; I requested the original inventor to make me two, one for the king, and one for you; but the States prohibited him from making any but for themselves. They have, however, given orders to execute some for me, that I may send them to you, which I will not fail to do as soon as I receive them."* It is quite clear, from the foregoing passage, that the States General contemplated making use of the instrument of Lipperhey in military reconnaissances; and their formal approval of it already referred to, when taken in connexion with this circumstance, implies that it possessed a considerable magnifying power.

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* "Lettres et Négociations du Président de Jeannin," fol. Paris, 1656 (cited by D

We have seen how quickly the Dutch telescopes passed into France and Italy. If we are to believe the German astronomer, Simon Marius, he commenced astronomical observations in the year 1609, with a telescope which he procured from Holland. Telescopes were also imported into England soon after their invention. The late Professor Rigaud found, by an inspection of the manuscripts of Hartiot, the celebrated mathematician, that even as early as July, 1609, he had been making observations of celestial phenomena with one of these instruments. This fact tends very much to disprove the assertion that the telescopes originally made in Holland were totally unfit for scientific purposes, for it can hardly be doubted that the one which Harriot used was obtained from that country. On the 6th of July, 1610, Sir Christopher Heyden thus writes to Camden, the famous antiquarian:—"I have read Galilæus, and, to be short, do concur with him in opinion, for his reasons are demonstrative; and, of my own experience, with one of *our ordinary trunks* I have told eleven stars in the Pleiades, whereas no age ever remembers above seven, and one of them, as Virgil testifieth, not always to be seen."* There can be no doubt that the expression—"one of our ordinary trunks"—refers to the Dutch telescopes which were in common use in England, as distinguished from the superior telescopes of Galileo recently imported from Italy.

In a letter dated Tra'venti in Caermarthenshire, Feb. 6, 1610, Sir William Lower thus writes to Harriot:—"I have received the perspective cylinder that you promised me, and am sorrie that my man gave you not more warning, that I might have also the two or three more that you mentioned to chuse for me. Henceforward he shall have orders to attend you better, and to defray the charge of this and others, for he confesseth to me that he forgot to pay the workman.

"According as you wished, I have observed the moone in all his changes. In the new manifestlie I discover the earthshine a little before the dichotomie; that spot which represents unto me the man in the moone (but without a head) is first to be seene. A little after, neare the brimme of the gibbous parts towards the upper corner appeare luminous parts like starres; much brighter than the rest; and the whole brimme along looks like unto the description of coasts in the Dutch books of voyages. In the full she appeares like a tart that my cooke made me last weeke; here a vaine of bright stuffe, and there of darke, and so confusedlie all over. I must confesse I can see none of this without my cylinder; yet an ingenious younge man that accompanies me here often, and loves you, and these studies much, sees manie of these things, even without the helpe of the instrument, but with it sees them most plainlie—I mean the young Mr. Protheroe." †

We have deemed it proper to dwell at some length upon the early history of the Dutch telescopes, because those instruments have been thrown into unmerited oblivion by the splendour of Galileo's discoveries. We cannot reasonably avoid the conclusion, when the subject is taken into complete consideration, that, even although the last-mentioned philosopher had never lived, the Dutch telescopes would have been gradually perfected in their construction, and would have been eventually applied with success to the great purposes of physical science. The merits of the original inventor of the telescope cannot affect the reputation of the illustrious Italian, who owed his brilliant discoveries in celestial physics much less to his good fortune in executing a telescope, than to his sagacity as an interpreter of nature.

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* Supplement to Bradley's Miscellaneous Works, p. 27. † Ibid., p. 42.

Kepler first explained the construction of the telescope formed by a combination of two convex lenses, in his treatise on Dioptrics, which was published in 1611. Possessing no aptitude for observation, however, he did not attempt to reduce his ideas to practice. Scheiner appears to have been the first person who actually executed a telescope upon the principle suggested by Kepler. In his *Rosa Ursina*, which was published in 1630, he states that, thirteen years previously, he made observations in the presence of the Archduke Maximilian, with a telescope composed of two convex lenses. The Galilean telescope, however, still continued to be preferred, on account of the superior distinctness of the image which it formed.

The first person who appears to have discovered the peculiar value of the telescope recommended by Kepler, and who applied it to the purposes of astronomical observation, was Gascoigne. This highly-gifted young man did not fail to perceive that it possessed an immense advantage over the Galilean telescope, in affording a situation within the tube where any object being placed might be viewed as distinctly as a celestial object. This circumstance suggested to his inventive mind the use of telescopic sights in astronomical observations, and the application of the micrometer to the telescope. The micrometrical measures of that astronomer, preserved by Flamsteed in the first volume of the *Historia Celestis*, may be considered as the earliest collection of facts that have been established by the use of the Keplerian telescope. In this respect they possess an interest analogous to that associated with the early discoveries of Galileo relative to the *physical constitution* of the celestial bodies.

It was not until about the middle of the seventeenth century, that telescopes composed of two convex lenses came to be generally employed in astronomical observations. The use of such telescopes, however, was then suggested, not on account of the advantage which Gascoigne found them to possess, but because they afforded a much larger field of view than could be obtained by means of the Galilean telescope. The first person who distinguished himself by his labours in connexion with the Keplerian, or astronomical, telescope was Huyghens. This distinguished philosopher attained great excellence in the grinding and polishing of lenses, and succeeded in constructing telescopes far superior to any hitherto executed. In the year 1655 he discovered a satellite revolving round Saturn in sixteen days, by the aid of a telescope 12 feet long, and by prosecuting observations of the planet with the same instrument, he was shortly afterwards enabled to establish the real nature of the appendage with which it is furnished.

As the definition of objects was very imperfect in telescopes of this construction, it was found to be indispensable in every instance wherein a high magnifying power was required, to increase the focal length of the object-glass to an immense extent. Huyghens states that he and his brother made object-glasses of 170 and 210 feet focal length! These glasses were used without tubes. Huyghens devised mechanical combinations of great ingenuity, by means of which they could be directed with facility towards any object in the visible heavens. From the circumstance of their being used without tubes, these contrivances were termed *aërial telescopes*. Huyghens presented one having a focal length of 123 feet to the Royal Society of London. This philosopher effected a capital improvement in the construction of telescopes by the invention of an eyepiece consisting of a combination of lenses which, by their respective refractions, served to correct the effects of spherical aberration.

While Huyghens was engaged in these labours, the art of executing telescopes was attaining a high degree of excellence in Italy. Eustachio Divini at Rome, and Campani at Bologna, more especially signalised themselves in this department of practical optics. It was with telescopes executed by the latter that Cassini made the multitude of interesting discoveries in the heavens which have rendered the name of that astronomer so deservedly celebrated. The object-glasses had, in some instances, focal lengths of 90, and even 130, feet. Cassini was enabled, like Huyghens, to make use of such glasses without tubes.

Telescopes of great length were constructed in most other countries of Europe during the seventeenth century, especially in France and England. Auzout is said to have made a telescope of 300 feet focal length. It does not appear, however, that he actually employed such an instrument in astronomical observations. In England the construction of telescopes seems to have attained considerable perfection at a comparatively early period. Among the instruments with which the observatory of Copenhagen was furnished, on the occasion of its original establishment in 1656, was a telescope magnifying 100 times, which Longomontanus brought with him from England in the year 1642. The telescopes of Sir Paul Neile, as well as those of Reeves and Cox, are said to have been equal to the best telescopes executed at the same time in any country of Europe. It speaks very much in favour of the English telescopes made during the latter part of the seventeenth century, that with one of such instruments, of 38 feet focal length, William Ball of Mainhead, in Devonshire, having observed Saturn on the 13th October, 1665, perceived that the ring around the planet was double *. This observation was made ten years before Cassini remarked a similar appearance on the ring. It appears that Bradley occasionally employed telescopes of great length, in the early part of his career. On the 27th of December, 1722, while he was residing at Wanstead with Pound, his uncle, he measured the diameter of Venus with a telescope, or rather object-glass, of 212¼ feet focal length †.

Notwithstanding the vast amount of labour and skill bestowed upon the construction of refracting telescopes, it was still found that the images formed in these instruments were by no means so well defined as was necessary for delicate observations. This circumstance was attributed to the spherical aberration of the lenses, but, although the indistinctness no doubt partly arose from that cause, it was mainly due to the influence of a physical principle which had not yet been discovered. When, to the imperfect formation of the image, the inconvenience attending the use of telescopes of enormous focal length was taken into account, it obviously became desirable to devise a different mode of construction. In 1663, James Gregory published his *Optica Promota*, in which he explained the construction of the reflecting telescope which has since been called by his name. He proposed that the rays of light from a remote object should be received by a concave parabolic speculum, and, after forming by reflexion an image of the object at the focus, should hence diverge and fall upon a smaller concave speculum of an elliptic figure having the same focus as the larger one. The rays would now be reflected by the elliptic speculum, so as to form a second image of the object near the anterior surface of the larger, or parabolic, speculum; and this image might finally be viewed by the observer through an eye-lens placed behind the latter speculum, which was to be perforated for that purpose. Gregory expected that, by substituting the principle of the reflexion of light for that of refraction, so as to get rid of the effect of spherical aberration, the image of the object would be rendered much more distinct. When he was in London, about the year 1664, he employed an optician to execute a telescope for him according to the above design, but the attempt to give a true parabolic figure to the larger speculum proved a failure; and, although he subsequently tried to effect the same object by means of a spherical speculum, yet, in this instance also, the result was so unsatisfactory that he seems to have been discouraged from prosecuting the matter further.

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* Phil. Trans. 1666, p. 153.

It appears that, before the publication of the *Optica Promota*, Mersenne had already suggested the idea of a reflecting telescope. This he did originally in a letter to Descartes written about the year 1639 *, and, afterwards, in his Catoptrics, which was published in 1651. Descartes offered several objections to Mersenne's proposal, and no attempt was made to carry it into effect. It has been said, by Fontenelle, that Father Zucci was acquainted with the reflecting telescope as early as 1618. Indeed, Montucla has justly remarked, that as soon as it was known that the image of an object might be formed in the focus of a concave mirror, the idea of applying the principle of reflexion to the construction of telescopes (after the invention of refracting telescopes) was a very obvious suggestion. It is impossible to ascertain whether Gregory was indebted to any previous writer for the hint of the reflecting telescope, or not; but, at any rate, it must be allowed that he was the first person who gave a complete explanation of the construction of such a telescope. Moreover, he has the merit of having suggested a form of the instrument which has been in use ever since, and which, as far as ordinary purposes are concerned, has been found more convenient in practice than any other that has subsequently been devised.

The first person who actually executed a reflecting telescope was Newton. As soon as that philosopher discovered the unequal refrangibility of light, about the year 1666, it occurred to him that any further improvement of the refracting telescope was impossible, and he was led, in consequence, to consider the application of the principle of reflexion to the same object. Finding that for the different rays of the prism, the angle of reflexion was in each case equal to the angle of incidence, he perceived that, by means of this property of light, optical instruments might be brought to any degree of perfection, provided a substance could be found which reflected a sufficient quantity of light, while at the same time it was capable of being highly polished, and of receiving a true parabolic figure. But the difficulties which stood in the way of accomplishing the fulfilment of these conditions, were very great; nay, they almost seemed to him to be insuperable, when he recollected, that every irregularity in a reflecting surface would make the rays deviate five or six times more out of their due course than in a refracting telescope *. While his mind was occupied with these reflections, he was compelled, by the prevalence of the plague, to quit Cambridge, and more than two years elapsed before he again turned his attention to the subject. Having now reflected upon the construction of Gregory's instrument, he came to the conclusion that it would be preferable to place the eye-glass in the middle of the tube. Accordingly, having succeeded, after much trouble, in discovering a substance that was adapted for the formation of the larger speculum, and having also devised a delicate mode of polishing the surface, he actually executed a telescope upon this principle. He wisely abstained from attempting to make the speculum truly parabolic, on account of the extreme difficulty of the operation, contenting himself with giving it the figure of a segment of a sphere. With this instrument he could see the satellites of Jupiter and the horns of Venus; but the latter were not very distinct, nor could they be perceived at all without some nicety of adjustment. Encouraged by the success of his efforts, he made a second telescope, which proved to be still better than the former one. The focal length of the speculum was $6\frac{1}{3}$ inches, and that of the eye-glass (which was a plano-convex lens, with the flat side turned towards the eye) was $\frac{1}{6}$th of an inch; the magnifying power of the instrument was therefore 38.

In the month of December, 1671, Newton transmitted to London the second reflecting telescope executed by him, in order that it might be submitted to the inspection of the Royal Society. At the meeting of the Society, held on the 11th of January, 1672, it was announced that Mr. Newton's instrument had been examined by the President, Sir Robert Moray, Sir Paul Neile, Dr. Wren, and Mr. Hooke, and that it had received their unanimous commendation. The Society at the same time resolved, that in order to secure the right of the inventor, a description of the instrument should be sent to Huyghens, who was then residing at Paris †. Huyghens communicated an account of it to the Academy of Sciences, and in consequence it soon became generally known upon the Continent ‡.

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* Phil. Tran., 1672, p. 3080.

† Birch, Hist. Roy. Soc., vol. iii. p. 1. The same telescope was afterwards presented to the Royal Society. This interesting relic of the immortal philosopher is still to be seen at the apartments of the Society in Somerset House.

‡ Huyghens addressed a letter to the Academy of Sciences, in which he gives a description of Newton's reflecting telescope, and enumerates its peculiar advantages. The following passage, extracted from this letter, is worthy of notice.—"Je compte pour un troisième avantage, que par la réflexion du miroir de métal, *il ne se perd point de rayons*, comme aux verres qui en réfléchissent une quantité notable, par chacune de leurs surfaces, et en interceptent encore une partie par l'obscurité de leur matière."—(*Mém. Acad. des Sciences*, tome x., p. 506.) M. Arago cites this passage as a proof that Huyghens was not aware of any light being lost in the course of its reflexion from a metallic surface, and he naturally expresses his astonishment that the Dutch philosopher should have been unacquainted with so obvious a principle of physics. (*Annuaire*, 1842, p. 293.) It would appear, however, from the following passage of a letter addressed by Huyghens to the Royal Society in answer to their communication respecting Newton's telescope, that the case cannot be made out so clearly against him:—"Again, by the meer reflexion of the metallic speculum, *there are not so many rays lost* as in glasses, which reflect a considerable quantity by each of their surfaces, and besides intercept many of them by the obscurity of their matter."—(*Phil. Trans.*, 1672, p. 4008.) Whether Huyghens had corrected his mistake in the latter of the two passages just cited, or whether his views on the subject have been misrepresented in one or other of the two passages, we leave our readers to decide. The communication inserted in the Memoirs of the Academy of Sciences, bears no date. The letter to the Royal Society is dated Feb. 13, 1672.

In 1672 Cassegrain, a Frenchman, devised a new form of the reflecting telescope. It differed from the Gregorian telescope in the small mirror being *convex*, and in its being placed so as to intercept the rays *before* they came to a focus. This telescope is more convenient than the Gregorian, inasmuch as it is shorter, *cæteris paribus*, by twice the focal length of the small mirror. Ramsden remarked, that the effect of spherical aberration was also less than in the Gregorian telescope *, and more recently, Captain Kater shewed, by a series of experiments, that it was greatly superior in point of light and distinctness to any telescope of the latter construction, possessing an equal aperture †. Notwithstanding these alleged advantages, this form of the reflecting telescope has been comparatively seldom used since its invention. As has been already hinted at, the Newtonian telescope exhibits objects not in their true direction, but at right angles to it. Hooke was the first person who executed a telescope, the observations with which were made by viewing the object directly. This he effected in 1674, by perforating the centre of the larger speculum agreeably to the suggestion of Gregory ‡.

Although both Newton and Hooke shewed that the construction of reflecting telescopes was perfectly practicable, it does not seem that for a long time any further attempts were made to execute such instruments; at all events no progress was made in bringing them to perfection. The first person who succeeded in accomplishing this object was Hadley, the individual whom we have had occasion to allude to, as one of the independent inventors of the reflecting octant. In 1723 he presented to the Royal Society a reflecting telescope of the Newtonian form, which he executed with his own hands. The diameter of the speculum was 6 inches, and its focal length was 5 feet 3 inches. Pound and Bradley, having compared it with the refracting telescope of 123 feet focal length, which Huyghens had presented to the Society, found that it bore as high a magnifying power as that instrument, and exhibited objects equally distinct though not quite so bright. Notwithstanding its inferiority in the latter respect, they were enabled to perceive with it all the phenomena which they had hitherto discovered with the Huygenian telescope, such as the transits of Jupiter's satellites; their shadows upon the disk of the planet; the *black list* on Saturn's ring; and the edge of his shadow upon the ring. On several occasions they succeeded in seeing with it the five satellites of Saturn §. Reflecting telescopes were executed by Hawksbee, about the time that Hadley was prosecuting his labours with such success. This individual made a telescope of 3½ feet focal length, which proved to be even more perfect than any of Hadley's. About the same time Bradley and Molyneux, having been instructed by Hadley in the grinding and polishing of metallic specula, began to turn their attention to the construction of reflecting telescopes. After much trouble, they finally succeeded in executing several telescopes of considerable magnitude. One of these had a focal length of 8 feet ||.

Hitherto the execution of reflecting telescopes was confined exclusively to persons engaged in scientific pursuits, whence it may naturally be presumed that the number of such instruments actually made continued to be very limited. Molyneux, however, having acquainted Scarlett and Hearne, two London opticians, with the mode of grinding and polishing specula, these individuals commenced making reflecting telescopes for sale; and, in consequence, this class of instruments soon afterwards came into general use[*].

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* Phil. Trans., 1779, p. 427.

† Phil. Trans., 1813, p. 206; 1814, p. 231.

‡ Birch, Hist. Roy. Soc., vol. iii., p. 122.

§ Phil. Trans., 1723, p. 382.

The next person who distinguished himself by his skill in the construction of reflecting telescopes, was James Short, of Edinburgh. In 1732, when he was only twenty-two years of age, this individual turned his attention to the polishing of specula, in which he soon attained an unrivalled degree of excellence. He also discovered a method of giving them a true parabolic figure, which had not hitherto been done by any person. In his first attempts he confined himself to glass specula, which were made to reflect the light by covering them behind with quicksilver. James Gregory had recommended the use of glass for reflectors, on the ground of its absorbing less light than metal, and being at the same time less liable to tarnish[†]. With one of Short's glass reflectors, which had a focal length of only 15 inches, Maclaurin asserts that it was possible to read in the *Philosophical Transactions* at a distance of 280 feet[‡]. Finding that in such telescopes the light was fainter than in those in which the speculum was composed of metal, Short abandoned the use of glass, and confined himself in future to metallic reflectors. The telescopes of the latter description made by him appear to have been exquisite specimens of skill. Maclaurin states, that having compared some of them with instruments of much greater focal length, made by London artists, he found the former to be far superior in respect of brightness, distinctness, and magnifying power. With a metallic reflecting telescope of Short's, which had a focal length of only 15 inches, it was possible to read distinctly in the *Philosophical Transactions* at a distance of 500 feet, and to see the five satellites of Saturn all at the same time. In 1742 Short removed to London, where he continued to prosecute his former vocation with great success till his death, in 1768. One of the last instruments executed by him was a large reflecting telescope, which his brother Thomas mounted equatorially for the observatory of Edinburgh, and for which he was offered 1200 guineas by the King of Denmark. The telescopes of Short were all of the Gregorian construction.

It has been already mentioned, that as soon as Newton discovered the unequal refrangibility of light, he perceived that the existence of that principle offered a serious obstacle to any further improvement of the refracting telescope. This great philosopher did not fail to make experiments on the passage of a ray of light through several contiguous media, in order to ascertain whether the different refractions might not correct each other, so that the emergent ray, *while deflected from its original course*, might still be free of colour. The results of his labours on this occasion served, however, to convince him that no such compensation could be effected. He found, in fact, that when the ray emerged in a direction parallel to its original course, it was invariably white; but when it emerged obliquely with respect to the direction of incidence, it became gradually tinged with different colours in passing from the place of emergence *. Having satisfied himself in this manner that no combination of substances could be devised which would be capable of refracting the rays of light so as to produce a colourless image of an object, he seems to have despaired of improving the refracting telescope by any other means than that of increasing the focal length of the object-glass.

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[*] Smith's Optics, Book III. chap. 1.

[†] Newton in his *Optics* (1704) states that, about five or six years previously, he tried to make a reflecting telescope 4 feet in length, that would magnify 150 times, the speculum of which was of glass, and that he satisfied himself that there wanted nothing but a good artist to bring the design to perfection. He recommends glass specula in preference to metal, as being more easily polished, less liable to tarnish, and at the same time capable of reflecting more light. (*Optics*, part i. prop. vii.) Smith has inferred from the passages of Newton's work just cited that "the method of making reflecting telescopes with glass speculums, quicksilvered over," was first recommended by that philosopher. (*System of Optics, Remarks*, No. 489.) This, however, is a mistake. The person who first recommended glass specula for telescopes, was James Gregory, as has been mentioned in the text. Gregory made this suggestion in a letter to Collins, dated St. Andrew's, March 7, 1673, pointing out at the same time the peculiar advantages of glass specula, and requesting that his views on the subject might be communicated to Newton. The letter of Gregory was read at the meeting of the Royal Society, held on the 26th of March, 1673, and it was ordered, *that it should be communicated to Newton as the person most concerned in it.* (*Birch, Hist. Roy. Soc.*, vol. iii., p. 79.)

[‡] Smith's Optics (Remarks, No. 489).

It was reserved for Dollond † to shew that the views which guided Newton in arriving at this conclusion were erroneous, and to apply the true principle of physics to the construction of refracting telescopes free of colour. This highly-gifted individual, although moving in a humble sphere of life, had devoted much attention to mathematical and physical pursuits. He seems to have been originally led to consider the subject of the dispersion of light by the researches of Euler. In 1747 that distinguished mathematician communicated a memoir to the Berlin Academy of Sciences, in which he attempted to shew that, by a combination of different substances, it would be possible to construct object-glasses for telescopes, in which the effects arising both from spherical aberration and the unequal refrangibility of light would be completely corrected. He remarked that the different humours of the human eye refracted the rays of light, so as to produce a perfectly-distinct image of an object, and he argued by analogy, that a similar effect would be produced by the object-glass of a telescope, provided a suitable combination of substances was employed in its construction. Proceeding upon a certain hypothetic law of the dispersion of light, he accordingly devised a combination of hollow spherical lenses, enclosing water within their concave surfaces, which, he asserted,

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* Optics, Book II. Part I. Exper. viii. Newton states that he made this experiment with a prism glass and one of water, so adapting the two prisms that they refracted the light equally in opposite directions. In such a case, although the rays of light emerged parallel to the direction of incidence, they ought also to have been coloured, as Dollond, upon repeating the experiment in the following century, actually found them to be; but Newton asserts that he found the emergent rays to be white. Sir David Brewster states that the mistake of Newton arose from his having mixed a little sugar of lead with the water, in order to increase its refractive power, and that the high dispersive power of the ingredient caused the residuum of uncorrected colour to disappear (*Life of Newton*, p. 58). The same statement is made by Sir John Herschel, in his Treatise on Light, in the *Encyclopædia Metropolitana*. Neither of the two distinguished philosophers just cited has acquainted his readers with the source whence this explanation of Newton's mistake has been derived. Newton himself makes no mention of his having added any ingredient to the water in the part of his work above referred to, where he alludes to the experiment.

† John Dollond was born at Spitalfields, in London, in the year 1706. He was descended from French ancestors, who were compelled to quit Normandy upon the revocation of the edict of Nantes, by Louis XIV. In early life he worked at the loom, but in 1752 he joined his son as an optician. He died in 1761, having been struck with apoplexy, while engaged in an intense study of Clairaut's Theory of the Moon.

Although Dollond is entitled to the credit of having executed achromatic telescopes by the aid of his own researches on the dispersion of light, there is reason to believe that he was not the first individual who accomplished that object. It appears, in fact, that as early as the year 1729, Chester More Hall, a gentleman residing in Essex, having reflected upon the circumstance that the different humours of the eye so refract the rays of light as to produce images free of colour, was led to consider the possibility of combining together different substances, so as to form object-glasses for telescopes which would produce a similar effect. After devoting some time to the enquiry, he found that by combining together different kinds of glass, the effect of the unequal refrangibility of light was corrected; and, in 1733, he succeeded in executing telescopes which exhibited objects quite free of colour. One of these instruments, although its focal length was no more than 20 inches, bore an aperture of 2½ inches. More, who was living in independent circumstances, and who appears to have been nowise anxious about his reputation, did not take any trouble in communicating his invention to the world *. It has been insinuated that Dollond had seen one of More's telescopes, and that he was indebted to this circumstance for his construction of the achromatic object-glass; but the statement does not rest upon any solid foundation, and therefore it can only be regarded as an unfounded calumny.

The achromatic telescopes of Dollond soon became known throughout the whole civilised world, and were universally adopted by astronomers as affording an incontestable advantage over the ancient refracting telescopes. In one respect, however, they laboured under a defect which very much restricted the range of their efficiency. This consisted in the impossibility of obtaining pieces of pure flint-glass of sufficient magnitude to serve for telescopes of large aperture. It happened, from this circumstance, that no astronomical discoveries of importance resulted from the invention of the achromatic telescope. Huyghens and Cassini, by their combined exertions in the preceding century, seemed to have so thoroughly scrutinized the celestial regions, as to leave little or nothing to be gleaned by their successors, until instruments should be constructed capable of penetrating still further into the bosom of space. This remark is applicable to reflecting as well as refracting telescopes; for it is to be borne in mind, that not a single discovery had yet been made in the heavens by the use of a telescope constructed upon the principle of reflexion.

At length the discoveries of Herschel introduced a new era in the history of celestial physics. This highly-gifted individual, while occupying the humble situation of organist of the Octagon Chapel, Bath, to which he was appointed in the year 1766, was in the habit of devoting his leisure hours to the study of mathematics, and various branches of physical science, more especially optics and astronomy. Happening, on one occasion, to obtain the temporary use of a 2-feet Gregorian reflector, he was so transported with the celestial wonders which it revealed to him, that he conceived a passionate desire to procure a similar instrument for himself, and he instructed a friend in London to purchase one for him. The price

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* This account is taken from the *Gentleman's Magazine* for 1790, Part II., p. 890. The author states, that in a trial at Westminster Hall, about the patent for making achromatic telescopes, Mr. Hall was allowed to be the inventor; but Lord Mansfield observed, that "it was not the person who locked his invention in his scrutoire that ought to profit for such invention, but he who brought it forth for the benefit of the public."

demanded by the optician proving too great for his slender means, he resolved, with the true instinct of genius, to rely upon his own personal resources for attaining the object of his wish; and after a course of persevering efforts, he at length, in the year 1774, enjoyed the satisfaction of surveying the heavens with a 5-feet Newtonian reflector, which he constructed with his own hands. This was speedily followed by other reflectors of 7-feet, 10-feet, and even 20-feet focal length, all of which were exquisite specimens of artistic skill.

Armed with such powerful instruments, Herschel now proceeded to explore the heavens with all the enthusiasm which genius, stimulated by success, is capable of inspiring. He soon convinced astronomers that he was no ordinary amateur, who had betaken himself to the observation of celestial phenomena, by the high magnifying powers which he employed, amounting to 2000, 3000, and even in some instances to 6500. These numbers far surpassed the magnifying powers hitherto employed in telescopic observations. In 1781 his perseverance was rewarded by the discovery of the planet Uranus. Soon afterwards, George III. having bestowed upon him a liberal pension, he abandoned his original vocation, and henceforth devoted himself exclusively to astronomical pursuits. Having removed from Bath, he established himself first at Datchet, in the neighbourhood of Windsor, and subsequently at Slough, where he continued during the remainder of his life to prosecute a career of astronomical discovery, which has few parallels in any age or country. The telescopes which Herschel employed in the early part of his astronomical observations, were all of the Newtonian construction. In 1786, however, he laid aside the small mirror, and adopted the form of construction which he distinguished by the appellation of *the front view*. By giving a slight inclination to the speculum, so as to throw the image a little to one side of the tube, it was possible to view the latter directly with an eye-glass. By this contrivance the light usually absorbed by the small mirror was saved, and the illumination of the image increased in a corresponding degree. In such a telescope it is obvious that the observer looks at the image with his back turned towards the object. Herschel applied this form of construction to all the instruments which he subsequently employed in his astronomical observations. His discovery of two satellites around Uranus was a result which speedily followed its adoption. It is right to mention that a similar form of construction had been already proposed by Lemaire, a Frenchman, as early as the year 1732.

In 1789 Herschel surpassed all his former efforts as a practical optician by the completion of a telescope of 40-feet focal length and 4-feet aperture. This gigantic instrument was no sooner turned towards the heavens than it revealed to him the existence of two satellites around Saturn which had hitherto escaped the scrutinies of astronomers. It is impossible, within a moderate compass, to give even a simple enumeration of the multitude of brilliant discoveries in celestial physics which rewarded the labours of this great astronomer. It may be remarked, however, that admirable as were the immediate results of his telescopic observations, they would have failed to secure to him the exalted place which is now universally assigned to him in the history of astronomical discovery, if he had not at the same time been endowed with a mind of rare originality and power, combined with a strong turn for speculation.

For many years after the invention of the achromatic telescope, the manufacture of flint-glass continued to be confined to England, which country, in consequence, enjoyed an exclusive privilege in the construction of instruments of this description. At length Guinand, a humble mechanic of Brenetz, a small village in the Canton of Neufchatel, Switzerland, having turned his attention to the manufacture of flint-glass towards the close of the eighteenth century, succeeded, after a long course of persevering efforts, in producing masses of that substance perfectly free of striæ, and therefore adapted for the construction of object-glasses of telescopes. Frauenhofer*, the Bavarian optician, having been made acquainted with the wonderful success of Guinand, resolved to take advantage of the circumstance, and with a view to this object he induced the Swiss artizan to remove to Munich, in the year 1805. This eminent individual, who was no less remarkable for sagacity in philosophical enquiries than for skill as an artist, possessed peculiar qualifications for profiting by the instructions of Guinand, and he soon succeeded in attaining unexampled excellence in the construction of achromatic object-glasses. Telescopes were now executed by Frauenhofer, the apertures of which far exceeded anything hitherto known since the discovery of achromatism. It was with one of these instruments, having an aperture of 9.9 inches, and a focal length of 13½ feet, that M. Struve made the series of micrometrical measurements of double stars at Dorpat, between the years 1824 and 1837, which have rendered the name of that astronomer so deservedly famous. With another of them, having an aperture of 12 inches, and a focal length of 18 feet, M. Lamont, of Munich, made those interesting observations of the satellites of Uranus, to which allusion has been made in one of the preceding chapters. Frauenhofer was contemplating the execution of object-glasses of still greater magnitude than either of those above-mentioned, when his brilliant career was unfortunately brought to a close by a premature death. His successors, MM. Merz and Mahler, have succeeded in effectually realising his views. Among the *chef-d'œuvres* of these artists may be cited the famous refractors of Pulkowa and Cambridge, U.S., to both of which instruments allusion has already been made in the foregoing part of this work.

Guinand remained at Munich from 1805 till 1814, in which year he finally returned to his native country. A few years afterwards he was visited by Lerebours, an eminent French optician, who purchased of him all the flint-glass in his possession. Subsequently he also supplied Cauchoix, another artist of Paris, with portions of the same valuable substance. The French opticians skilfully worked the material into object-glasses, and in this manner refracting telescopes came to be constructed in France, rivalling the most finished productions of the Munich artists. England, which continued long to be the exclusive seat of the manufacture of achromatic telescopes, had the mortification of finally seeing both Germany and France completely outstrip her in this branch of practical optics. This result she owed to the short-sighted policy of the Government, which had placed an exorbitant duty on the manufacture of flint-glass. The removal of this pernicious impost a few years since, has given a new impulse to the art, and already results have been achieved which seemed to indicate that before a long period shall have elapsed, refracting telescopes will be executed in this country, rivalling the most powerful instruments of the same kind which have emanated from the workshops on the Continent*.

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* Joseph Frauenhofer was born in the year 1787, at Straubing, in Bavaria. Besides having attained unrivalled eminence in his professional vocation, he achieved many important discoveries in physical optics. He died in 1826, while yet in the prime of life; but he left behind him an immortal reputation

While the refracting telescope has been rendered vastly more efficient in modern times, the reflecting telescope has received a corresponding degree of improvement. During the present century several very large telescopes were constructed upon this principle by the late Mr. Ramage, of Aberdeen, an individual of considerable originality, who possessed a remarkable aptitude for mechanical inventions. The perfection of these instruments does not seem, however, to have been commensurate with their magnitude, for they have been employed only to a very limited extent in astronomical observations. In more recent times, Mr. Lassell, of Liverpool, has especially distinguished himself by his skill in the construction of reflecting telescopes. With a Newtonian reflector of 2-feet aperture, which he executed with his own hands, he has discovered the single satellite of Neptune, and the eighth satellite of Saturn; while, at the same time, he has been enabled by its use, to make several very interesting physical observations of the planetary bodies. In the construction of reflecting telescopes the Earl of Rosse has attained a degree of excellence which far surpasses all previous efforts of the kind. It is impossible here to give any account of the multitude of admirable contrivances by means of which that distinguished nobleman has succeeded in bringing his telescopes to so high a state of perfection. It must suffice to state, that in 1840, he completed a Newtonian reflector of 3 feet aperture, which he subsequently employed in astronomical observations. Phenomena of a highly-interesting nature were soon disclosed by the use of this powerful instrument. Several nebulæ, hitherto observed as such in the most powerful telescopes, were resolved into clusters of stars, while others exhibited forms totally different from those which had been hitherto assigned to them. In 1845 Lord Rosse gave to the world a still more striking proof of his practical talents, and his devotion to astronomical science, by the construction of a reflecting telescope of 6-feet aperture, and 54-feet focal length! This magnificent instrument, by far the most powerful which the genius of man has hitherto executed for the purpose of exploring the grand phenomena of the heavens has already, in the hands of its noble owner, done valuable service to astronomy, by the light which it has thrown upon the structure of the nebular part of the universe†.

The brilliant success which has attended Lord Rosse's efforts to construct reflecting telescopes, has suggested to several eminent scientific men of the present day the expediency of transporting a powerful instrument of this description to the southern hemisphere, for the purpose of employing it in physical observations of the celestial bodies. At the meeting of the British Association, which was held at Edinburgh in 1850, it was agreed to memorialise the Government upon the advantages which would accrue to astronomical science from the practical adoption of such a scheme. It is to be hoped that, ere long, steps will be taken towards its complete realisation.

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* At the meeting of the British Association, held in the month of July of the present year (1851), at Ipswich, the Astronomer Royal, in his opening address as President of the Association, stated that Mr. Simms had completed the flint-glass for an achromatic object-glass of 13 inches in diameter, and that he was engaged in executing one of 16 inches. He also mentioned that Mr. Ross was attempting to make an object-glass of 2 feet in diameter! It is to be hoped that these glasses may turn out to be remarkable for quality as well as size.

† Dr. Robinson, in a brief account of Lord Rosse's optical labours, delivered at the meeting of the British Association, held at Cork in 1843, has remarked that "between the spherical and parabolic figures, the extreme difference is so slight, even in the telescope of 6-feet aperture, that if the two surfaces touched at their vertex, the distance at the edge would not amount to the $\frac{1}{10,000}$th of an inch, a space which few can measure, and none without the microscope." This statement may give the reader some idea of the exquisite delicacy of the operation necessary for giving the speculum its true parabolic figure.

CHAPTER XXI.

Origin of Stellar Astronomy.—Physical Changes observed in the Starry Regions.—Disappearance of Stars from the Heavens.—New Stars.—Stars of Variable Lustre.—Photometric Researches on the Stars.—Attempts to Determine their Apparent Diameters. —Space-penetrating Power of Telescopes.—Applied to ascertain the relative Distances of the Stars. —Absolute Distances of the Stars determined by Photometric Principles.—Parallax of the Fixed Stars.—Early Researches on the Subject.—Modern Researches.—Bessel.—Henderson.—Struve.—Peters.—Proper Motions of the Stars.—Motion of the Solar System in Space.—Double Stars.—Discovery of their Physical Connexion by Sir William Herschel.—Methods for determining the Elements of their Orbits.—Nebulæ.—Speculations of Sir William Herschel.—Modern Researches on the Subject.—Sir John Herschel.—The Earl of Rosse.—Early Speculations on the Milky Way.—Theory of Wright.—Observations of Sir William Herschel. —Speculations of that Astronomer on the breaking up of the Milky Way.—Researches of Struve on the Distribution of the Stars in Space.—Gauges of Sir John Herschel in the Southern Hemisphere.—Speculations of M. Struve on the Extinction of Light in its Passage through Space.

The aspect of the starry firmament furnishes one of the most glorious spectacles which nature displays throughout the entire range of her widely-diversified empire. The multitude of luminaries with which the blue vault of heaven appears bespangled, the endless variety of lustre which they exhibit, the striking configurations which they form, and the calm regularity with which they pursue their nocturnal courses—all conspire together in awakening a feeling of adoration in the most listless spectator, while at the same time they are eminently calculated to excite the enquiries of the more rational observer of physical phenomena. Attention could not long have been directed to the appearance of the starry heavens, before it was discovered that, except in a few instances, the entire multitude of luminaries constantly maintained the same relative position. Hence they received the appellation of *Fixed Stars*, an expression, however, which is now found to be inaccurate, since the researches of modern astronomers have served to demonstrate that many of the stars are affected by very minute movements, while at the same time it can hardly be doubted that they are all subject to a similar influence.

During the early period of astronomical science, the fixed stars were observed merely on account of their utility in forming reference points, by means of which the apparent positions of the planetary bodies might be determined with greater accuracy, and the laws of their movements hence ascertained. Indeed, the Ptolemaic astronomy, by making the whole sphere of the stars perform a complete revolution round the earth once in every twenty-four hours, assigned to those luminaries a very subordinate place in the physical universe. When the immortal Copernicus restored the true system of the world, the stars assumed their just dignity as vast bodies placed at an immeasurable distance from the earth; but it seemed hopeless ever to arrive at any knowledge respecting their physical constitution. At length the invention of the telescope, about the beginning of the seventeenth century, by removing the veil which had so long concealed those remote orbs from the scrutinies of the human mind, gave an unexpected impulse to researches of this nature. From the epoch of Galileo's brilliant discoveries in the celestial regions, may be said to date the origin of Stellar Astronomy, a branch of enquiry which has ever since attracted more or less attention, and which, in recent times especially, has excited an intense degree of interest.

That the stars are so many suns shining by their own light, is an opinion which seems to have been entertained by some of the ancient philosophers. This view of their nature came to be universally adopted upon the establishment of the true system of the world by Copernicus, since it was considered to be improbable in the highest degree, that bodies shining by reflected light would be visible at the earth, when at the same time their distance was so immense, that they did not exhibit any sensible change of position even when viewed at the opposite extremities of a diameter of the terrestrial orbit.

The number of stars visible to the naked eye, is found to amount only to a few thousands; for although at first sight they seem to be innumerable, yet a closer scrutiny of the heavens soon serves to convince the observer that he has been labouring under a delusion in this respect. But what the unaided organ of vision merely suggests to the imagination, without affording any substantial grounds of belief in its existence, an examination of the starry heavens with the telescope has proved to be a reality. Multitudes of stars are then perceived, which were invisible to the naked eye. The number also thus disclosed to view, continually augments with each successive increase of the optical power of the instrument, until the imagination is at length absolutely overwhelmed with the countless myriads of suns, and systems of suns, which are found to people the immensity of space.

The ancient philosophers supposed the celestial bodies to be essentially incorruptible and eternal, and hence maintained that they were not subject to physical changes, as in the case of bodies at the surface of the earth. The progress of astronomical science, however, has served amply to demonstrate that those brilliant orbs are not exempt from the great law of mutation, to which all the other objects of the created universe appear to be liable. Exclusively of the evidence afforded by the observations of astronomers on the physical constitution of the sun, there are various stellar phenomena of a highly-interesting nature, which concur in establishing this important fact. A brief allusion to some of them may not perhaps be unacceptable to the reader.

In some instances, stars which have shone for ages in the firmament, have ceased to be visible. A comparison of catalogues of the stars constructed for different epochs, would indeed seem to indicate that a great number of such objects that were formerly visible in the celestial sphere, have totally vanished; but it is probable that in most of these cases the discordance is attributable to the imperfection of the earlier observations of astronomers. Stars, however, have been mentioned by Montanari*, Maraldi †, and Sir William Herschel ‡, respecting whose extinction there hardly exist grounds for entertaining any reasonable doubt.

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* Phil. Trans., 1671, p. 2202.

† Mém. Acad. des Sciences, 1709, p. 40.

‡ Phil. Trans., 1792, p. 26.

Various new stars are recorded in history, which after having shone with great splendour for a short time, then gradually faded away until they ceased to be visible. Thus Pliny asserts, that the appearance of a new star in the time of Hipparchus, about the year 130 A.D., was the occasion of inducing that astronomer to construct his famous catalogue of the stars. A new star is said to have appeared in the reign of the Emperor Honorius, about the year 390 A.D.; also one in the reign of the Emperor Otho, about the year 945; and again a phenomenon of a similar nature in the year 1264.

In the year 1572, there appeared a splendid new star in the constellation of Cassiopeia, which is memorable for being the earliest of such phenomena respecting which any authentic particulars have been recorded by astronomers. This star was observed by Tycho Brahé, who, in a long work, written expressly on the subject, has given a detailed account of the various circumstances connected with its appearance; including a critical discussion of the speculations of his contemporaries on its probable origin. It was first seen by him on the evening of the 11th of November, 1572. It then surpassed in lustre the brightest of the fixed stars, and was even more brilliant than the planet Jupiter, although then in opposition and near perihelion. It almost rivalled Venus, and, like that planet, was seen by some persons even in the daytime. During the remaining part of November, it continued to shine with undiminished lustre; but it subsequently began to decline, and it gradually grew fainter, until at length, in the month of March, 1574, it ceased to be visible. The colour of this extraordinary object underwent a succession of changes during the period of its appearance. When it first became visible, it shone with a bright white light, like Venus or Jupiter. It then acquired a yellowish tinge; afterwards became ruddy, like Mars or Aldebaran; and finally exhibited a leaden hue, like the planet Saturn *. Tycho Brahé supposed it to be generated from the ethereal substance of which he imagined the Milky Way to be composed, and to have been afterwards dissipated by the light of the sun and the other stars, or to have dissolved spontaneously from some internal cause †. A more modern hypothesis has referred the origin of the phenomenon to some vast combustion, an explanation which receives some degree of support from the gradual change of colour which the light of the star exhibited.

A few years after the close of the sixteenth century, another splendid new star burst forth in the constellation of Serpentarius. In this instance the phenomenon was witnessed by many eminent astronomers, including Kepler, who wrote an interesting dissertation on the subject of its appearance ‡. It was first seen by that astronomer on the 17th of October, 1604. It surpassed in brightness the stars of the first magnitude, as well as the planets Mars, Saturn, and Jupiter, all of which were in its vicinity. Like the star of 1572, it began to decline soon after its appearance, and finally ceased to be visible between October, 1605, and February, 1606. Kepler was of opinion that it was generated from an ethereal substance, not confined exclusively to the region of the Milky Way, as Tycho Brahé had supposed in the case of the star of 1572, but pervading all space. In connexion with this explanation of the origin of the star, he remarked, that the luminous ring observed around the dark body of

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* Progymnasmata, p. 297. † Ibid., p. 795.

‡ "De Stella Nova in Pede Serpentarii," 4to, Pragæ, 1606.

the moon at Naples during the total eclipse of the sun which happened in the year 1605, was occasioned by the existence of such a substance around the sun *.

The new stars of 1472 and 1604, are memorable in the history of astronomy, for the extraordinary splendour with which they suddenly burst forth upon the world. Several phenomena of a similar nature have subsequently been observed, but they have not exhibited such remarkable features as the stars just mentioned.

A comparison of the writings of the ancient astronomers with the results of actual observation, would seem to indicate that some stars have undergone a permanent change of brightness. Besides these, however, there are several stars which have been recognized in modern times to be subject to periodic variations of lustre, completing the cycle of their phases in comparatively short intervals of time. The most celebrated of these is the star in the neck of the Whale, which Bayer has marked in his charts with the Greek letter *omicron*, and which, in consequence of the singular changes exhibited by it, was termed *Mira Ceti*. This star was first seen by David Fabricius, on the 13th of August, 1596 †; and as it disappeared in the following October, it was considered by Kepler and the other astronomers of the time to have been a new star, like the one of 1572. It was first recognised as variable by Phocylides, a Dutch philosopher, who commenced his observations of it in the year 1638 ‡. Bouillaud, by a comparison of all the observations included between the years 1638 and 1667, ascertained that its variations are periodic, and that it passes through the course of its phases in about 333 days. The more accurate researches of modern astronomers make the period to be $331^{d}\ 15^{h}\ 7^{m}$ §. This star is generally invisible to the naked eye for about five months. It then gradually increases until it attains the brightness of a star of the second magnitude; and after remaining in this state for about fifteen days, it begins to decline in brightness, diminishing nearly at the same rate as that at which it had formerly increased.

A great number of stars have been recognised by modern astronomers to be subject to variations of brightness similar to those remarked in the case of ο Ceti. One of the most interesting of these is the bright star in the head of Medusa, termed Algol or β Persei. Montanari, and afterwards Maraldi, found that it was subject to singular fluctuations of brightness; but no attempt was made to ascertain either the period or law of variation, until Goodricke commenced his observations of the star in the year 1782. By continuing for some time to watch its successive phases of brightness, that acute observer found that, after resembling a star of the second magnitude, for about two days thirteen hours and three quarters, it descended in the short space of three hours and a half to the fourth magnitude, and then in an equal interval of time regained its former brightness, thus completing the cycle of its variations in about two days twenty hours and three quarters ‖. By a subsequent comparison of an observation of the star by Flamsteed, who in the year

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* Kepler, *De Stella Nova*, cap. xxiii. p. 115.

† Ibid., cap. xxii., p. 112.

‡ Hevelius, *Historiola Miræ Stellæ*, p. 147, fol., Gedan, 1662.

§ The researches of Bouillaud on the subject appeared in a small tract, entitled " Ad Astronomos mo nita duo," Paris, 1667.

‖ Phil. Trans., 1783, p. 474.

1696 noted it as a star of the third magnitude, with a corresponding observation of his own, he determined the period more accurately to be $2^{d}\ 20^{h}\ 48^{m}\ 56^{s}$*.

Another interesting star, the discovery of whose variability is due to Goodricke, is *β Lyræ*. It was first recognised by him as subject to changes of brightness in the year 1784. He was originally induced to fix the period of variation at $6^{d}\ 9^{h}$; but after continuing his observations for some time, he found that, although the course of the phases very nearly agreed with this period, an *exact* compensation of brightness did not take place in less than $12^{d}\ 19^{h}$, there being in reality two maxima and two minima. When it attains its maximum brightness, it is equal to a star of the third magnitude. At one of its minima, it appears between the fourth and fifth magnitude, and at the other between the third and fourth †. These interesting particulars have been fully confirmed by the recent researches of M. Argelander.

It may be remarked respecting variable stars, that in passing through their successive phases, they are subject to sensible irregularities, which have not hitherto been reduced to fixed laws. In general they do not always attain the same maximum brightness, their fluctuations being in some cases very considerable. Thus, according to Argelander, the variable star in *Corona Borealis*, which Pigott discovered in 1795, exhibits on some occasions such feeble changes of brightness, that it is almost impossible to distinguish the maxima from the minima by the naked eye; but after it has completed several of its cycles in this manner, its fluctuations all at once become so considerable, that in some instances it totally disappears. It has been found, moreover, that the light of variable stars does not increase and diminish symmetrically on each side of the maximum, nor are the successive intervals between the maxima exactly equal to each other.

Several persons have endeavoured to account for the phenomena of variable stars, but no satisfactory view of the subject has yet been arrived at by any enquirer. Bouillaud sought to account for the variations of ο Ceti, by attributing to the star a rotation round a fixed axis, and supposing the greater part of its surface to be obscure. According to Maupertius, some stars, by whirling rapidly round their axes, became flattened to such an extent as to resemble millstones: they were, moreover, liable to periodic perturbations from the action of opaque bodies revolving round them. Hence, when one of such stars presented its flattened surface to the earth, it shone with its maximum brightness, and when seen edgewise, its lustre was manifestly a minimum. Goodricke suggested that the variations of Algol might perhaps arise from the periodic transit of an opaque planet revolving round the star. Pigott assimilated the phenomena of variable stars to the solar spots, and this view of the subject is beyond doubt the most satisfactory that has yet been advanced.

The Photometry of the stars, although forming one of the most important subjects of Stellar Astronomy, has hitherto remained in a very imperfect condition. The usual mode of designating the brightness of the stars by arranging them according to different magnitudes, is both vague in theory and contradictory in practice. It is vague, inasmuch as the place of a star in the scale of magnitudes conveys no definite idea of the quantity of light emitted by it relative to any other star whose place is assigned in the scale; and it is contradictory, inasmuch as in numberless instances the same star has a different magnitude assigned to it by different authorities. Bailly proposed to amend the system of stellar nomenclature by determining the brightness of each star upon strict photometrical principles. To this end he suggested that all the stars should be observed with the same telescope, and that in every instance the aperture should be diminished until the star just ceased to be visible *. It is obvious that the light usually emitted by any star to the earth would then be inversely proportional to the corresponding aperture. This mode of determining the relative brightness of the stars is unexceptionable in point of theory, but practical difficulties have hitherto stood in the way of its adoption as the basis of a new system of nomenclature.

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* Phil. Trans., 1784, p. 289.

† Phil. Trans., 1785, p. 153, et seq.

Sir William Herschel rejected the system of magnitudes altogether, as unworthy of any reliance, and determined the relative brightness of a great number of stars visible to the naked eye, by comparing each star with the other stars in its vicinity, and noting those which were sensibly equal to it in lustre, or differed from it only in a very small degree. The results of his labours on this occasion form a valuable record for ascertaining at some future epoch whether any of the stars to which they refer have undergone a variation of lustre †.

More recently Sir John Herschel has employed a similar method in connecting a great number of stars distributed over both hemispheres in one unbroken chain of relative brightness, and has also succeeded in adapting the results to the conventional system of magnitudes so as to represent the brightness of the various stars according to the usual nomenclature, but in a vastly-improved condition as respects accuracy of detail. The same distinguished astronomer has moreover determined, by a series of photometrical experiments, the quantity of light actually emitted by each star relative to a certain standard star considered as the unit of light, and has instituted a comparison between the results and the corresponding magnitudes of the stars. The conclusion to which he was conducted by his researches on this occasion, is at once curious and instructive. It appeared that if the conventional magnitudes of the stars as rectified by him, were all increased by the common fraction 0·414, the squares of the resulting magnitudes would then be inversely proportional to the corresponding photometric numbers, representing the light of the stars as determined by experiment. Now this is precisely the relation that would subsist between the distances of the stars from the earth, and their respective photometric intensities, on the probable supposition that they all yield the same quantity of light at the same distance. It follows, therefore, that the ordinary nomenclature of magnitudes, when sufficiently amended, represents pretty nearly the order of the distances of the stars, and would even rigorously do so, provided the magnitudes were augmented by the common fraction above mentioned ‡. An example will illustrate this curious conclusion more clearly. The bright star *α Centauri* being assumed as the standard unit in the scale of conventional magnitudes, the star *Antares*, according to Sir John Herschel, will be represented in the same scale by 1·6. Now if the distance of *α Centauri* from the earth be at the same time assumed as the unit of distance, it would follow from the photometric experiments of the astronomer just mentioned, that the distance of *Antares*, as deducible from the quantity of light which it emits, would be represented by 2·014, which is equal to 1·6 + ·414. Consequently, if the nomenclature of the stars was so modified that the magnitude of *Antares*, as it stood in the old scale, was increased by ·414, the resulting magnitude would then afford an accurate representation of the relative distance of the star; and the same may be said of all the other stars whose conventional magnitudes have been rectified by Sir John Herschel[*].

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* Mém. Acad. des Sciences, 1771, p. 580.

† See *Phil. Trans.*, 1796, 1797, 1799.

Hooke appears to have been the first person who considered the mode in which the visibility of the stars depends upon the telescope. In his *Micographia* he remarks that the number of stars visible in a telescope will increase with the enlargement of the aperture, for, by uniting a great number of rays into one point, many stars, which from their faintness would otherwise be invisible, are thereby brought into view, and rendered conspicuous[†]. In 1717, J. Cassini alluded to the same subject in a communication to the Academy of Sciences, on the parallax of Sirius. He remarked that the stars of the sixth magnitude are six times more remote than those of the first, and hence he concluded, that with a telescope magnifying 200 times, it would be possible to perceive stars that were 1200 times more remote than those of the first magnitude[‡]. The visibility of the stars is here made to depend wholly upon the magnifying power of the telescope. That some obscure connexion really does exist between the two principles appears evident from the observations of subsequent astronomers[§], but it has been no less unequivocally established that the visibility of the stars mainly depends upon the magnitude of the aperture.

Lambert, in his "Cosmological Letters," accounts for the power of the telescope to render small stars visible by the superior precision of the image which it forms, when compared with that formed by the naked eye. He makes no allusion whatever to the magnitude of the aperture, but he expressly asserts that the visibility is independent of the magnifying power. Michell, in the year 1767, took a more correct view of the subject, making the visibility to depend on the magnitude of the aperture. Assuming the diameter of the pupil of the eye to be equal to one-third of an inch, and roughly estimating the quantity of light lost in passing through the telescope, he was enabled to compare the distance of the faintest stars seen in a telescope of given aperture, with the distance of those barely visible to the naked eye [*].

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[*] The researches of Sir John Herschel do not extend to stars lower than those of the fourth magnitude. It would appear from a recent paper by Mr. Dawes (*Mo. Proc. Ast. Soc.*, June, 1851), that the nomenclature of *telescopic* stars, as indicated by the observations of Lalande, Bessel, and Argelander, is based upon a different principle.

[†] See the work cited, p. 241.

[‡] Mém. Acad. des Sciences, 1717, p. 260.

[§] Thus Sir William Herschel, in one of his earlier communications to the Royal Society, cites several instances of stars which could not be seen with a magnifying power of 227, but which became distinctly visible when a power of 460 was applied (*Phil. Trans.*, 1782, p. 92). Mr. Dawes has recently remarked that the aperture necessary to render the stars steadily visible to him in a telescope magnifying sixteen times, is *less* than the pupil of his eye, being only ·15 inch in diameter, whereas he estimates that of the pupil of his eye at ·25 inch (*Mo. Pro. Ast. Soc.*, June, 1851). Now in such a case it might be expected that the aperture of the telescope would be *greater* than the pupil of the eye, in order that a compensation might be effected for the light necessarily lost in telescopic observation. Is not this apparent anomaly due to the influence of the magnifying power, which, under certain circumstances, is favourable to the visibility of the object, as in the instances above alluded to?

The subject of the visibility of the stars has been fully considered by Sir William Herschel, in an admirable paper which appears in one of the volumes of the *Philosophical Transactions* [†]. By a series of photometrical experiments, he determined the quantity of light lost in telescopes of different forms of construction. Assuming, moreover, that the pupil of the eye was equal to two-tenths of an inch in diameter, he compared the distance at which a star would be barely visible in a telescope of given aperture with the distance of the smallest stars visible to the naked eye, considered as the linear unit. The former of these distances was termed by him the space-penetrating power of the telescope. Herschel, in accordance with these principles, determined the space-penetrating powers of various telescopes, differing either in construction or aperture, which he employed in his observations.

Since the brightness of a star seen in a telescope depends upon the magnitude of the aperture, it follows, that by gradually diminishing the latter, the image of a star may be reduced to any degree of faintness we please. Hence, if two stars of unequal brightness be observed with two telescopes exactly similar in all respects, and if the aperture of the telescope directed to the brighter star be reduced until both stars appear of the same brightness, it is manifest that the absolute quantity of light emitted by each star to the earth, will be in the inverse ratio of the aperture of the telescope through which it is thus seen. Now, knowing the relative quantities of light emitted by the two stars, their relative distances may be readily determined, supposing, as before, that both stars possess the same degree of intrinsic brightness. Sir William Herschel applied these principles to the determination of the relative distances of the stars [‡] and clusters of stars [§] visible in his powerful telescopes. It is impossible within the limits to which we are confined, to give any account of the sublime results to which he was conducted by his labours on this occasion.

The question with respect to the absolute distance of the stars from the earth, is one which has excited a high degree of interest among astronomers ever since the re-establishment of the true system of the world by Copernicus. This might be ascertained in any case by a simple and direct process, if the star exhibited a change of position depending on the motion of the earth in her orbit, but such a parallactic displacement was long found to be insensible. Another mode of effecting the same object was founded upon a knowledge of the apparent diameter of the star. Assuming the star to be equal in absolute magnitude to the sun, it is clear that the distance of the sun from the earth would be to the distance of the star from the same body, as the apparent diameter of the star to the apparent diameter of the sun. This mode of determining the distance of a star was less satisfactory than that founded upon a knowledge of its parallax, seeing that it involved an arbitrary assumption with respect to the magnitude of the star; but still it was exceedingly desirable to arrive at some probable conclusion upon the subject. It was soon found, however, that the measurement of the apparent diameter of a star was an object of as great delicacy as the detection of its parallax. The earlier astronomers, indeed, supposed that the principal stars possessed an apparent magnitude of 2′ or 3′[*]; but the telescope shewed this to be to a great extent an illusion, produced by the false light surrounding the star. Galileo attempted, by a very ingenious method, to get rid of the effects of irradiation in measuring the apparent diameter of α Lyræ, and he so far succeeded in his object as to assign to the star an apparent diameter of only 5″[†]. This was undoubtedly a much nearer approximation to the truth than any estimate that had been hitherto formed of the apparent magnitude of a star.

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[*] Phil. Trans., 1767, p. 234, et seq.

[†] Ibid., 1800, p. 49, et seq.

[‡] Ibid., 1817, p. 302, et seq.

[§] Ibid., 1818, p. 429, et seq.

Horrocks first remarked a phenomenon which furnishes a striking proof of the extreme smallness of the apparent diameters of the stars. In company with his friend Crabtree, he witnessed an occultation of the Pleiades by the moon, on the evening of the 19th of March, 1637. As soon as the stars approached in succession the dark limb of the moon, they were in each instance observed to vanish instantaneously[‡]. Horrocks justly asserted, that if the whole of the light in each case had emanated directly from the body of the star, the latter ought to have disappeared by sensible gradations. He therefore concluded that the apparent diameters of the stars are mere points which are not capable of measurement.

Hevelius, by diminishing the aperture of his telescope, succeeded in giving a round planetary appearance to the stars, and he determined the apparent diameters of the resulting disks by comparing them in succession with Mercury, whose apparent diameter he had previously ascertained from observations of its transit across the sun's disk. In this manner he found the apparent diameter of Sirius to be 6″ 21‴; that of Procyon, 4″ 58‴, &c., &c.[§]. In 1717, J. Cassini observed Sirius with a telescope, the aperture of which he had similarly reduced, and by comparing the round disk thus formed with the planet Jupiter, he concluded that the apparent diameter of the star did not exceed 5″[‖]. Both Hevelius and Cassini erred in supposing that the round appearance of stars, seen in telescopes with reduced apertures was real. The phenomenon is, in fact, a spurious image produced by the diffraction of the light in passing the contour of the aperture.

Halley did not fail to express his suspicion that the apparent diameter ascribed by J. Cassini to Sirius was an optical fallacy, occasioned by the great contraction of the aperture of the object-glass[¶]. From the instantaneous disappearance of stars on the occasion of their occultation by the moon, he concluded, as Horrocks had already done, that the apparent diameters of the stars is excessively small. He considered that the apparent diameters of Spica Virginis and Aldebaran were in both cases less than 1″. Michell supposed that the diameter of Sirius was even less than 0″.02. We thus see that the progress of research on the subject tended invariably to assign a less and less apparent diameter to the stars.

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[*] Tycho Brahé estimated the apparent diameters of the stars of the first magnitude at 2′; those of the second at 1½′; those of the third at 1$\frac{1}{12}$′; those of the fourth at ¾′; those of the fifth at ½′; and those of the sixth at ⅓′. (*Progymnasmata*, p. 482.)

[†] Opere de Galileo, tome iv., p. 259.

[‡] "Venus in Sole Visa," p. 139. The merit of having originally deduced from this phenomenon a proof of the extreme smallness of the apparent diameters of the stars has been erroneously ascribed by some writers to Halley, upon the strength of a remark made by that philosopher, in 1718, relative to the same subject. (*Phil. Trans.*, 1718, p. 853.)

[§] "Mercurius in Sole Visus," p. 92. Hevelius states on this occasion that he saw Sirius with the naked eye, on the morning of October 2, 1661, *when the sun had already ascended above the horizon.*

[‖] Mém. Acad. des Sciences, 1717, p. 258.

[¶] Phil. Trans., 1720, p. 3.

Sir William Herschel devoted considerable attention to the subject of the apparent diameters of the stars, but he was unable to effect any measurement on which he could rely with sufficient confidence. As might be expected, he made the real apparent magnitudes of the stars to be less than any other astronomer had hitherto determined them to be from observation. On the 22nd of October, 1781, he observed the bright star α Lyræ, with a power of 6450, and, having measured the apparent diameter with the micrometer, he found it amount to 0″.3553 *. It may not be uninteresting to compare the absolute magnitude which this result would assign to the star, with the absolute magnitude of the sun. To this end it may be remarked that the parallax of α Lyræ, as determined in recent times by M. Struve, amounts to 0″.261. Its distance from the earth exceeds, therefore, the radius of the terrestrial orbit in the proportion of 790,283 to 1. Hence, if we suppose the sun to be transported to this distance, his apparent diameter (estimated at the mean value of 32′) would be diminished in the same proportion, and would therefore be equal only to 0″.0024. Now the apparent diameter of α Lyræ, as determined by Herschel, exceeds this quantity in the proportion of 148 to 1. It follows, therefore, either that the result obtained by Herschel on this occasion differs widely from the true value, or that α Lyræ vastly exceeds the sun in absolute magnitude. It can hardly be doubted that the physical anomaly which here presents itself, is mainly due to an erroneous determination of the apparent diameter of the star. Indeed, as has been already remarked, Herschel does not appear to have reposed any confidence in such measurements, being of opinion that the round appearance of the stars, even when seen in the most perfect telescopes, was almost wholly spurious.

When the stars are observed with telescopes of great optical perfection, furnished with high magnifying powers, they are generally found to exhibit a well-defined planetary appearance, even although the aperture should not be reduced. There is also visible around the disk of the star an alternate succession of dark and bright rings of uniform breadth. It has been already mentioned that the round disk is a spurious phenomenon, occasioned by the diffraction of light. The rings have also been satisfactorily explained by the same principle. It is important to remark, that these phenomena would be produced in greatest perfection if the star was merely a physical point of light. Mr. Airy, by a mathematical investigation, has rigorously accounted for the appearance of the disk and surrounding rings, upon the principle of the mutual interference of the rays of light, and has also explained a part of the phenomenon which had hitherto seemed very obscure, namely, the dependence of the magnitude of the disk on the intensity of the light of the star †.

The parallax of the fixed stars having been found for a long time to be insensible, attempts were made to determine their distances from the earth by photometrical principles. Assuming the stars to be equal to the sun, both in magnitude and intrinsic splendour, it is clear, since light diminishes in the inverse ratio of the square of the distance, that the light of the sun will be to the light of any of the stars, as the square of the distance of the star from the earth to the square of the distance of the sun. Hence, if the quantity of light sent to us by the star be determined relatively to the light of the sun, its distance from the earth

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* Phil. Trans., 1782, p. 147.

† Camb. Phil. Trans., vol. v., p. 283.

may be ascertained by a simple process of arithmetic. James Gregory appears to have first suggested this mode of determining the distance of a star from the earth. Huyghens, by applying it to Sirius, found that the distance of that star from the earth exceeded the distance of the sun 28,000 times. The same method subsequently attracted the attention of various astronomers, among whom may be mentioned Chésaux, Lambert, Michell, and Olbers, who all agree in assigning to the stars of the first magnitude a parallax less than 0″.5, as the result of its application.

Wollaston found that the light of Sirius appeared equal to that of the sun when reflected from the surface of a sphere $\frac{1}{10}$th of an inch in diameter, and seen at the distance of 210 feet. Supposing the half of the solar light to be lost during reflexion, he hence concluded that the light of the sun was to that of the star as 20,000 millions to one. Hence, if we assume the star to be equal in magnitude and intrinsic splendour to the sun, this result would imply that the distance of the star from the earth is 141,421 times greater than the distance of the sun, whence its parallax would amount to 1″.8[*]. It has been ascertained, however, from the observations of astronomers, that the parallax of Sirius is in reality less than half a second. The conclusion therefore is, that in magnitude the star greatly surpasses the sun.

Wollaston found by a similar method, that the light of the sun was to the light of α Lyræ, as 180 thousand millions to one, and consequently that the light of α Lyræ was only one-ninth of that of Sirius. This would imply that α Lyræ was three times more distant than Sirius, and that its parallax was therefore only 0″.6. Even this, however, considerably exceeds the true value as recently determined by M. Struve.

It has been already mentioned, that the attempts of the earlier astronomers to detect an annual variation in the apparent places of the stars, depending on the motion of the earth in her orbit, was not attended with any success. Copernicus attributed the absence of all sensible indication of parallax to the immense distance of the stars compared with the radius of the terrestrial orbit. Tycho Brahé, although possessing instruments of much more perfect construction than any hitherto employed for the purposes of astronomical observation, was unable to detect the slightest trace of an annual displacement in any of the stars. On the other hand, he estimated the stars of the first magnitude to have an apparent diameter of about 2′ or 3′. He asserted, therefore, that if the earth really moved round the sun, it would necessarily follow that the stars of the first magnitude exceeded in dimensions the whole amplitude of the terrestrial orbit, a conclusion, which appeared to him to be palpably absurd, and fatal to the Copernican theory of the universe.

Galileo suggested a mode of investigating the parallax of the fixed stars, which has received more than one successful application in recent times. He remarked, that in those instances wherein two stars of unequal magnitudes are apparently very near to each other, it may be presumed that the smaller of the two stars, appears so only from its being more distant, and therefore that its parallax may be considered as insensible relative to the parallax of the larger star. He imagined, therefore, that by continuing to observe the position of the larger star with respect to the smaller, throughout the course of the year, the parallax of the larger star might be detected[†]. This method obviously afforded a more favourable chance of detecting the parallax of a fixed star, than that founded on observations of the absolute position of the star in the celestial sphere. It does not appear that Galileo attempted on any occasion to reduce it to practice.

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[*] Phil. Trans., 1829, p. 19, et seq.

[†] Opere di Galileo, tome iv., p. 272.

Hooke first employed the telescope in observations for the purpose of detecting the annual parallax of the fixed stars. In order to avoid the effects of refraction, he confined his observations to the star γ *Draconis*, which passes near the zenith of Gresham College, London, where his observatory was established. From four observed zenith distances of the stars, as determined by him in the months of July, August, and October of the year 1669, with a zenith sector 36 feet long, he concluded that the star had an annual parallax, amounting to somewhere between 27″ and 30″*. It is to be remarked, however, that Picard, a few years afterwards, observed the altitude of α Lyræ at both solstices, but was unable to detect the slightest trace of parallax†.

Previous to the discovery of the Aberration of Light, the displacement due to that principle exhibited itself in the observations of several astronomers, who, in some instances, were induced erroneously to attribute it to the effects of parallax. In all probability the variations in the apparent place of γ *Draconis*, observed by Hooke, proceeded from this cause. This was certainly the case with respect to Flamsteed, who, from a series of observations of the zenith distance of the polar star, made by him at Greenwich with the mural arc, between the years 1689 and 1697, concluded that the star possessed an annual parallax equal to at least 40″‡. J. Cassini shortly afterwards remarked, with justness, that the displacement indicated by the observations of Flamsteed, was incompatible with the supposition of its being produced by parallax§. It has been already mentioned, that the variations in the position of the pole star, detected by Flamsteed on this occasion, were, in reality, the results of aberration, the maximum value of which is deducible with an astonishing degree of accuracy from the observations of that astronomer‖. It may be mentioned, that, about the same time, Wallis suggested observations of the greatest azimuth, east or west of a circumpolar star, as favourable for the detection of parallax, since the effect of refraction would thereby be completely avoided¶. It does not appear that any astronomer attempted to reduce this method to practice.

Horrebow states that, in the years 1692–3, Roemer remarked a series of irregularities in the declinations of the stars, which could not be accounted for either by parallax or refraction, but which he suspected to arise from a variation in the position of the terrestrial axis, the theory of which he hoped to assign on some future occasion**. It cannot admit of any doubt that the anomalies observed by that distinguished astronomer were attributable to the effects of aberration which hitherto continued to be unaccounted for. Dismissing the declinations as not sufficiently trustworthy in so delicate an enquiry, Roemer attempted to deduce the parallax of the fixed stars from observations of their right ascensions. By a series of observations of Sirius and α Lyræ, made at different seasons of the year, he found that the aggregate of the parallaxes of the two stars produced an annual variation in the difference of their right ascensions, amounting to somewhere between 1^{m} and $1\frac{1}{2}^{m}$. It has been proved by the observations of subsequent astronomers, that such a displacement could not have arisen from the effects of parallax. The same may be said respecting a series of results which Horrebow deduced at a subsequent period from the observations of Roemer, and which he published with great confidence, as affording incontestable evidence of the motion of the earth, in his work entitled "*Copernicus Triumphans.*"

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* "Attempts to Prove the Motion of the Earth," p. 7.

† Lemonnier, Hist. Céleste, p. 252.

‡ Wallis's "Opera Mathematica," tom. iii., p. 705.

§ Mém. Acad. des Sciences, 1699, p. 177.

‖ See chapter xiv.

¶ Phil. Trans., 1693, p. 844.

** Basis Astronomiæ, p. 66.

It has been already mentioned, that, in an attempt to establish the existence of a sensible parallax in *γ Draconis*, Bradley was conducted to his immortal discovery of the aberration of light. This important result put an end to the anomalous irregularities which continued to affect the observations of astronomers, ever since they became sufficiently accurate to render sensible the displacement arising from that cause. It was now evident that the parallax of the fixed stars was in all cases vastly smaller than it had hitherto been supposed to be. Bradley was of opinion that if the parallax of *γ Draconis* had amounted even to 1″, his observations with the zenith sector could not fail to have detected its existence*.

Allusion has been made to the method proposed by Galileo for determining the parallax of a star. The invention of the micrometer, about the middle of the seventeenth century, tended very much to enhance the practical utility of that method. It was probably this circumstance which induced James Gregory to recommend its application in a letter to Collins, the Secretary of the Royal Society, dated June 24, 1673†. Gregory does not seem to have been aware that Galileo had already suggested the same method. Huyghens appears to have been the person who first reduced it to practice. He mentions, in one of his works, that he in vain attempted to detect any traces of an annual change in the relative position of the two stars composing the middle star in the tail of the Great Bear‡ (*ζ Ursæ Majoris*). In the following century Dr. Long endeavoured to detect a similar variation in the relative positions of several double stars, but all his efforts were fruitless§. It is to be remarked, with respect to the stars upon which he made his observations, that the component objects in each case were nearly equal in apparent magnitude, whence it might be presumed that they were equally distant from the earth. The absence of an indication of change in their relative positions did not, therefore, necessarily imply the non-existence of a sensible parallax.

The parallax of the fixed stars was one of the earliest subjects which engaged the attention of Sir William Herschel‖. He did not fail to perceive the many peculiar advantages which the method of Galileo offered, and, with a view to its practical application, he selected a great number of double stars, which, in consequence of the inequality of the component members, in every case appeared to be well adapted for that purpose. His labours on this occasion are memorable in the history of astronomy, on account of their having been instrumental in enabling him to establish the existence of a physical connexion between the bodies composing Double Stars.

About the beginning of the present century, the celebrated Piazzi endeavoured to ascertain the parallax of some of the principal stars by means of their declinations, as observed with the famous altitude and azimuth instrument of Ramsden. From such data he determined the parallax of Sirius to be 4″, and that of Procyon to be 3″. The researches of subsequent astronomers have not confirmed these results; indeed, there was very little confidence reposed in them even at the time of their first announcement. The same may be said respecting the contemporaneous labours of Calandrelli, the countryman of Piazzi, who, from observations of α Lyræ at Rome, deduced a parallax of 3″.9.

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* Phil. Trans., 1728, p. 637.

† Birch, Hist. Roy. Soc., vol. iii. p. 225.

A few years afterwards Dr. Brinkley undertook a series of observations of several of the principal stars, with the view of detecting the existence of a sensible parallax in some of them. His researches were founded on the declinations of the stars, as determined by him at the Observatory of Dublin, with a magnificent altitude and azimuth circle, eight feet in diameter, designed by Ramsden, and partially executed by that famous artist. The results at which he finally arrived seemed to indicate that some of the principal stars really possessed a parallax amounting to a few seconds of space, but their accuracy was disputed by Pond, the Astronomer Royal, who was unable to deduce analogous results from the observations with the mural circle, recently erected at Greenwich. A controversy thereupon arose between the two astronomers, which was maintained for several years with great vigour and ability on both sides, but which finally terminated, leaving the question at issue still undecided. It is now well ascertained that Pond was right in his conclusions, but it must be admitted, on the other hand, that the anomalous results obtained by his opponent have not been satisfactorily accounted for. This celebrated controversy, if it did not lead to the object originally contemplated by Brinkley, was not unproductive of advantages of great importance, in so far as the ultimate accomplishment of that object was concerned; since it had the effect of placing in a vastly clearer light than hitherto, the influence of the various disturbing causes which go to complicate the observations of astronomers, and which completely efface the delicate variations arising from the parallax of the fixed stars, unless rigorously taken into account*.

During the interval included between the years 1818 and 1821, M. Struve made a series of observations of circumpolar stars at Dorpat, with the view of detecting a sensible parallax in some of them. His method consisted in observing the stars by pairs, selecting those which were nearly opposite in right ascension, so that the superior passage of one of the stars took place almost simultaneously with the inferior passage of the other; and the evidence of parallax was sought for, by comparing the differences of the right ascensions of the two stars as determined at various times throughout the year. The researches of M. Struve seemed to indicate the existence of a sensible parallax in several instances, but the results were in all cases so insignificant, and consequently the probability was so much the stronger of their being produced by disturbing causes, whose influence had not been taken into account, that, notwithstanding the acknowledged reputation of the astronomer to whom they were due, they did not generally command the confidence of the scientific world.

In the year 1835 M. Struve commenced a series of observations with the view of detecting the parallax of the bright star α Lyræ. The method pursued by him on this occasion, consisted in measuring with a micrometer the distance between the star and another very small star, situate about 43″ from it, repeating the operation at different seasons throughout the year. The result of this enquiry went to assign a parallax of 0″.261 to the brighter of the two stars.

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* See, on the subject of this controversy, the *Philosophical Transactions* for 1810-17-18-21-23-24; also vols. xii. and xiv. of the *Transactions of the Royal Irish Academy.*

Shortly afterwards, the existence of an annual variation of sensible amount in the apparent position of a star, depending on the motion of the earth in her orbit, was established beyond all doubt, in two instances, by the independent labours of two contemporary astronomers. The stars, whose absolute distances have been thus ascertained, are 61 Cygni and α Centauri, the parallax of the former having been determined by Bessel, and that of the latter by the late Professor Henderson, of Edinburgh. It is impossible, within the limits to which we are confined, to do anything else than merely bestow a passing glance on the important researches of these astronomers on this occasion.

The star 61 Cygni, besides being double, is remarkable for an unusually large proper motion. It appears, in fact, from the observations of astronomers, to be transported through space with an annual motion of rather more than 5″ of arc. This circumstance induced Bessel to suspect that its parallax must be very considerable, and he resolved accordingly to ascertain its amount. He chose for this purpose two very small stars, one at the distance of 11′.8 from the centre of the line joining the two component members of 61 Cygni, and the other at the distance of 7′.7 from the same point; and the establishment of the parallax of the star depended on the detection of an annual variation of these distances, as determined by observation at different seasons of the year. The distances were ascertained by means of a magnificent heliometer, which Fraunhofer, of Munich, had recently executed for the Observatory of Königsberg, and which, from the principle of its construction, was eminently adapted for micrometric measurements on so large a scale. The observations were commenced in the month of October, 1837, and were continued till March, 1840. By a subsequent discussion of them, in which every imaginable cause of disturbance was taken into careful consideration, and its effect rigorously calculated, Bessel arrived at the final conclusion that the parallax of the star amounts to 0″.3483. This result has received a complete confirmation from the recent researches of Mr. Peters, who, from a series of zenith distances of the star, determined at the Observatory of Pulkowa during the years 1842–3, has found its parallax to be equal to 0″.349.

It has been mentioned, that the other astronomer who succeeded in establishing by unequivocal evidence the existence of a sensible parallax in one of the fixed stars, was the late Professor Henderson, of Edinburgh. The double star, α Centauri, which was the object of his researches on that occasion, is one of the brightest sidereal objects in the southern hemisphere. His investigation was founded on the zenith distances of the star as observed by himself at the Cape of Good Hope, with a mural circle, during the years 1832–3. These observations were made with the view of establishing the mean declination of the star. It was only from a consideration of the large proper motion of the star, which amounts annually to 3″.6 of arc, that he was induced several years afterwards to institute an enquiry into its parallax. The result at which he arrived confirmed his previous suspicion, the star having been found by him to have a parallax of 1″.16. The researches of Mr. Maclear the successor existence of a sensible parallax in the star, the final result obtained by him being only a small fraction less than that deduced by Henderson. From observations made at the Cape, during the years 1839-40, Mr. Maclear determined the parallax of the star to be 0″.9128, and from still more recent observations, extending down to 1848, he has found it to be 0″.9187. The latter may be considered as the definitive value of the parallax of the star.

The parallax of α Centauri, as determined by the researches of Mr. Maclear, assigns to the star a distance from the earth which may be said to amount, in round numbers, to 20 billions of miles. The successive propagation of light supplies the only means of forming anything like an adequate conception of this immense distance. Now light traverses space at the rate of 192,000 miles in a second of time, and consequently the distance traversed in a year would extend to 6.059 billions of miles. Hence it follows, that notwithstanding the amazing velocity of this agent, it would require about three years and a quarter to pass from α Centauri to the earth. The distance of 61 Cygni is about 60 billions of miles; consequently the light of that star does not reach the earth in less than ten years after its emission!

Knowing the parallaxes of 61 Cygni and α Centauri, and also their apparent proper motions, it is hence easy to deduce the absolute amount of space through which each of these stars is transported in the course of a year. Thus, in the case of 61 Cygni, which has an annual proper motion of about 5″, since the radius of the terrestrial orbit subtends only an angle of 0″.348 when viewed at the distance of the star, it follows, that the space through which the star is annually transported in virtue of its proper motion, exceeds the radius of the terrestrial orbit in the proportion of 5″ to 0″.348. Now the radius of the terrestrial orbit may be estimated, in round numbers, at 95 millions of miles. Hence it may be ascertained, by a simple process of arithmetic, that the star, 61 Cygni, whose proper motion can only be established by observations of the most delicate kind, and which in consequence was long regarded as a *fixed* star, is continually transported through space at the annual rate of 1333 millions of miles! In the same way it may be shewn, that the linear space through which α Centauri is transported in the course of a year, amounts to no less than 371 millions of miles. It is manifest, from these numbers, that the epithet, *fixed*, can no longer be regarded as an appropriate designation of any class of the celestial bodies.

Allusion has been made to M. Struve's investigation of the parallax of α Lyræ. The more recent researches of M. Peters, tend to prove that the star has a sensible parallax, although the result is considerably less than the parallax previously assigned to the star by M. Struve. Assuming the latter to be the true value, as being determined by a more trustworthy method than that founded on observations of the zenith distance of the star, which formed the groundwork of M. Peters' researches, we may, by combining it with the light of the star as ascertained by the experiments of Wollaston, arrive at an evaluation of the absolute magnitude of the star. To this end it may be remarked that the parallax 0″.261, arrived at by M. Struve, supposes the distance of α Lyræ from the earth to exceed the distance of the sun from the earth in the proportion of about 800,000 to 1. Now if the sun was transported to this distance, it would follow, from the diminution of light according to the reciprocal of the square of the distance, that the actual light of the sun would exceed the light he would then emit, in the proportion of 640,000 millions to 1. But Wollaston found that the actual light of the sun exceeded the light of α Lyræ, only in the proportion of 180,000 millions to 1. It follows, therefore, that the light of α Lyræ would exceed that of the sun placed at the same distance, in the proportion of 64 to 18, or, in other words, the light of α Lyræ is equal to that afforded by 3.5 suns.

In addition to the parallaxes of 61 Cygni and α Lyræ, M. Peters has investigated those of several other stars. The groundwork of his researches in all these instances consisted of a series of apparent zenith distances of the stars determined at the Observatory of Pulkowa, with the great vertical circle by Ertel, during the years 1842–3. The results have been employed by him in establishing the mean value of the parallax of a star of the second magnitude, which he finds to amount to 0″.116. Hence it may be readily inferred that the light from one of such stars would not reach the earth in less than 28 years. Combining the result at which he had arrived relative to the mean parallax of a star of the second magnitude, with the relative distances of the stars of successive magnitudes, as determined by M. Struve, in his *Etudes d'Astronomie Stellaire*, M. Peters was enabled to estimate the parallaxes of the stars of the various orders, from the stars of the first magnitude down to the smallest stars visible in the 20-feet reflecting telescope of Herschel. In this manner he found that the distance of the smallest stars visible to the naked eye is such, that the light emitted by them does not reach the earth in less than 138 years. He also found that the light from one of the smallest stars visible in the 20-feet reflecting telescope of Herschel, occupied 3541 years in traversing the distance between the star and the earth! These results can of course only be regarded as provisional, until observations of sufficient delicacy be amassed, which shall form the groundwork of a more rigorous investigation of the subject; but, being founded upon principles which present a strict accordance with analogy, they cannot fail to prove highly interesting and suggestive to the reflecting mind.

Although it was long generally supposed that the stars are absolutely immoveable with respect to each other, there were occasionally individuals of an original turn of mind who refused their assent to a principle which formed so striking an anomaly to all the other arrangements of nature, and who ventured to suggest the probability of a motion of the stars, *inter se*, before the observations of astronomers acquired a sufficient degree of precision to indicate even the slightest trace of its real existence. Among these may be mentioned Jordano Bruno, an Italian philosopher of the sixteenth century, whose abjuration of the Romish faith, and vigorous exposure of the fallacies of the Aristotelian physics, the adherents of which were closely leagued with the Church in repressing all rational enquiry, unfortunately led to the forfeiture of his life. This daring, although somewhat eccentric individual, believed that the stars are all equal to the sun in magnitude and splendour; and that the universe is peopled with an innumerable multitude of such bodies. He maintained that we are not warranted in supposing that they are all fixed with respect to each other, since their distance from the earth is so immense, that it would be difficult to estimate their motions; and he remarked, that it could only be decided after a long course of observation, whether the stars revolved round each other, or what other motions they might have*. Hooke, whose genius threw a gleam of light on every subject of physics which engaged his attention, did not fail to perceive the improbability of the stars being absolutely fixed with respect to each other, and he suggested that not only these bodies, but the whole solar system might be in a state of continual motion*.

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* Jordano Bruno was born about the middle of the sixteenth century, at Nole, in the kingdom of Naples. In early life he entered the order of Dominicans; but having

The first person who suspected, from observation, that the stars have a proper motion, was the celebrated Edmund Halley. His remarks on the subject are contained in a paper which is inserted in the *Philosophical Transactions* for 1718. By comparing the observations of the earlier astronomers of the Alexandrian school with those of more recent times, he was led to conjecture that the stars Aldebaran, Arcturus, and Sirius were advancing slowly towards the south. In 1738 J. Cassini communicated a memoir on the same subject to the Academy of Sciences, which served to confirm the justness of Halley's surmise. By comparing the observations of Arcturus made by Richer, at Cayenne, in 1672, with similar observations executed in his own time at Paris, he found that the latitude of the star had undergone a sensible change during the included interval. It was still uncertain, however, whether this proceeded from a displacement of the ecliptic, or from an actual change in the position of the star. J. Cassini demonstrated, by the most conclusive evidence, that the phenomenon was attributable to the latter cause. He found, in fact, that while Arcturus had shifted to the extent of 5′ in latitude since the time of Tycho Brahé, the star η Bootes, situate in its vicinity, did not exhibit any sensible displacement. Now the change of latitude ought to have been sensibly the same for both stars if it had arisen from a displacement of the ecliptic. It was therefore manifestly due to an alteration in the position of Arcturus with respect to the other stars†.

Bradley, in the paper containing the account of his discovery of the nutation of the earth's axis, inserted in the *Philosophical Transactions* for 1748, has remarked, that the apparent motions of the stars may arise either from a motion of the solar system in space, or from a real change in the positions of the stars themselves; but he considered that many ages would elapse before it would be possible to arrive at a definitive conclusion on the subject.

The motion of the solar system in space formed part of the cosmical system of Thomas Wright, propounded by him in his "Theory of the

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become disgusted with the corrupt morals of his brother monks, he soon afterwards retired from the cloister. About the year 1580 he repaired to Geneva, where he abjured the Romish faith, and embraced the doctrines of Calvin. It does not appear, however, that he was a steadfast adherent to the principles of his new creed. From Geneva he passed into France, and afterwards visited England and Germany. During his residence in these countries he distinguished himself by the originality of his ideas on different subjects of philosophy, and by his merciless exposure of the absurdities of the schoolmen. Being naturally desirous of revisiting his native country, he proceeded to Venice in 1598; but he had no sooner arrived in that city than he was arrested, and sent as a prisoner to Rome. Here he was detained for two years in the dungeons of the Inquisition, without being brought to trial. At length, on the 9th of February, 1600, he was condemned to die the death of a heretic and infidel. It is said, that when his sentence was read to him, he replied to his judges in the following terms:—"This sentence, pronounced in the name of a God of mercy, terrifies you more than it does me." In pursuance of the sentence, the unfortunate man was carried to the place of execution on the 17th of the same month, and forthwith committed to the flames. There is reason to suspect that, as in the more lenient case of Galileo, the baffled Aristotelians had some complicity with the Romish Church in the perpetration of this atrocious crime. Jordano Bruno was the author of a great number of works on philosophy, all of which display great boldness and originality of thought.

* Posthumous Works, p. 506.

† Mém. Acad. des Sciences, 1738, p. 337.

Universe," published in 1750[*]. He supposes that the sun, with his cortege of planets, as well as all the stars of the firmament, are in continual motion. We shall have again occasion to refer to the speculations of this individual.

Mayer was the first astronomer who endeavoured to deduce some definite result from an examination of the proper motions of the stars. His researches on the subject are contained in a memoir which he communicated to the Academy of Sciences of Gottingen, in 1760, and which was subsequently inserted in vol. i. of his "*Opera Inedita*," published in 1771. They were based upon the proper motions of 80 stars, determined by a comparison of the observations of Roemer, in 1706, with corresponding observations of his own and Lacaille's, in 1750 and 1756. He remarked, that if the solar system was advancing towards any determinate part of the celestial sphere, it would necessarily follow that the stars in that region would appear to be gradually approaching together, whereas in the opposite region they would seem to be withdrawing more and more apart from each other. He arrived, however, at the conclusion that the observed proper motions of the stars did not afford evidence of the solar system being transported through space towards any particular region of the heavens.

In 1783 Herschel examined the same subject, and arrived at a conclusion diametrically opposed to that of Mayer. His investigation was founded, in the first instance, upon the proper motions of seven principal stars, as determined by Maskelyne. These data seemed to indicate that the solar system was advancing towards a point in the constellation of Hercules, corresponding to about 257° of right ascension. An examination of the proper motions of several of the stars contained in Mayer's list tended to confirm this opinion. He arrived at the final conclusion, that by taking a point somewhat to the north of the star λ Herculis, the observed proper motions would be reconciled to a great extent with the hypothesis of a motion of the solar system in that direction. The point thus assigned by him was situate in 257° of right ascension and 25° north declination[†].

The same year which produced the researches of Herschel on the motion of the solar system in space, was also distinguished by a similar investigation on the part of Prevost. By a discussion of Mayer's proper motions, he found that the solar system was advancing towards a point situate in 230° of right ascension and 25° north declination.

The subject of the motion of the solar system in space was subsequently considered by Klugel in the Berlin Ephemeris for 1789. His researches were founded on the proper motions contained in Mayer's list. He found that the apex of the solar motion was situate in 260° of right ascension and 27° north declination. This result presented a very satisfactory agreement with that obtained by Herschel.

In 1805 Sir W. Herschel communicated to the Royal Society a second paper on the motion of the solar system. His researches on this occasion were based upon Maskelyne's catalogue of the proper motions of 36 stars published in 1790. He obtained for the solar apex a position whose right ascension was 245° 52′ 30″ and north declination 49° 38′. This result differed considerably from that to which his previous researches had conducted him.

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[*] "An Original Theory, or New Hypothesis of the Universe, founded upon the Laws of Nature, &c., by Thomas Wright, of Durham." 4to. London

Biot, in the additions to his *Astronomie Physique*, published in 1812, has considered the subject of the motion of the solar system in space. An investigation, based on the proper motions of several stars, as determined by a comparison of the observations of Bradley and Mayer with those of Maskelyne and Piazzi, induced him to conclude that there did not exist sufficient grounds for believing that the solar system is advancing towards any determinate point of the heavens. The same conclusion was arrived at in the year 1818 by Bessel, who examined the subject in the *Fundamenta Astronomiæ*, the groundwork of his researches consisting of the proper motions of a considerable number of stars, determined by a comparison of the observations of Bradley with those of Piazzi.

No enquirer seems to have attempted to verify or disprove the assertion of Bessel until Argelander was induced to direct his attention to the subject. The researches of that distinguished astronomer were published in the year 1837, in vol. iii. of the "*Mémoires Presentés par Divers Savans*," of the Academy of St. Petersburg. They were based upon the proper motions of 390 stars, determined by a comparison of the observations of Bradley with those executed by himself at Abo. The result at which he arrived was favourable to the supposition of the motion of the solar system in space. The following are the co-ordinates of the point towards which he found it to be advancing, referred to the mean equinox of 1792:—

Right Ascension.	Declination North.
259° 47′.6	32° 29′.5.

This result, deduced by a method founded upon some of the most refined principles of mathematical investigation, presents a remarkable agreement with the rough determination of Herschel in 1783.

The next person who undertook to investigate the subject of the solar motion was Lundahl. His researches were based upon the proper motions of about 150 stars, determined by a comparison of the observations of Bradley with those of Pond. He obtained, for the apex of the solar motion, the following co-ordinates, reduced to the mean equinox of 1792:—

Right Ascension.	Declination North.
252° 24′.4	14° 26′.1.

This result presents a considerable discordance with most of those hitherto deduced.

M. Otto Struve soon afterwards undertook the investigation of this interesting subject. His researches were based on the proper motions of about 400 stars, determined by a comparison of their places, as observed by Bradley, with those deduced from the observations of M. Struve at Dorpat. The following are the co-ordinates he obtained for the apex of solar motion, reduced to the beginning of the year 1790:—

Right Ascension.	Declination North.
261° 23′.1	37° 35′.7.

By combining this result with the corresponding results arrived at by Argelander and Lundahl, M. Otto Struve determined the co-ordinates of the point in the celestial sphere, towards which the sun is advancing to be:—

Right Ascension.	Declination North.
259° 9′.4	34° 36′.5

This may be considered as the definitive position of the apex of solar motion, deducible from the proper motions of stars in the northern hemisphere, as hitherto determined. A remarkable confirmation of its accuracy has been recently afforded by the researches of Mr. Galloway on the same subject, founded on the observations of stars in the southern hemisphere *. The data which formed the groundwork of Mr. Galloway's investigation, consisted of the proper motions of 81 stars, determined by a comparison of their places, as assigned by Lacaille, about the middle of the eighteenth century, with those deducible from the modern observations of Johnson and Henderson. The following are the values of the co-ordinates which he obtained for the apex of solar motion, the epoch of the mean equinox being the beginning of 1790:—

Right Ascension.	Declination North.
259° 46′.2	32° 29′.6.

Mr. Main, of the Royal Observatory, Greenwich, has recently communicated to the Astronomical Society a paper on the proper motions of 875 stars, which may be considered as establishing, beyond all doubt, the general accuracy of the various investigations above referred to †.

The researches of M. Otto Struve have conducted him to a determination of the actual velocity with which the solar system is being transported through space. Having found that the space traversed by the sun in a year would subtend an arc equal to 0″.3392, if viewed at the mean distance of stars of the first magnitude, and having, moreover, ascertained that the mean parallax of the stars of the latter class amounted to 0″.209, he was enabled hence to conclude that the space through which the solar system annually moves, exceeds the radius of the terrestrial orbit in the proportion of 0″.3392 to 0″.209. In this manner he found that the absolute space traversed by the solar system in the course of a year amounts to 1.623 radii of the terrestrial orbit, which is equivalent to 154 millions of miles. Comparing this result with the corresponding results obtained for 61 Cygni, and α Centauri, we have—

The sun	154 millions of miles.
α Centauri	371 ,, ,,
61 Cygni	1333 ,, ,,

These numbers are fairly comparable with each other, and are very interesting on account of the analogy which they exhibit as existing between the sun and the stars.

The elder Struve has thus summed up the results to which the three astronomers, MM. Argelander, O. Struve, and Peters have been conducted by their researches in stellar astronomy:—"*The motion of the solar system in space is directed to a point in the celestial sphere, situate on the right line, which joins the two stars of the third magnitude, π and μ Herculis, at a quarter of the apparent distance between these stars, measured from π Herculis. The velocity of this motion is such, that the sun, with the whole cortege of bodies depending on him, advances annually in the direction indicated, through a space equal to* 1.623 *radii of the terrestrial orbit, or* 154 *millions of miles.*"‡

Although the recent researches of astronomers have thus fully established that the sun, and his attendant planets, are at present advancing towards a determinate point in the heavens, it would be inconsistent with analogy to suppose that the motion will always be directed towards the same point. From a consideration of all the other celestial movements that have been hitherto recognised by astronomers, it is impossible to avoid the conclusion that the path traced out by the sun is in reality curvilinear. But, indeed, motion in a right line would be utterly incompatible with the principle of gravitation, as established by Newton, between the different parts of the material universe. It can hardly be doubted, therefore, that the apex of solar motion is slowly shifting its position in the celestial sphere, and that it will eventually exhibit a sensible displacement. It is not improbable that by thus tracing out the path of the solar apex in the heavens, the actual path of the sun in absolute space may be one day determined. This would, indeed, be a magnificent triumph of inductive science; but a countless series of ages may, perhaps, elapse before its achievement will be realised.

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* Phil. Trans., 1847, p. 79, et seq.

† Mem. Ast. Soc. vol. xix. p. 121 et seq.

Guided, also, by the analogy of the planets and satellites revolving round their respective primaries, several enquirers into the cosmical structure of the heavens have come to the conclusion, that the whole solar system is revolving round some central body. This principle formed part of the system of Wright, as expounded in his remarkable work on the "Theory of the Universe," to which allusion has already been made. It was also maintained by his successors, Kant, Lambert, and Michell. Sir William Herschel has cautiously abstained from expressing a decided opinion upon this point. In more recent times M. Mädler has made the hypothesis of a central sun the groundwork of some very remarkable speculations. His final conclusion is, that the Pleiades may be regarded as the central group of the stars composing the system of the milky way, and that the bright star of that group, Alcyone, is the central body, round which they are all revolving. It is manifest that all such speculations are far in advance of practical astronomy, and therefore they must be regarded as premature, however probable may be the suppositions on which they are based, or however skilfully they may be connected with the actual observations of astronomers.

When astronomical observations came to be made with telescopes possessing a considerable degree of optical power, it was found that several stars, which to the naked eye appeared single, consisted in reality of two stars, so very near to each other that they severally failed to produce an impression of their individuality upon the unaided organ of vision. As early as the middle of the seventeenth century, Riccioli remarked that the star in the middle of the tail of the Great Bear (ζ Ursæ Majoris), when observed with the telescope, was found to exhibit the appearance of two distinct stars in close juxtaposition*. A few years afterwards a similar remark was made by Hooke with respect to the double star γ Arietis†. Several other stars, such as α Geminorum or Castor, γ Virginis, &c., were also found to be double, about the beginning of the seventeenth century. The star θ Orionis, in the middle of the great nebula, discovered by Huyghens in 1656, even presented the appearance of three distinct stars in close proximity to each other.

Occasional discoveries of double stars continued to be made throughout the eighteenth century; but these objects cannot be said to have excited a general interest among astronomers until Sir William Herschel directed his attention to them. In the year 1779, that illustrious astronomer undertook an extensive examination of the heavens with a view to the discovery of double stars, hoping that he might be enabled in some instances to establish an annual change in the relative positions of the two component members, which might indicate the existence of a sensible parallax. In 1782 he exhibited the first fruits of his labours by communicating to the Royal Society a paper containing the places of 269 double stars, together with the distances of the component members, and also their angles of position corresponding to a given epoch. Of the stars contained in this catalogue there were 227 which had not hitherto been recognized as double by any observer[*]. In 1785 Herschel communicated to the Royal Society a second catalogue of double stars, containing 434 more of such interesting objects[†].

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* Almag. Nov., tom. i., part i., p. 422.

† "Attempt to prove the Motion of the Earth," p. 7 (1674).

Michell appears to have been the first person who suggested that the close proximity of the constituent members of double stars was owing to some physical connexion existing between them. In a remarkable paper, which he communicated to the Royal Society, he presented this view of the subject in a very strong light, by an application of the doctrine of probabilities to the theory of the distribution of the stars in the celestial sphere[‡]. In a subsequent communication, which appears in the *Philosophical Transactions* for 1784, he expressed his firm conviction that the double and triple stars discovered by Herschel were so many systems of stars, so near to each other as to be liable to be affected by their mutual gravitation; and he considered it as not unlikely that the periods of the revolutions of some of these about their principals might some time or other be discovered[§].

If double stars formed in reality independent systems, maintained under the influence of the principle of gravitation, it would follow as a necessary consequence, that the constituent members of each system would be subject to a constant change of relative position from their revolution round their common centre of gravity. It is worthy of remark, however, that there is another fact connected with double stars which, when established by observation, affords equally unequivocal evidence of the physical connexion of the constituent members. If among those double stars which have a sensible proper motion, it be found in any instance that both stars are transported with the same apparent velocity, we have just grounds for believing that a physical relation exists between them. Such a fact had been noticed towards the close of the eighteenth century, but, strange to say, the astronomer who directed attention to it, did not perceive the legitimate conclusion which was deducible from it. The bright star Castor, besides being double, is, moreover, remarkable for a sensible proper motion, which amounts to about 0″.15 of space every year. Now, Dr. Hornsby remarked, in the year 1798, that notwithstanding the large proper motion of this star, the mutual distance of the two constituent members did not seem to have undergone any change during a period of twenty years embraced by his observations. The only conclusion which he drew from this remarkable fact was, that both stars were moving with the same velocity and in the same direction[‖].

[*] Phil. Trans., 1782, p. 112, et seq.

[†] Ibid., 1785, p. 40, et seq.

[‡] Ibid., 1767, p. 234, et seq.

[§] Ibid., 1784, p. 36, et seq.

The glory of establishing the physical theory of Double Stars upon the solid basis of observation and sound reasoning was reserved for Sir William Herschel. The results of his researches on this occasion are contained in two papers communicated by him to the Royal Society, which are inserted in the *Philosophical Transactions* of that body for the years 1803 and 1804 *. By a comparison of his earlier observations of a great number of double stars, with corresponding observations made after an interval of about twenty years had elapsed, he found that in many instances the relative positions of the constituent bodies had undergone a sensible change. He demonstrated by incontrovertible reasoning, that all those double stars, which afforded evidence of such a change of relative position in the constituent members, formed so many independent systems of bodies revolving under the influence of their mutual attraction around their common centre of gravity. He even carried his researches so far as to assign the periods of revolution of several of these Binary Systems. Thus to Castor he assigned a period of 342 years, to δ Serpentis, a period of 375 years, and to γ Leonis, a period of 1200 years. These numbers were of course assigned merely as rough approximations to the true periods of revolution.

The last contribution of Sir William Herschel, on the subject of Double Stars, was a paper which he communicated to the Astronomical Society upon the occasion of its establishment in the year 1820. It contains the places of 145 double stars, with their distances and angles of position at certain epochs. The observations which form the groundwork of this catalogue appear to have been all anterior to the year 1802.

In 1824 fresh interest was awakened in the subject of Double Stars by the communication of a paper to the Royal Society on the part of Sir John Herschel and Sir James South, containing the results of their joint labours in this field of enquiry. This paper contained micrometrical measures of 380 double and triple stars, carefully observed with a 7-feet equatorial. A comparison of these measures with the earlier observations of Sir William Herschel, afforded in many instances an interesting confirmation of the result arrived at by that astronomer relative to the physical connexion of the constituent members of double stars. One of the objects composing the double star, η Corona, had accomplished a complete circuit round its primary, and was already well advanced in a second revolution. The star τ Serpentarii, which Sir William Herschel in his first catalogue communicated to the Royal Society had stated to be double, no longer exhibited the slightest trace of duplicity—one of the constituent stars having been now exactly projected upon the other in virtue of its relative motion. On the other hand, ζ Orionis, which Sir William Herschel had marked as a single star, was now recognised to be double even in telescopes of moderate power †.

In 1826 Sir James South communicated to the Royal Society a paper containing micrometrical measures of 458 double stars executed solely by himself. The results of Sir John Herschel's subsequent labours in the same field, exclusive of his more recent observations in the southern hemisphere, are contained in a series of papers communicated by him at different times to the Astronomical Society. The stars whose duplicity was recognised by him on this occasion, amount in number to 3347, being such as he happened to meet with, while engaged in an extensive survey of the nebular contents of the heavens with a 20-feet reflector. The original observations were transmitted to the Society in six successive catalogues; but as they were considered by their author to be subordinate to the main object of his labours, they did not possess any pretension to extreme accuracy. He, however, communicated to the Society two papers containing micrometrical measures of a considerable number of double stars, carefully determined with a 7-feet equatorial*.

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* Phil. Trans., 1803, p. 339, et seq.; 1804, p. 353, et seq.

† Annuaire, 1834.

No astronomer of the present day has contributed in so high a degree to the advancement of the subject of Double Stars as the illustrious M. Struve. His labours in this interesting field of astronomical enquiry were commenced in the year 1813, upon his being appointed Director of the Observatory of Dorpat. He continued to prosecute them with more or less assiduity till the year 1824†, when, having obtained possession of a magnificent refractor of 9.9 inches aperture, executed by Fraunhofer, he conceived the design of examining with it all the stars down to the eighth magnitude, situate between the north pole and 15° of south declination. This design was followed up by him with admirable perseverance during many years‡. In 1837, he finally published the results of his labours in a large folio volume§. It contains micrometrical measures of the distances and angles of position as well as the approximate places of 3112 double stars, as determined between the years 1824 and 1836, besides the results of his earlier labours on the same subject, extending from 1813 to 1824.

While M. Struve was engaged in his extensive micrometric measurements of double stars, an important advance was made in the theory of the movements of bodies of this class. The growing evidence of a physical connexion between the constituent members of double stars, which was afforded in many instances by a comparison of the more recent with the earlier observations of astronomers, suggested the possibility of deducing from observation the elements of the elliptic orbit, in which it was presumed that the one star revolved around the other. Researches undertaken with a view to effect this object were attended with complete success. To M. Savary is due the honour of having first demonstrated that the elements of the orbit of a double star might be derived from a determinate number of the observed distances and angles of position of the star. In the *Connaissance des Temps* for 1830, he has explained a method invented by him for the attainment of this end, and has exhibited a proof of its practical utility by employing it in calculating the elements of the orbit of the double star ξ Ursæ Majoris. M. Encke shortly afterwards assigned a method of his own, by means of which he computed the elements of the orbit of the double star 70 Ophiuchi. A third method, totally different from either of the two others just mentioned, was invented about the same time by Sir John Herschel *.

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* Mem. Ast. Soc., vols. v., viii.

† In 1822, he published a catalogue of 795 double stars which had passed under his examination since the commencement of his labours at Dorpat.

‡ In 1827, he published a catalogue of about 3000 double stars, which he detected during the first two years of his labours, with the great refractor. In the course of his survey on that occasion he examined no fewer than 120,000 stars! The objects of this catalogue were chiefly those of which he published the micrometric measures in 1837.

§ "Stellarum Duplicium, &c., Mensuræ Micrometricæ per magnum Fraunhoferi tubum, 1824-37, speculâ Dorpatensi institutæ; adjecta, Synopsis Observationum de Stellis Compositis, 1814-24, per minora instrumenta," folio, Petrop, 1837. In 1845, M. Struve published a catalogue of 514 double and multiple stars discovered at Pulkowa with the great refractor of 14.9 inches aperture.

The elements of a considerable number of double stars have been calculated by one or other of the methods above referred to, and an elliptic orbit has thus, in each instance, been deduced, in the focus of which the primary star is supposed to be situate. The place of the revolving star has then been computed in a great many different positions of its orbit, upon the supposition that the motion is regulated by a force tending constantly to the focus, and the results have been compared with the corresponding places assigned by observation. The observed and computed places of the star have thus been found in general to agree within the limits assigned by the probable errors of observation. We are therefore warranted in concluding that the principle of gravitation, as announced by Newton, extends to systems of bodies placed at an almost inconceivable distance from the earth, and that by its controlling agency those systems are perpetually upheld. This may be justly asserted to be one of the most sublime truths which Astronomical Science has hitherto disclosed to the researches of the human mind.

No mention has hitherto been made of observations of double stars in the southern hemisphere. A considerable stock of such observations has, however, been already amassed. In 1828, Mr. Dunlop communicated to the Astronomical Society a catalogue of 253 double stars observed by him at Paramatta, in New South Wales. When Sir John Herschel was engaged at the Cape of Good Hope, in examining the nebulæ of the southern hemisphere with his 20-feet reflector, he did not fail to make observations of such double stars as he happened to meet with. The number of such objects which he thus detected in the course of his sweeps amounted to 2095. Accurate micrometrical measures were also made by him with his 7-feet equatorial.

With respect to those stars which have been discovered by astronomers to be double, many are doubtless merely optically so, their apparent proximity arising from the circumstance of their being situate nearly in the same line of vision. It is only in those cases wherein a change in the relative position of the two stars has been established by a comparison of observations made at different times, that we are enabled definitively to infer the existence of a physical connexion. It is to be remarked, however, that the lapse of time is constantly unfolding indications of such changes, and is thereby leading to a continual increase of the number of stars recognised as physically double. Stellar objects of this class have been distinguished from stars that are merely optically double by the appellation of *Binary Systems*.

The subject of Double Stars has engaged the attention of several astronomers of recent times, besides those to whose labours allusion has just been made. In this country, Captain Smyth, R.N., and the Rev. Mr. Dawes, have especially distinguished themselves by their exquisite observations connected with this department of sidereal astronomy. The Bedford Catalogue, executed by Capt. Smyth, and inserted in vol. ii. of his "Cycle of Celestial Objects," contains the results of observations of the most interesting double and multiple stars, of which the primaries are in Piazzi's catalogue. It comprises 580 double stars, 20 binary systems, and 80 triple and multiple stars [*]. The micrometric measures of these objects were executed by Capt. Smyth, at a private observatory which he erected at Bedford. This is one of several instances in which we have had occasion to allude to the labours of officers in the naval and military service of this country, who have adorned their profession by an enthusiastic devotion to scientific pursuits. The results of Mr. Dawes' micrometric measurements of double stars are contained in several important papers communicated by him on various occasions to the Astronomical Society, and subsequently inserted in the "Memoirs" of that body.

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* Mem. Ast. Soc., vol. v.

Among the astronomers of the Continent who, besides the elder Struve, have occupied themselves with observations of double stars, the illustrious Bessel is entitled to the first place. In the present day, M. Mädler has acquired just distinction by his labours in this interesting branch of astronomy. The theory of the subject has recently received some important elucidations from the researches of M. Yvon Villarceau, of Paris.

We now proceed to notice briefly the history of the Nebulæ of the celestial regions. Ptolemy has inserted five stars in his catalogue, which, on account of their hazy aspect, he distinguished by the appellation of *Cloudy Stars*. When these objects were examined with the telescope, they were found, in each instance, to consist of a cluster of stars situate so near to each other, that they failed to produce an impression of their distinct existence upon the naked eye. In the year 1612, however, Simon Marius, the German astronomer, discovered a nebulous object, which appeared to be of a totally different nature from any of those hitherto discovered. To the naked eye, indeed, it presented a dull nebulous aspect, like the other objects of the same class; but when examined with the telescope, it did not exhibit any indications of a sidereal structure. There were merely visible some faint rays of light, increasing in brightness towards the centre. The nucleus consisted of a dim light, fading away insensibly on all sides. Marius compared its appearance to that presented by the flame of a candle shining at night through a transparent horn [†]. In 1656, Huyghens discovered another nebula of this class in the middle of the sword of Orion. It struck its distinguished discoverer with amazement, as something totally different in its nature from those sidereal aggregations of which nebulous objects had hitherto been found to consist. The aspect of the heavens around this nebulous light was intensely black, a circumstance which suggested to Huyghens the idea of the phenomenon being occasioned by looking through an aperture in the heavens into a luminous region beyond [‡].

In the year 1714, Halley communicated a short paper to the Royal Society, in which he gave an account of the various nebulæ with which he was acquainted as having been hitherto recognised by astronomers [§]. In number they amounted only to six. Two of these were the nebulæ above mentioned. Another was situate between the head and bow of Sagittarius; according to Halley it was discovered by Abraham Ihle in 1665.

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[*] The Bedford Catalogue also comprises monographs of 170 nebulæ and clusters, which Capt. Smyth had selected for observation from Messier's Catalogue, and from the papers of the two Herschels. The "Cycle of Celestial Objects" was published in two volumes in 1844, and is justly esteemed to be one of the most instructive and delightful works on astronomy in our language.

[†] Simon Marius has given an account of his discovery of this nebula in the preface to his "Mundus Jovialis," published in 1612.

[‡] Opera Varia, tom. ii., p. 540 (*Systema Saturnium*).

[§] Phil. Trans. 1714, p.

A fourth was in the constellation Centaurus, and was marked by Bayer with the Greek letter ω; it was discovered by Halley at St. Helena, in 1677, while engaged in observing the stars of the southern hemisphere. A fifth preceded the right foot of Antinous; it was discovered by Kirch in 1681. The sixth was discovered by Halley in 1714; it was situate in the right line joining the stars η and ζ of the constellation Hercules. Halley was of opinion that the light of these objects came from an extraordinary distance. He supposed it to be occasioned by a lucid medium diffused throughout the ether, and shining with its own proper lustre.

If Halley had consulted the observations of Hevelius, he would have been enabled to assign a more ample list of nebulæ than that contained in his paper to the Royal Society. In the *Philosophical Transactions* for 1733, Derham has enumerated sixteen nebulæ, in addition to those contained in Halley's paper, which he extracted from the observations of the astronomer just cited.

An important contribution to the existing stock of nebulæ was made by Lacaille. In 1755, that distinguished astronomer communicated to the Academy of Sciences a catalogue of 42 nebulæ in the southern hemisphere, observed by him during his residence at the Cape of Good Hope*. They were divided by him into three distinct classes, each of which contained an equal number of objects. The first class contained all those nebulæ which failed to afford any indications of a sidereal structure. They were assimilated by Lacaille to small patches of the Milky Way. The objects of the second class were nebulous only in appearance, consisting in reality of so many congeries of stars, whose close proximity rendered them individually imperceptible to the naked eye. The objects of the third class were stars surrounded by a nebulous substance. He supposed that the latter were identical in structure with the nebulæ of the first class, and that the appearance of stars in the midst of them was merely accidental, arising in each case from the circumstance of the nebula and the star being both projected on the same region of the heavens.

Messier was the next astronomer who by his observations contributed to the enlargement of the number of recognised nebulæ. His first collection of these objects was published in the *Memoirs of the Academy of Sciences* for 1771. This list contained 45 nebulæ and clusters of stars. It was subsequently inserted, with successive additions, in the volumes of the *Connaissance des Temps* for 1783 and 1784, the number of objects in the last instance amounting to 103. Messier did not pronounce any opinion respecting the nature or physical constitution of the nebulæ; but, with the view of arriving at some conclusion on this subject, he recommended to future astronomers to make careful observations of them, in order to ascertain whether they exhibited any indications of a change of form or structure.

It appears from the foregoing brief notice of the nebulæ discovered by astronomers previous to the time of Sir William Herschel, that their number did not altogether amount to one hundred and fifty. In 1786, it received a notable accession from the labours of that distinguished astronomer, who communicated to the Royal Society a catalogue of 1000 new nebulæ and clusters of stars†. In 1789, he communicated to the same

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* Mém. Acad. des Sciences, 1755, p. 194, et seq.

† Phil. Trans., 1786, p. 457, et seq.

Society a second catalogue, containing 1000 additional nebulæ[*] and clusters, and in 1802, a third catalogue, which included 500 more of similar objects[†]. The nebulæ and clusters in each of these catalogues were distributed by Herschel into eight different classes; but this mode of arrangement had reference to the convenience of the astronomer who might be desirous of re-examining the objects, rather than to any peculiarities of physical structure. In the paper containing his third catalogue, he has enumerated the various objects of which he considered the fabric of the material universe to be composed. They are distributed by him into twelve different classes. A brief notice of each of these in succession may serve to acquaint the reader, in some degree, with the ideas entertained by Herschel on the construction of the material universe.

I. Insulated Stars.—Under this class, Herschel comprehended all those stars which do not afford any indication of being resolvable into two or more constituent objects. Among the stars of this class are included Arcturus, Capella, Sirius, Canopus, and the Sun. He supposed each of these stars to be sufficiently insulated in space, not to be sensibly affected by the attraction of the other bodies of the universe. They appeared to him, on this account, to be peculiarly adapted for constituting the centres of planetary systems. He was, in fact, inclined to suspect that the stars which appear in clusters, are unattended by revolving bodies; but that each star forms a magnificent world by itself, fulfilling all the purposes of a planetary body[‡].

II. Binary Stars.—These are systems composed in each case of two stars revolving around their common centre of gravity.

III. Triple and Multiple Stars.—These are systems of a more complicated structure than those just mentioned; but like them are resolvable into a determinate number of constituent bodies.

IV. Clustering Collections and the Milky Way.—Herschel was of opinion that in many instances the stars by their mutual attraction are gradually forming into clusters. The Milky Way, in many parts, appeared to him to afford unequivocal indications of such a clustering tendency. We shall presently have occasion to allude to his remarkable speculations on the structure of the Milky Way.

V. Groups of Stars.—Under this class, Herschel comprehended all those collections of stars which do not exhibit any regularity of outline, nor superior condensation in any particular part: at the same time they appear sufficiently insulated from the stars around them, to justify the conclusion that they formed so many separate systems[§].

VI. Clusters of Stars.—These were justly considered by Herschel to be the most magnificent objects of the heavens[||]. They are round in

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[*] Phil. Trans., 1789, p. 212, et seq.

[†] Ibid., 1802, p. 477, et seq.

[‡] Ibid., 1795, p. 69.

[§] One of the most interesting objects of this class is the group in the southern hemisphere, surrounding the star κ Crucis. According to Sir John Herschel it occupies an area of about 1-48th part of a square degree, and consists of about 110 stars, from the seventh magnitude downwards, eight of the more conspicuous of which are coloured with various shades of red, green, and blue, so as to give to the whole the appearance of a rich piece of jewellery. (*Results of Ast. Obs.*, &c., pp. 17, 102; *Outlines of Astronomy*, p. 597.)

[||] Sir John Herschel, the highest living authority on Nebulæ, states that by far the form, and are gradually more condensed towards the centre. These facts clearly point out the existence of an attractive force, binding the constituent bodies together, so as to form an independent system*.

VII. Nebulæ.—These were suspected by Herschel to be so many collections of stars, reducible to one or other of the three classes last mentioned, the nebulous aspect assumed by them being merely an illusion arising from the impossibility of discerning the stars individually, in consequence of the immense distance at which they are placed. Herschel calculated that the light from one of the faint nebulæ seen in his 40-feet telescope, must have occupied about two millions of years in its passage to the earth, although traversing space at the rate of 192,000 miles in a second! Well might the illustrious astronomer remark, that the telescope had the power of penetrating into time as well as into space.

VIII. Stellar Nebulæ.—These were supposed by Herschel to be in all probability clusters of stars whose light, on account of their immense distance, is collected so closely into one point, that the only evidence remaining of their resolvability into discrete bodies, consists in the appearance of burs.

IX. Milky Nebulosity.—According to Herschel, the phenomena of this class were of two distinct kinds. One of these was nebulous only in appearance, consisting in reality of so many systems of clustering collections of stars contiguous to each other, like the series of clustering collections of which he had found the Milky Way to be composed. The other species of objects consisted of those whose structure was purely nebulous. One of the most remarkable of such objects was the great nebula of Orion; which Huyghens had discovered in 1656. From a comparison of his earlier with his more recent observations of this nebula, Herschel concluded that during the intermediate period it had undergone a sensible change of aspect, a circumstance which appeared to him clearly to indicate that its structure was not sidereal.

X. Nebulous Stars.—These are stars surrounded by a pale nebulous atmosphere. Herschel had, in a former communication to the Royal Society, directed the attention of astronomers to these remarkable objects†. The star, in each case, appeared to be situate exactly in the centre of the nebulosity, whence it was evident that they were connected together by some physical relation. The question with respect to the real nature of these objects, was supposed by Herschel to be involved in great obscurity.

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in the southern hemisphere. To the naked eye it appears like a dim cometic object, equal in brightness to a star of between the fourth and fifth magnitudes. Viewed in a powerful telescope it resembles a globe of fully 20' in diameter, very gradually increasing in brightness to the centre, and composed of innumerable stars of the thirteenth and fifteenth magnitudes. (*Results of Ast. Obs.*, &c., pp. 21, 104; *Outlines of Astronomy*, p. 595.) This is the object alluded to in the text, as having been discovered by Halley in 1677, under the impression of its being a nebula. The globular cluster between η and ζ Herculis, which was also first observed by Halley as a nebula in the year 1714, is the most magnificent in the northern hemisphere. Sir William Herschel estimates the number of stars contained in it to amount to as many as 14,000! (*Phil. Trans.*, 1806, p. 230.)

* If the stars were regularly distributed in a globular cluster, this circumstance alone would manifestly cause the cluster to appear more condensed towards the centre. It appears, however, that in many cases the condensation of the cluster is greater than that which would arise from a uniform distribution of the stars. The obvious inference is, that the cluster forms an independent system, the constituent bodies of which, under the influence of their mutual attraction, have a tendency to congregate towards the centre.

† Phil. Trans., 1791, p. 71, et seq.

If the nebulosity consisted of stars so remote as to put on a milky appearance, it necessarily followed that the central body, which usually resembled a star of the seventh or eighth magnitude, must possess enormous dimensions. If, on the other hand, the central body be supposed not to exceed the ordinary size of the stars, how small, argued Herschel, must be the remaining stars of the system, and how inconceivably great must be their compression to produce the observed nebulosity. Instead of adopting either of these hypotheses, Herschel was inclined to believe that the phenomenon arose from a star of the ordinary magnitude being involved in a shining fluid of a nature totally unknown.

XI. Planetary Nebulæ.—These are nebulæ of a pale uniform aspect, and of a perfectly round form. Like the objects of the preceding class, they appeared to Herschel to be of a very mysterious nature. If they were admitted to be suns, it would be difficult to account for the faintness of their light. On the other hand, the supposition of their being sidereal aggregations was at variance with their uniform structure.

XII. Planetary Nebulæ with Centres.—The objects of this class were suspected by Herschel to form the connecting link between nebulous stars and planetary nebulæ. This view of their nature suggested to him a conclusion which he subsequently developed at greater length. "If," says he, "we might suppose that a gradual condensation of the nebulosity about a nebulous star could take place, this would be one of them in a very advanced state of compression."*

In the year 1811, Herschel communicated to the Royal Society a paper in which he gave an exposition of his famous hypothesis of the transformation of nebulæ into stars†. Assuming a self-luminous substance of a highly-attenuated nature to be distributed through the celestial regions, he endeavoured to show that, by the mutual attraction of its constituent parts, it would have a tendency to form itself into distinct aggregations of nebulous matter, which in each case would gradually condense, from the continued action of the attractive forces, until the resulting mass finally acquired the consistency of a solid body and became a star. In those instances wherein the collection of nebulous matter was very extensive, subordinate centres of attraction could not fail to be established, around which the adjacent particles would arrange themselves, and thus the whole mass would in process of time be transformed into a determinate number of discrete bodies, which would ultimately assume the condition of a cluster of stars. Herschel pointed out various circumstances which appeared to him to afford just grounds for believing that such a nebulous substance existed independently in space. He maintained that the phenomena of nebulous stars, and the changes observable in the great nebula of Orion, could not be satisfactorily accounted for by any other hypothesis. Admitting, then, the existence of a nebulous substance, he concluded, from the extensive indications of milky nebulosity which he encountered in the course of his observations, that it was distributed in great abundance throughout the celestial regions. The vast collection of nebulæ which he had observed, of every variety of structure, and in every stage of condensation, were employed by him with admirable address in illustrating the *modus operandi* of his hypothesis. The planetary nebulæ, whose nature had formerly appeared to him to be totally inexplicable, were now reasonably supposed to be nebulæ which had finally assumed the consistency of a solid body, so that the aspect exhibited by them in the telescope thenceforward necessarily failed to afford any indication of their internal structure.

Notwithstanding the ingenuity of illustration and incontestable force of reasoning by which Herschel sought to establish this bold hypothesis, it has not received that confirmation from the labours of subsequent enquirers which is so remarkable in the case of many of the other speculations of that great astronomer. It is now generally admitted, that the changes which at one time were supposed to be taking place in some nebulæ were altogether illusive, having been suggested partly by erroneous delineations of the objects as they actually appeared in the telescope, and partly by the different aspect which they necessarily assumed when viewed in telescopes possessing different degrees of optical power. Moreover, the phenomena denominated Nebulous Stars—which seemed to Herschel to be incapable of any satisfactory explanation, except by adopting the hypothesis of a self-luminous fluid—when examined with the powerful telescopes of Lord Rosse, have been found to exhibit an aspect totally different from that which appeared to Herschel so enigmatical. In fact, the greater the optical power of the telescope with which the heavens are surveyed, the more strongly do the results tend to produce the impression that all nebulæ are in reality vast aggregations of stars, which assume a nebulous aspect only because the telescope with which they are observed in each instance is not sufficiently powerful to resolve them into their constituent parts, and thereby disclose their real nature*.

The only nebulæ visible exclusively in the southern hemisphere, with which astronomers hitherto were acquainted, were those discovered by Halley and Lacaille, to which allusion has already been made. In 1828, the number of such objects was considerably augmented by the late Mr. Dunlop, who in that year communicated to the Royal Society a catalogue containing 629 nebulæ and clusters observed by him at Paramatta, in New South Wales. From the small optical power possessed by his telescope, which was a Newtonian reflector of nine inches aperture, and doubtless, also, in some degree from his inexperience in such observations, this catalogue by no means afforded a faithful representation of the objects to which it related.

In 1833, Sir John Herschel communicated to the Royal Society a paper containing the results of an examination of the nebulæ in the northern hemisphere, undertaken in the year 1825, with a twenty-feet reflector, and prosecuted during the course of the following eight years. This catalogue contained 2306 nebulæ and clusters. Of these about 500 were new: the remaining objects had been already discovered by his father. It would be out of place here to attempt to give a detailed account of this magnificent production, which is destined to form one of the great landmarks of sidereal astronomy.

In 1847, the distinguished astronomer just referred to, published his "Results of Astronomical Observations made at the Cape of Good Hope," which, among other objects of importance, contained a catalogue of 1708 nebulæ and clusters in the southern hemisphere. The observations which form the groundwork of this catalogue were made by Sir John Herschel at the Cape of Good Hope, between the years 1834 and 1838, with an instrument of the same optical power as that employed by his illustrious father and himself in their respective examinations of the nebulæ of the northern hemisphere. This great catalogue, in conjunction with the one of 1833, by the same astronomer, may be said to afford an accurate representation of all the nebulæ in the celestial sphere, which are visible in a reflecting telescope of 18 inches' aperture. From the unrivalled powers of description by which the author of these catalogues is distinguished, the monographs which they contain have imparted to them an interest extending far beyond the mere circle of strictly scientific enquirers, who see in them mainly the solid groundwork of ulterior researches of high importance in stellar astronomy.

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* It may be mentioned, in connexion with this remark, that the great nebula of Orion has finally begun to exhibit indications of resolvability, upon being observed with the 6-feet reflector of Lord Rosse, and with the powerful refractor of Cambridge, U.S. The same may be said respecting an examination of the nebula of Andromeda, by the aid of the latter telescope. For magnificent delineations of these two nebulæ, as observed at Cambridge, by Mr. Bond, the Director of the Observatory, and Mr. G. P. Bond, his son, see *Mem. of the Amer. Acad. of Arts and Sciences,* New Series, vol. iii.

The most recent contribution to the subject of nebulæ has been furnished by Lord Rosse, who in the year 1850 communicated to the Royal Society a paper containing the results of a re-examination of several of the nebulæ comprised in Sir John Herschel's catalogue of 1833*. These observations were made with the reflecting telescope of three-feet aperture, originally executed by Lord Rosse, but chiefly with the more powerful instrument of six-feet aperture subsequently constructed by him. New phenomena of a highly interesting nature have been revealed in the course of these observations. Many nebulæ, which had hitherto resisted all attempts to resolve them with instruments of inferior power, were now found to consist wholly of stars. Others exhibited peculiarities of structure totally unexpected. Among these there are several which have been found to exhibit unequivocal indications of a spiral arrangement. One of the most remarkable objects of this class is the nebula marked No. 51 in Messier's catalogue. This nebula has a smaller one in its immediate vicinity. Messier describes the two objects as a double nebula without stars. The powerful telescopes of Sir William Herschel disclosed the existence of a new feature in its structure of a very interesting kind. He has represented the object as a bright round nebula, surrounded by a halo or glory at a distance from it, and accompanied by a companion. Sir John Herschel discovered that the south following half of the ring was divided into two parts, producing an appearance bearing some resemblance to the bifurcation of the Milky Way. This circumstance suggested the probability of the nebula being a vast sidereal system, identical in structure with that to which it offered so striking an analogy. The telescopes of Lord Rosse have served to destroy this interesting surmise, by showing the nebula to be of a totally different structure from that which it exhibited in instruments of inferior power. It has been found to be in fact composed of a series of spiral convolutions, arranged with remarkable regularity. A connexion has also been traced by means of these spirals between the nebula and its companion. The number of nebulæ in which Lord Rosse had already detected unequivocal indications of a spiral structure, amounted to fourteen.

The remarkable class of objects termed Planetary Nebulæ, when viewed in the 6-feet reflector of the distinguished astronomer just alluded to, have failed to exhibit that uniform aspect which had proved such a stumbling-block to Herschel in his attempts to explain their structure. Five nebulæ, which previous astronomers had represented as possessing a round uniform disk, were found by Lord Rosse to exhibit unequivocal indications of an annular structure. Nebulæ of this description are extremely rare in the heavens. Only four of such objects had hitherto been recognised by astronomers, two in the northern and two in the southern hemisphere.

The class of objects comprehended under the designation of Nebulous Stars, whose appearance Sir William Herschel was unable to account for, except by assuming the existence of a shining fluid existing independently in space, have also exhibited, in the telescopes of the distinguished nobleman just referred to, an aspect which entirely obviates the necessity of having recourse to such an hypothesis. One of the most interesting objects of this class is the nebulous star marked No. 450 in Sir John Herschel's catalogue of 1833. The star-like point of this object, when viewed in the 6-feet reflector, is seen to be placed in the centre of a nebulous nucleus, and beyond there appears a nebulous ring completely separated from the latter. Another nebulous star, ι Orionis, has been found to exhibit a somewhat analogous aspect. The pale nebulous light surrounding the star is found to contain a dark cavity, not exactly symmetrical with respect to the star.

Of the other nebulæ examined by Lord Rosse, one of the most interesting is the one marked No. 2098 in Sir John Herschel's northern catalogue. This object has been found to exhibit *ansæ*, indicating, in all probability, a nebulous ring seen edgeways. This phenomenon appears to afford a valuable *experimentum crucis* in deciding the question relative to the structure of those nebulæ which exhibit an annular aspect. It is not difficult to see that such an appearance might be produced either by a real ring or by a hollow spherical shell of nebulous light. In the latter case, however, it would always exhibit a perfectly round appearance; whereas, in the former, there is only one position of the ring in which it would appear round. In every other position it would appear more or less elliptical, and when its plane passed through the earth, it would resemble two opposite *ansæ* attached to the central object, provided there was one. Now, since observation has, in one instance at least, disclosed indications of such *ansæ*, and thereby afforded unequivocal evidence of the actual existence of a ring composed either of stars or of a nebulous substance, we may reasonably conclude that the annular appearance exhibited by several other nebulæ is occasioned by a similar cause, and not by a hollow sphere of nebulous light. We are also warranted by the same consideration in supposing that those elliptical nebulæ which exhibit dark chinks are in reality nebulous rings of more or less regular structure, seen obliquely with respect to the line of vision, and not hollow elliptical shells of nebulous light.

Sir John Herschel has remarked, in the paper containing his catalogue of nebulæ visible in the northern hemisphere, that further discoveries of such objects can only be expected to result from the use of telescopes possessing a higher degree of optical power than a reflector of eighteen inches aperture, the instrument with which all the nebulæ hitherto known to exist were mainly discovered. The observations of Lord Rosse are plainly destined to be instrumental in realising this expectation. Although no direct search for new nebulæ has yet been undertaken by his lordship, several objects of this class, which it was impossible to discern in increase of optical power, new accessions will be constantly made to the visible extent of the universe.

The results which Lord Rosse has already achieved by means of his powerful telescopes, clearly point out the course which must be pursued in order to acquire some degree of knowledge respecting the nature and constitution of those magnificent systems of sidereal bodies which have been found to pervade the immensity of space. In the first place, it is manifest that a careful delineation of the different parts of each system forms an indispensable preliminary to any progress in this branch of enquiry. It is only by a subsequent comparison, instituted between nebulæ of every observable variety of form, that any knowledge can be obtained respecting the real structure of those wonderful systems. Among them there are, doubtless, systems of different orders, as we find exemplified to a limited extent in the solar system, but among those of the same order we have just grounds for suspecting that a kindred form of structure prevails. It is probable that such systems are viewed in every variety of position relative to the earth, and hence we may reasonably expect that, by an extensive collation of their apparent forms, it will be possible to arrive at some definite conclusions respecting their actual configuration in space. Formidable as the problem relative to the structure of these systems may appear when considered from this point of view, its difficulties are vastly enhanced by the internal changes which are constantly taking place in each system. It must be borne in mind that, although the various bodies of which each system is composed are doubtless linked together by intimate physical relations, the condition which thus subsists is one of dynamical stability rather than statical equilibrium. In fact, we are amply justified, both by reason and analogy, in supposing that the constituent bodies of every system are revolving in curvilinear orbits under the influence of their mutual gravitation. It is manifest that from this cause each system will be liable, in process of time, to undergo a modification of form. Hence it follows, that not only will systems of the same order exhibit different external forms when compared with each other, but even the same system will vary from time to time relatively to itself. That the eccentric position of the observer, and the displacements arising from the internal movements of its constituent bodies, may cause a system to appear very complicated, while at the same time its various parts are adjusted in the most perfect harmony, is evident, from a consideration not only of the planetary system itself, but also of those miniature systems of which it is mainly composed. What more admirable arrangements can the imagination conceive than those which pervade the system of Jupiter's satellites, and yet when this exquisite mechanism is casually viewed through a telescope, what can be more devoid of symmetry than the configurations of its different parts?

It is manifest, from the foregoing considerations, that, in order to acquire just ideas respecting the construction of those vast systems of worlds which the telescope has shown to be distributed throughout the immensity of space, an assiduous course of observation and research, prosecuted throughout a long succession of ages, is absolutely indispensable. The investigation of the dynamical laws by which those systems are governed and their stability is assured, must be regarded as an ulterior object. It is only in the case of the more simple systems, the relative movements of of research. To attempt to discover the mechanical structure of more complicated systems, before observations were amassed capable of forming a sure groundwork of research, would be manifestly a hopeless undertaking. All speculations directed to this end must therefore be regarded as premature, in the present state of sidereal astronomy*.

The remarkable Zone of light termed the Milky Way, which is seen encircling the starry heavens, has in all ages excited the admiration of mankind, and stimulated the enquiries of the contemplative observer of nature. It would serve no useful purpose to refer here to the erroneous opinions entertained by various ancient philosophers respecting the nature of this luminous circle. Aristotle supposed it to be composed of a substance occupying an intermediate position between the terrestrial atmosphere and the region of the stars. In no part of his great work has Ptolemy expressed an opinion respecting the nature of the Milky Way. It is a remarkable fact, however, that Democritus hit upon the true explanation of its aspect. According to Plutarch, he maintained that it was composed of a multitude of small stars, so very near to each other, that their light became blended together so as to produce the appearance of a luminous zone†. A similar opinion respecting the Milky Way is expressed by Manilius, in his poem on the sphere‡.

Copernicus has cautiously abstained from any allusion to the Milky Way, in his immortal work wherein he expounds the true system of the universe. Tycho Brahé supposed it to be formed of a nebulous substance, as is evident from his speculations on the origin of the new star which appeared in the year 1572§. A similar view of its structure was implied in Kepler's speculations on the origin of the new star of 1604‖.

The invention of the telescope, about the beginning of the seventeenth century, had the effect of conducting astronomers to a more accurate knowledge respecting the constitution of the Milky Way than had hitherto prevailed. When Galileo first explored the heavens with his telescope, he discovered everywhere, to his unspeakable admiration, a multitude of stars which were too faint to be perceived with the naked eye. The aspect of the Milky Way was found by him to arise wholly from its being composed of a vast collection of small stars in close proximity to one another. In the *Sidereus Nuncius*, a small work containing the first announcement of his telescopic discoveries in the heavens, he congratulates himself on having put an end to the ancient controversy respecting the Milky Way, by actually exhibiting its structure to the senses¶. Thenceforward it has been generally admitted, that the Milky Way is, in fact, no other than a vast assemblage of stars too small to be individually visible.

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* Among those who have partially laboured on the subject of nebulæ, no one exhibited such high qualifications for cultivating so delicate a branch of stellar astronomy as Ebenezer Porter Mason, a young American of great promise, who unfortunately died in the year 1840, at the early age of twenty-one years. An important paper, containing the results of his observations of some remarkable nebulæ, is inserted in vol. vii. of the *Transactions of the American Philosophical Society*. There is a very interesting account of the life and writings of this true son of genius, written by Professor Olmsted, of Yale College, Connecticut. (12mo, New York, 1842.)

† De Placit. lib. iii., cap. i.

‡ "An major densâ stellarum turba coronâ
Contexit flammas, et crasso lumine candet,
Et fulgore nitet collato clarior orbis."—Lib. i., cap. ix.

§ Progymnasmata, p. 795.

‖ "De Stellâ Novâ," cap. xxiii., p. 115.

Although the nature of the Milky Way was now well understood, no attempt was made for a long time to investigate the particulars of its structure, and to connect its appearance with the distribution of the stars throughout the other parts of the visible heavens. This important object was at length accomplished by Thomas Wright, in his "Theory of the Universe," a work to which we have already had occasion to allude. The author has given an exposition of his theory in nine letters addressed to a friend. Alluding to the current opinion respecting the Milky Way, that it is composed entirely of stars, he asserts that this view of its nature was supported by his own observations with a reflector of one foot focal length. The following statement embraces the more important points of his theory.

If we judge of the Milky Way by phenomena only, we must conclude it to be a vast ring of stars, scattered promiscuously round the celestial regions in the direction of a perfect circle. This view of its structure, however, does not accord with the aplanatic position and irregular distribution of multitudes of other stars of the same nature, dispersed throughout the celestial regions. It is not consistent with the harmony which pervades all the other arrangements of nature, that one portion of the stars should be disposed with the most perfect regularity, while all the others were scattered about in the utmost confusion, without any regard to symmetry. It is more probable that the whole visible creation of stars forms one vast system, the parts of which are adjusted with the most perfect harmony, and that its incongruous aspect is due to the eccentric position in which it is viewed, and to the motions of the constituent bodies relatively to each other. When we reflect upon the various configurations of the planets, and the changes which they perpetually undergo, we may be assured that nothing but a like eccentric position of the stars could occasion such confusion among bodies otherwise so regular. In like manner we may conclude that, as the planetary system, if viewed from the sun, would appear perfectly symmetrical, so there may be some place in the universe where the arrangement and motions of the stars may appear most beautiful.

If we suppose the sun to be plunged in a vast stratum of stars, of inconsiderable thickness compared with its dimensions in other respects, it is not difficult to see that the actual appearance of the heavens may be reconciled with a harmonious arrangement of the constituent bodies of such a system, relative to some common centre, provided it be admitted, at the same time, that the stars have all a proper motion. In such a system it is manifest that the distribution of the stars would appear more irregular the farther the place of the spectator was removed from the centre of the stratum towards either of the sides. It is also evident that the stars would appear to be distributed in least abundance in the opposite directions of the thickness of the stratum, the visual line being shortest in either of those directions, and that the number of visible stars would increase as the stratum was viewed through a greater depth, until at length, from the continual crowding of the stars behind each other, it would ultimately assume the appearance of a zone of light. According to this hypothesis, then, the whole of the visible stars, including the sun, form part of the system of the Milky Way, their irregular distribution being occasioned by the eccentric position of the sun, combined with their own proper motions.

There are, in all probability, various systems resembling the Milky Way differing as much in the order and distribution of their constituent bodies as the zones of Jupiter do from the rings of Saturn. We may, in fact, suppose that some systems of stars move in perfect spheres, at different inclinations and in different directions; while others, again, may revolve, like the primary planets, in a general zone, or more probably in the manner of Saturn's ring; nay, perhaps, ring within ring, to a third or fourth order[*].

In propounding his theory of the Milky Way, of which the foregoing is a brief sketch, Wright does not recognise the existence of systems of stars subordinate to that great system. He, indeed, asserts, that those cloudy spots which are resolvable into stars, might be explained by the principles which he laid down, but he does not formally assign to them a place in his theory. In the concluding letter, however, he appears to admit the existence of a multitude of sidereal systems within the boundaries of the visible universe, subordinate, of course, to the great system of the Milky Way. We also find, in the same letter, an interesting expression of his opinion respecting those nebulæ which had hitherto proved irresolvable even in the best telescopes. Taking into consideration the multitude of sidereal systems included within the confines of the visible universe, it appeared to him not improbable that the immensity of space is occupied by an endless succession of systems, analogous in their structure to the great system (the Milky Way) of which the visible universe is composed. "That this, in all probability, may be the real case," says he, "is in some degree made evident by the many cloudy spots, just perceivable by us, as far without our starry regions, in which, although visibly luminous spaces, no one star or particular constituent body can possibly be distinguished; *those, in all likelihood, may be external creation, bordering upon the known one, too remote for even our telescopes to reach.*"[†]

The speculations of Wright on the Milky Way are so consistent with sound philosophy and the results of observation, that they cannot fail to obtain the sanction of every person who submits them to a careful examination. At the time of their original promulgation, however, the attention of mathematicians had become deeply engrossed with the development of the theory of gravitation, while astronomers, on the other hand, were impressed with the necessity of introducing a corresponding degree of refinement into their observations, and establishing with the utmost possible accuracy the elements of the planetary movements. It happened, from this cause, that only individuals of the same speculative turn of mind as Wright himself were induced to adopt his theory as the basis of further enquiry. Such was the case with respect to Kant, the celebrated German metaphysician, who, in the year 1755, published a work containing an exposition of his views respecting the cosmical arrangement of the celestial bodies. In the introduction to this work, he acknowledges that the germ of his ideas on the distribution of the stars was suggested to him by the speculations of Wright on the subject. His system, indeed, does not materially differ from that of the English philosopher. A system bearing a close affinity to either, was also propounded a few years afterwards, by Lambert, in his "Cosmological Letters."[‡]

The physical constitution of the Milky Way, and its connexion with the arrangement of the stars constituting the visible universe, formed a subject of too great importance in stellar astronomy not to have engaged the attention of Sir William Herschel. The results of his early labours relative to this branch of enquiry are contained in two papers inserted in the volumes of the *Philosophical Transactions* for 1784 and 1785. His theory agrees in the main with that of Wright and the other enquirers above alluded to; but in this, as in every other instance of his speculations, he produced a strong conviction of their reality in men's minds, by establishing a close connexion between them and the results of actual observation. His powerful telescopes enabled him to penetrate far more deeply into the structure of this wonderful zone than had been hitherto possible, when, to his admiration, he found it to consist, in every part of its visible course, of an innumerable multitude of small stars *. On one occasion he found that in the short interval of one-quarter of an hour no fewer than 116,000 stars had passed through the field of view of his telescope †. On another occasion he found that in forty-one minutes of time as many as 258,000 stars had passed in review before him ‡!

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[*] It is impossible not to be struck with the connexion subsisting between these sagacious remarks, and the phenomena which Lord Rosse has recently succeeded in disclosing by means of his powerful telescopes.

[†] Theory of the Universe, p. 83.

[‡] Lambert has aptly denominated the Milky Way the *Ecliptic of the Fixed St*

It would be inconsistent with the object of this work, to enter into a detailed account of the researches of Herschel on the Constitution of the Milky Way. A remarkable method devised by him for ascertaining the configuration in space of this great sidereal system, consisted in examining the heavens at different distances from the Galactic circle, and numbering the stars visible in the field of view of his telescope. Assuming that the stars are uniformly distributed throughout space, and that the telescope suffices to penetrate to the utmost limits of the sidereal stratum constituting the Milky Way, it is manifest that the number of stars visible in the field of view of the telescope would increase with the length of the visual line, and would thereby afford an indication of the distance from the observer to the exterior surface of the Milky Way. Hence, by comparing together the lengths of the various lines found in this manner, and taking into consideration their respective distances from the Galactic circle, the actual configuration in space of the Milky Way may be ascertained. Such is a brief outline of the celebrated method of *gauging the heavens*, which Herschel practised to a vast extent in the early period of his researches on the constitution of the Milky Way §.

Although Herschel assumed as one of the fundamental points of his theory of the Milky Way, that the stars are distributed uniformly throughout space, he had recourse to this principle, not from a conviction of its being absolutely true, but because, while there was a probability of its constituting a tolerable approximation to the actual state of nature, it at the same time had the advantage of affording a convenient basis of calculation. Even as early as the year 1785, he remarked that in many parts of the Milky Way the stars exhibited indications of a tendency to collect together into separate clusters[*]. It was this circumstance which induced him to assert that some parts of our sidereal system seemed to have suffered more from the ravages of time than other parts[†]. In confirmation of this statement, he remarked that in the constellation Scorpio there existed an absolutely dark space, about 4° broad, in which not a single star was visible; while on the other hand the object marked No. 80 in Messier's catalogue, one of the richest and most compressed clusters of stars in the heavens, was situate close upon its western border[‡].

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* Phil. Trans., 1784, p. 438. † Ibid., 1785, p. 244. ‡ Ibid., 1795, p. 70.

§ These observations were made by Herschel with the 20-feet reflector, the field of view of which was 15′ in diameter. In some instances the number of stars in the field of view did not exceed a few units. On one occasion, indeed, Herschel was enabled to count only three stars in four successive gauges. The paucity of stars in the field of view was of course mainly observable in those parts of the heavens which were at a considerable distance from the Milky Way. As the gauges approached the Galactic circle the number of stars rapidly increased, amounting in many instances to between 400 and 500, and on one occasion running as high as 588. A list of these gauges, with the elements of their respective positions, is given by Herschel in his paper inserted in the *Philosophical Transactions* for 1785. They amount in number to between three and four thousand

In his paper of 1802, Herschel again referred to the manifest evidence of a tendency of the stars in the Milky Way to form themselves into distinct sidereal aggregations. In all those instances wherein small spaces of a milky aspect in a state of isolation were discernible, he found that the brightness was invariably greatest in the middle, and least around the borders; and since he had shown, by means of his powerful telescopes, that the peculiar aspect of the Milky Way arose entirely from the presence of a multitude of small stars in close proximity to each other, this gradation of brightness clearly indicated that the stars of each distinct group were clustering towards a common centre. A striking example of the tendency of the stars to collect together into separate systems was cited by him in the paper just referred to. He remarked that in the interval between β and γ Cygni, including a space of about 5°, the stars seemed to be clustering towards opposite regions of the heavens. By a computation, based upon the number of stars contained in the field of view of his telescope when directed towards different parts of this space, he found that it contained at least 331,000 stars. Hence, admitting that the stars were really clustering in equal numbers towards opposite regions of the heavens, as observation seemed to warrant, he concluded that each of such clusters contained at least 165,000 stars[§]!

In a paper which he communicated to the Royal Society in the year 1814, Herschel again alluded to the tendency of the stars constituting the Milky Way to arrange themselves into separate systems. He also took into consideration the consequence which must inevitably ensue from the continued operation of such a clustering process[‖]. By a series of careful observations he found that the clusters of various kinds were much more numerous within the Milky Way and the regions bordering upon it, than they were in the more distant regions of the heavens with respect to the Galactic circle. He considered that under the influence of the mutual attraction of the stars this clustering process would continue until it would ultimately result in the complete breaking up of the Milky Way, and the formation of a number of sidereal systems totally distinct from one another. The grandeur of this conclusion was worthy of the genius of Herschel; but it may be urged against its admission that we have no reason to suppose that the various parts of the Milky Way are not already in a condition of dynamical stability, and that the clustering patches of light by which it is distinguished throughout its visible course, are not in reality so many sidereal systems, existing independently of one another, but subordinate to the great system of which they apparently form a part.

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[*] Phil. Trans., 1785, p. 255.

[†] Ibid., p. 256.

[‡] Ibid.

[§] Ibid., 1802, p. 495.

[‖] Ibid., 1814, p. 248 et seq.

Although the hypothesis of a uniform distribution of the stars in space, combined with their arrangement, in the form of a vast stratum of inconsiderable thickness, accounts for the general appearance of the starry heavens, still it is not improbable that the gradual increase in the number of stars visible upon approaching the Milky Way, may arise, in some degree at least, from a real condensation of them towards the plane of the Galactic circle. This point can only be decided by taking into account, in conjunction with the number of stars visible in the field of view of the telescope, their relative distances as indicated by their apparent magnitudes. If we suppose the observations to be limited to stars of a certain magnitude, and to include all those of superior brightness, which are presumed, in consequence, to be nearer to the earth, the question will then be reduced to the investigation of the law of distribution of the stars, comprehended within a sphere whose radius corresponds to the apparent magnitude of the extreme stars. Now, if in such a case the stars appear more numerous upon approaching the Milky Way, it is manifest, from this circumstance alone, that they must increase in density towards the plane of the Galactic circle. Moreover it is evident that, for two such spheres of different radii, the maximum number of stars would bear to the minimum a much higher ratio in the case of the larger sphere than it would do in that of the smaller, provided there was a real condensation of the stars. The work recently published by M. Struve, entitled "*Etudes d'Astronomie Stellaire*," contains some remarkable speculations on the constitution of the Milky Way, which bear directly upon this question. By a skilful discussion of the gauges of Herschel, and the catalogues of modern astronomers, chiefly those of Bessel and Argelander, he has ascertained that the stars of the visible universe are not uniformly distributed throughout space, but that they are gradually more condensed towards the plane of the Milky Way. He moreover has succeeded, by means of empirical formulæ, in representing the law of apparent condensation with remarkable fidelity. Our limited space prevents us from giving a detailed account of his interesting researches on this occasion.

During Sir John Herschel's residence at the Cape of Good Hope, he executed an extensive series of observations, with the view of discovering whether the distribution of the stars in the southern hemisphere corresponded with the results of Sir William Herschel's similar labours, prosecuted mainly on the opposite side of the Galactic circle. In order that the observations might admit of comparison with those of his father, they were made according to the same method, and with a telescope of the same optical power. They embraced a region of the celestial sphere extending from the south pole of the Galactic circle to a distance of 150°, measured upon a great circle passing through it. The whole number of stars counted in the telescope amounted to 68,948, which were included within 2299 fields of view*. The results were found to present a remarkable agreement with those deduced by M. Struve from the gauges of Sir William Herschel. It would appear from them, that the southern hemisphere is somewhat richer in stars than the northern. This would indicate that our system is not situate exactly in the plane of the Galactic circle, but is displaced a little towards the north. The apparent position of the Milky Way presents an interesting accordance with this conclusion; for it has been found that its mean course does not coincide exactly with a great circle of the sphere, but with a parallel distant about 92° from the Galactic north pole*.

By a computation based on the gauges in both hemispheres relative to the Galactic circle, Sir John Herschel found that the whole number of stars visible in a reflecting telescope of 18 inches aperture amounts to about five millions and a half†. It may be remarked in connexion with this fact, that the whole number of stars visible to the naked eye in both hemispheres does not exceed six thousand, even in the case of persons of the most acute vision‡.

We shall conclude our labours with a brief account of the speculations of astronomers on the subject of the Extinction of Light in its passage through space. We have seen that, even as early as the sixteenth century, Jordano Bruno ventured to assert that the universe is peopled with an infinite number of suns. As soon as the telescope disclosed the actual existence of innumerable stars invisible to the naked eye, the doctrine of an infinity of worlds presented itself with greater plausibility to minds imbued with a natural propensity towards speculation. In a short paper inserted in the *Philosophical Transactions* for 1720, Halley has considered the effect which would be produced upon the aspect of the heavens if the universe was peopled with an infinite number of shining bodies §. The result of his enquiry was not unfavourable to that hypothesis. He remarked that the light of the stars diminishes at a more rapid rate than the interval included between them does, the law of diminution being represented in the one case by the reciprocal of the square of the distance, and in the other simply by the reciprocal of the distance. He moreover added, that when the stars are very remote they vanish, even in the most powerful telescopes. Upon these grounds he concluded that, even if the number of bodies was infinite, the heavens could not exhibit a uniformly illuminated aspect from their combined lustre.

Plausible as the reasoning of Halley undoubtedly is, it cannot be regarded as satisfactory when submitted to close examination. Chésaux, the Swiss astronomer, to whom allusion has already been made on more than one occasion, arrived at a different conclusion. He asserted that the light of an infinite number of shining bodies would cause the heavens to appear everywhere equally illuminated with the sun, and upon this ground he maintained that we must either abandon the supposition of infinity, or admit that light is gradually extinguished in its passage through the celestial regions. A similar view of the subject was entertained by Olbers, the celebrated German astronomer. M. Struve, in his "*Etudes d'Astronomie Stellaire,*" has given an account of some interesting speculations on this subject, which were suggested to him by his researches on the law of the distribution of the stars constituting the visible universe. He considers that positive evidence in favour of the extinction of light is afforded by the circumstance that the space-penetrating power of a telescope, when computed according to the usual principles, far exceeds its value as indicated by actual observation. Thus, according to the usual theory, M. Struve found that the space-penetrating power of the 20-feet reflecting telescope of Herschel was 74.83, the linear unit being the distance of the smallest stars visible to the naked eye. On the other hand, he found by a computation based upon the law of condensation of the stars, which he had deduced from the observations of Herschel and other astronomers, that the radius of a sphere embracing the smallest stars visible in the 20-feet reflecting telescope of Herschel, does not exceed 25.672 of the same units. It would appear, therefore, that the telescope does not in reality penetrate into space beyond the third part of the distance assigned by theory. M. Struve maintains that the inconsistence between theory and observation which is thus found to prevail, can only be accounted for by supposing that the light of the stars is gradually extinguished in its passage through the celestial regions. Adopting this hypothesis, he proceeds to investigate the results deducible from it, assuming as a fundamental principle that the light is enfeebled in geometrical progression as the space traversed by it increases in arithmetical progression. By strict computation he found that the theory which usually assigns the space-penetrating power of the telescope might be reconciled with observation, by supposing that the light of the stars was enfeebled to the extent of $\frac{1}{107}$th of its intensity in its passage through a space equal to the distance of a star of the first magnitude.

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* Struve, *Etudes d'Astronomie Stellaire*, p. 61.

† Results of Ast. Obs., &c., p. 381. The precise number of stars assigned by his computations is 5,331,572. He considers, however, that the number really visible in the telescope is much greater, as in many parts of the Milky Way the stars appeared so crowded as to defy counting.

‡ Struve, *Etudes d'Astronomie Stellaire*, p. 2.

§ Phil. Trans., 1720, p. 22.

The extinction of light occasions a diminution in the brightness of all the stars that are visible, either to the naked eye or in the telescope. M. Struve found, that for the smallest stars visible to the naked eye, the diminution of brightness is equal to $\frac{7}{100}$ths of the whole quantity. For stars of the ninth magnitude the diminution of brightness amounts to $\frac{29}{100}$ths, and for the smallest stars visible in the 20-feet reflecting telescope of Herschel the diminution of brightness is no less than $\frac{88}{100}$ths of the whole quantity.

The effect of the extinction of light upon the space-penetrating power of telescopes is equally remarkable. The 20-feet reflector of Herschel, which M. Struve calculated to have a penetrating power of 813.9, abstraction being made of the extinction of light, was found by him to have a power of only 250.7, when the influence of that principle was taken into account. In like manner he found that the space-penetrating power of the 40-feet reflecting telescope of the same astronomer was reduced from 2080.3 to 368.5, in consequence of the extinction of light. It is manifest that, if these results be admitted as true, the distances assigned by Herschel to the celestial bodies, must necessarily undergo a corresponding diminution.

If light really diminishes in geometrical progression relative to the space through which it is transmitted, it is evident that, beyond a determinate distance, the light of a star can produce no sensible effect to an observer at the earth, and therefore the apparent illumination of the general ground of the heavens will depend wholly on the light of the stars included within that limit. This circumstance has suggested to M. Struve some curious calculations relative to the effect contributed by the different classes of stars towards the brightness of the heavens, as observable in the region of the Milky Way, and in the direction of the poles of the Galactic circle. The following are some of the more interesting conclusions at which he arrived:—

1st. The stars situate beyond the reach of the 20-feet reflecting telescope of Herschel, do not contribute more than 12 per cent. to the brightness of the Milky Way.

2nd. If the effect produced by the stars visible to the naked eye be abstracted, the illumination of the ground of the heavens in the middle of the Milky Way will be expressed by 0.92007, the total illumination being represented by unity.

3rd. The total illumination of the heavens, in the direction of the poles of the Galactic circle, is only $\frac{22}{100}$ths of what it is in the middle of the Milky Way.

4th. When the combined lustre of stars of distinct classes is considered, the ratio of the illumination in the Milky Way to that in the direction of its poles, gradually increases as the stars are more remote.

5th. If we exclude from consideration the stars visible to the naked eye, the brightness of the heavens in the direction of the poles of the Galactic circle, is only one-sixth of the brightness of the Milky Way.

It must be admitted that the hypothesis of the extinction of light, from which M. Struve has deduced the foregoing interesting conclusions, cannot be considered as a principle of physics, the reality of which has been established beyond all doubt upon the solid basis of observation. It has been mentioned, that his reason for adopting this principle was founded on the circumstance of the space-penetrating power of the telescope having been discovered by him to be in reality much less than what theory indicated it to be. The question may be considered under the following aspect:—In the Milky Way the average number of stars visible in the field of view of the 20-feet reflecting telescope of Herschel, was found by M. Struve to be 122, and the distance of the smallest of such stars, as deducible from his researches on the law of the density of the stars, was determined by him to be 25.672 units. Hence, if the density of the stars in the direction of the plane of the Milky Way was uniform, as M. Struve in his researches assumed it to be, the number of stars visible in the telescope at the distance of 74.83 units, ought to be 3021. Now, the space-penetrating power of the telescope, as computed upon the ordinary principles, was found by M. Struve to be represented by 74.83 units. The conclusion would, therefore, appear to be unavoidable; either that the density of the stars diminishes towards the limits of the Milky Way, or that the light of the stars is gradually extinguished in its passage through the celestial regions to the earth. With respect to the question of a diminution of density, it is to be remarked that, since the inconsistency between theory and observation, above referred to, is presumed to manifest itself in every part of the contour of the Milky Way, it cannot be considered as available in explaining that inconsistency, unless the sun be supposed to be situate near the centre of the great sidereal disk, constituting the Milky Way. M. Struve, however, contends that we have no knowledge of the limits of the Milky Way—that to us it is absolutely unfathomable, and consequently that there does not exist any probability of the sun being situate near the centre of the Galactic circle. Upon these grounds, therefore, he maintains that the explanation of the inconsistency, by a diminution in the density of the sidereal stratum, in the direction of its principal plane, is inadmissible, *in ipso limine*.

The assertion of M. Struve with respect to the absolute unfathomability of the Milky Way, appears to be founded on certain observations cited by Sir William Herschel in the course of his sidereal researches; but, with all due deference to the authority of the illustrious astronomer of

Pulkowa, we are inclined to believe that he has not apprehended the real drift of the language used by Herschel on those occasions to which he refers. It appears to us evident, from the tenor of Sir William Herschel's remarks on the Milky Way, scattered through his various papers, that he considered it to be a vast sidereal system of definite dimensions, and, generally speaking, of ascertainable limits. In his important paper on the subject, inserted in the *Philosophical Transactions* for 1785, he has remarked, that even in those cases wherein the gauges were very high, the stars were neither so small nor so crowded, as they must have been on the supposition of a much farther continuance of them, and when certainly a milky nebulous appearance must have come on [*]. On a subsequent occasion, indeed, he cited some observations of the Milky Way, from which it appeared that, notwithstanding the application of higher and higher degrees of optical power, there still remained traces of nebulosity in the telescope, indicating that the limits of the stratum had not been reached. It is important to remark, however, that the object he had in view in citing these observations, was not for the purpose of showing that the Milky Way was unfathomable even in those parts to which his observations referred, and to which such an expression, in a certain sense, was fairly applicable; but to demonstrate that the nebulosity in the telescope was not of an ambiguous nature—that, in fact, it was attributable to the circumstance of its consisting of stars too remote to be distinctly visible by any optical aid that was available, and not to its being in reality composed of nebulous matter [†].

A more serious objection urged by M. Struve against the hypothesis of a diminution of the density of the stars in the plane of the Milky Way, is founded on the circumstance that an examination of the gauges of Herschel conducts to the same law of condensation in a direction perpendicular to that plane, with the law which he found, from the observations of Bradley and Argelander, to prevail in the immediate vicinity of the sun, as far as the stars of the eighth and ninth magnitudes. By his researches on the gauges of Herschel, he found that, at a distance from the plane of the Milky Way equal to the radius of a sphere comprehending the stars of the seventh magnitude, the density of the stars was equal to 0.41365, the mean density in the Milky Way being represented by unity. His examination of the zones of Bessel gave him 0.40525 for the density at the same distance. In like manner he found that, at a distance from the Milky Way equal to the radius of a sphere embracing the stars of the eighth magnitude, the density of the stars, as deducible from the gauges of Herschel, was represented by 0.31083; while, again, the zones of Bessel made the density at the same distance equal to 0.28410 [‡]. The near agreement of the results for both classes of stars must, indeed, be regarded as very remarkable, especially when we take into consideration the very different sources from which they were in each case derived. It may be remarked, however, that the observations from which these results were derived, however trustworthy they may be in point of accuracy, can hardly be allowed to constitute a sufficiently ample basis for establishing beyond doubt so extensive a conclusion as that relative to the law of the distribution of the stars. Moreover, the results of Sir John Herschel's gauges in the southern hemisphere would seem to indicate, that for the stars in the vicinity of the sun a different law of density prevails from that deduced by M. Struve*. On the other hand, the hypothesis of a gradual diminution of the density of the stars in the plane of the Milky Way, as well as in a direction perpendicular to that plane, is not only consonant to sound philosophy, but is also strongly suggested by analogy from observations of the arrangements prevailing throughout the other systems of the sidereal universe.

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[*] Phil. Trans., 1785, p. 247.

[†] Herschel cites six of such observations in a paper inserted in the *Philosophical Transactions* for 1817 (pp. 325, 26, 27), and four additional observations of the same nature in a paper published in the following year (*Phil. Trans.*, 1818, p. 463).

[‡] Etudes d'Astronomie Stellaire, p. 77.

The other alternative suggested by M. Struve, as an explanation of the inconsistency between theory and observation, which he encountered in the course of his researches; namely, the hypothesis of a gradual extinction of light in its passage through the celestial regions, has been objected to on very strong grounds by Sir John Herschel. If such an hypothesis were true, we might reasonably presume that, in consequence of the light being everywhere extinguished at the same distance, the Milky Way would present a uniform aspect throughout its course. As, however, observations of the actual aspect of the Milky Way do not accord with this conclusion, the hypothesis from which it is deduced is manifestly inadmissible.

It is very evident, that in the present state of sidereal astronomy the interesting question proposed by M. Struve, and discussed with so much ability by that astronomer, does not admit of a definitive solution. To attain this end it will be desirable to institute an extensive series of observations relative to the apparent distribution of the stars, both in the Milky Way and in a variety of other positions, with respect to the plane of the Galactic circle, employing at the same time telescopes of different apertures. By such means alone can we reasonably hope to arrive at reliable conclusions relative to the constitution of the great sidereal system, which presents itself to our observation under the aspect of the Milky Way, and of which the sun, as well as the greater number of the stars visible even in the most powerful telescopes, may be regarded in all probability as so many of the constituent bodies.

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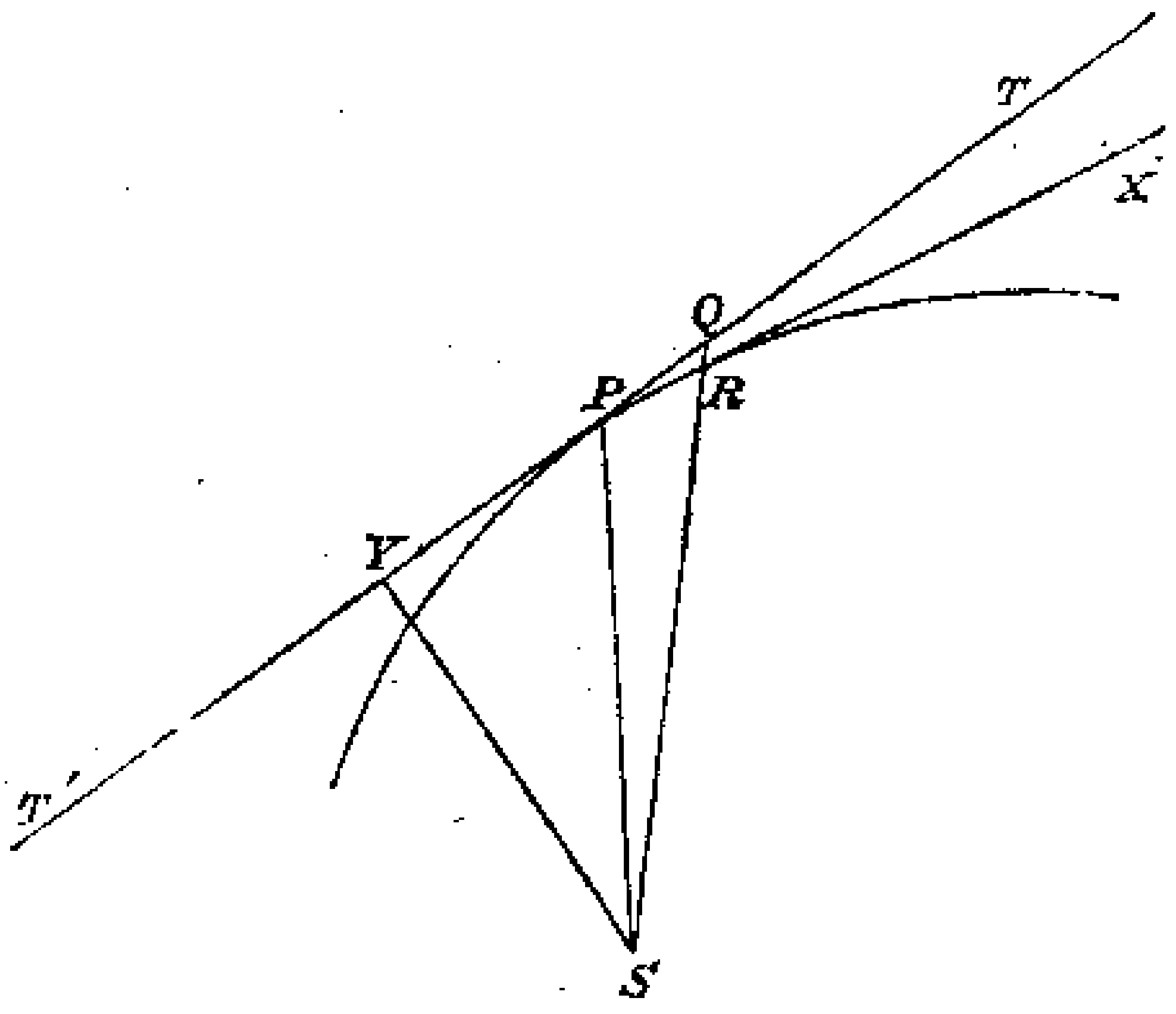
* See the table of apparent densities for stars of different magnitudes, which Sir John Herschel has given at page 382 of his *Results of Astronomical Observations at the Cape of Good Hope, &c.*

APPENDIX.

I.

Illustrations of Planetary Perturbation.

It is not intended here to attempt a detailed exposition of the principles of Planetary Perturbation. The object of the following remarks is mainly to elucidate the peculiar features of perturbation which characterise the mutual action of two revolving bodies, the mean motion of one of which around the central body is almost exactly double the mean motion of the other. Several examples of this mode of perturbation occur in Physical Astronomy, one of the most interesting of which relates to the mutual action of Uranus and Neptune; and it is chiefly with the view of exhibiting its influence in the theory of these planets that the series of illustrative notes, which we now submit to the attention of the reader, have been drawn up.



(1.) Let us suppose a body to receive an impulse in free space, and to be subjected at every instant to the action of a force tending to a fixed point S. Let P represent the initial position of the body, and let us first suppose it to be projected in the direction PT, so that the angle SPT is obtuse. If it was not acted upon afterwards by any force, it would advance along PT with a uniform velocity depending on the intensity of the impulse; and the angle contained between the radius vector and the line PT, representing the direction of motion, would continually increase. The central force at S, however, by its incessant action, prevents the body from moving in the same direction during any assignable interval of time, however short; so that while the body would have described the small space PQ with the velocity due to the impulse, it is in reality constrained to move in the curvilinear arc PR; and the angle contained between the direction of motion and the radius vector, instead of being equal to SQT, is now equal only to SRX. It appears, therefore, that the central force tends to make the angle contained between the direction of motion and the radius vector *less* than it would be, if the body had proceeded in the direction of the impulse. The same remark evidently applies to the case in which the body is impelled in the opposite direction

(2.) Since the central force acts obliquely at P, with respect to the direction of motion, it does not draw the body with its whole intensity out of the line PT. If we resolve it into two directions, one of which is parallel to the tangent PT, and the other perpendicular to that line, it will at once be seen, that only the force which acts in the latter direction is effective in pulling the body into the orbit. The intensity of this force is to that of the whole central force as the perpendicular upon the tangent to the radius vector, or as SY to SP. The remaining element of the force acts in the direction of the tangent TT′, and tends to retard or accelerate the body, according as it is proceeding towards T or T′, its intensity being to that of the whole force as PY to SP.

(3.) It appears, then, that when a body is compelled to revolve in a curvilinear orbit, by the action of a force directed constantly to a fixed point, its motion is continually retarded as it recedes to a greater distance from the centre of force; while, on the other hand, when the body is approaching the centre of force, its motion is continually accelerated. It may be shewn, by a strict investigation founded on dynamical principles, that the motion of a body, when maintained under such circumstances, is so regulated, that the areas swept over by the radius vector are proportional to the times in which the corresponding curvilinear spaces are described by the body. This celebrated theorem was first discovered by Kepler, who found it to be applicable to the elliptic movements of the planets; and it was subsequently demonstrated by Newton to be true in every case of curvilinear motion depending on the action of a central force. (*Principia*, lib. i., prop. i.)

(4.) Although the central force at S tends to make the angle contained between the direction of motion and the radius vector less than it would have been if the body had been allowed to proceed in the direction of the impulse, it does not necessarily follow that the same angle, when considered solely with reference to the orbit in which the body revolves, will actually undergo a diminution of magnitude. Thus, although the angle SRX representing the new inclination of the path of the body to the radius vector, is less than SQT, it is not necessarily less than the angle SPT, representing the angle of inclination at P. In fact the angle contained between the line representing the direction of motion and the radius vector, or the *tangential angle*, as we shall hereafter for the sake of brevity denominate it, may continually increase or continually diminish, or it may even constantly retain the same magnitude; the question of its variation at any given point depending on the circumstances which determine the motion of the body at that point; namely, the relative values of the radius vector, the tangential angle, the velocity, and the force. It is manifest that if the deflection of motion at any point be exactly equal to the angular displacement of the radius vector, the tangential angle will not vary; if it be greater than the displacement of the radius vector, the tangential angle will diminish; if less, the same angle will increase.

(5.) It may be demonstrated that if the velocity of a body revolving in a curvilinear orbit under the influence of a force tending to a fixed point, be less than that of a body revolving at the same distance in a circular orbit under the influence of a force of equal intensity, directed to the centre of the circle, the tangential angle will diminish; but if the velocity be greater than that in a circle under similar circumstances, the tangential angle will increase.

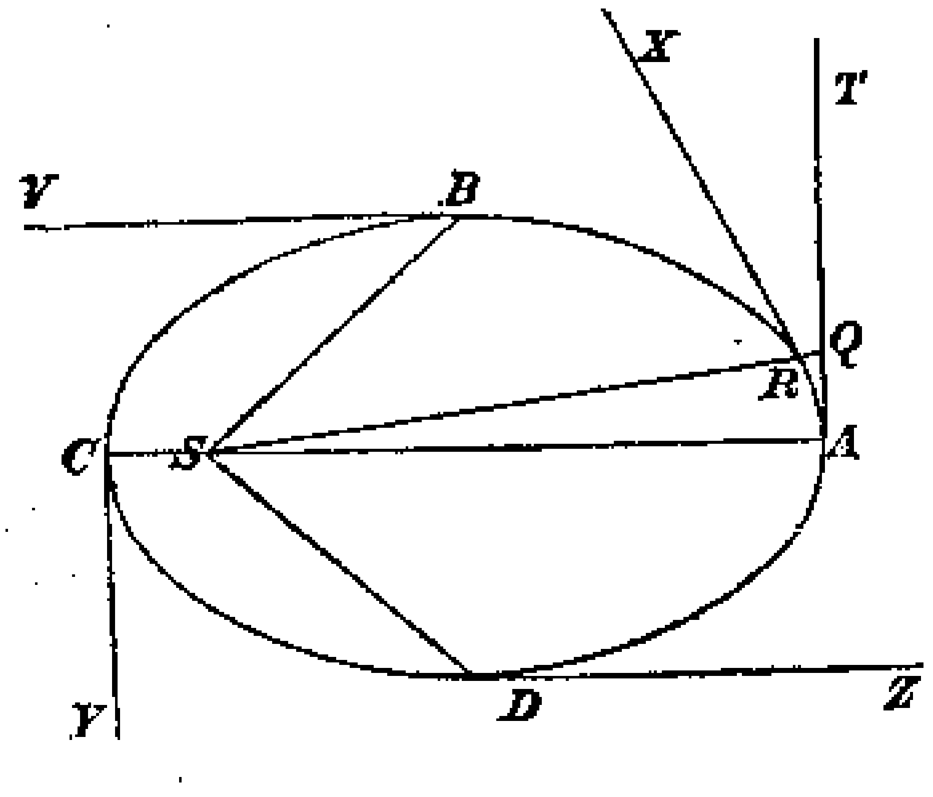
that in the circle at the same distance, the tangential angle will neither increase nor diminish at that point; and, therefore, it may be regarded as invariable for an infinitely short space of time. If this relation hold good for every point of the orbit, the tangential angle will always retain the same magnitude. The circle is a particular example of the orbit described under such circumstances; the force being supposed to be directed continually to the centre of the circle. In this case the tangential angle is always equal to a right angle. The orbit generally corresponding to any constant value of the tangential angle, is represented by a curve termed the equiangular spiral. Newton has demonstrated (*Principia*, lib. i., prop. ix.), that the force required to maintain a body revolving in such a spiral, varies inversely as the cube of the distance.

(7.) Since the circumstances which determine the motion of a body in a circle are, the relative values of the distance, velocity, and force, it is clear that the invariability of the tangential angle, in any case of curvilinear motion maintained by a central force, will be independent of the actual magnitude of the tangential angle at the point under consideration.

(8.) We have seen that if the motion of a body be supposed to be free, after it has received an impulse in any direction, the angle contained between the direction of motion and the radius vector will continually increase. Hence if the angle of projection be acute, there is a point in the subsequent path of the body at which the direction of motion is perpendicular to the radius vector. In the case of curvilinear motion, it is manifest that if the tangential angle at any point be acute, it would gradually increase so as to become equal to a right angle, provided the body was allowed to proceed in the direction of the tangent. Thus, if the body revolving at P was allowed to proceed in the direction P T′, uninfluenced by any force, the motion would be perpendicular to the radius vector; or, in other words, the tangential angle would be equal to a right angle, when the body arrived at Y. Beyond this point the tangential angle would continually increase, approaching nearer than any assignable limit to two right angles. It follows, therefore, that when the tangential angle is acute, the body is naturally approaching an apse; and, since the central force generally tends to diminish the tangential angle, its effect in such a case is manifestly to *retard* the arrival of the body at the apse. It is clear also, that the more intense the central force is, the more influential will it be in producing such an effect.

(9.) When the tangential angle is obtuse, the body, if free to move in the direction of the impulse, would continually recede from the position in which the direction of motion is perpendicular to the radius vector. The central force, however, by tending to diminish the tangential angle, manifestly retards the recess from perpendicularity; and if the deflective influence of the force so far prevail over the natural tendency of the body to persevere in the same direction as to occasion an actual diminution of the tangential angle, the latter may be reduced to such an extent as to become ultimately equal to a right angle. In such a case it is manifest that the more intense the force is, the *sooner* will the perpendicularity of the motion with respect to the radius vector be effected. It has been remarked that the contrary is true when the tangential angle is acute; or, in other words, when the body is approaching an apse, independently of the action of any force.

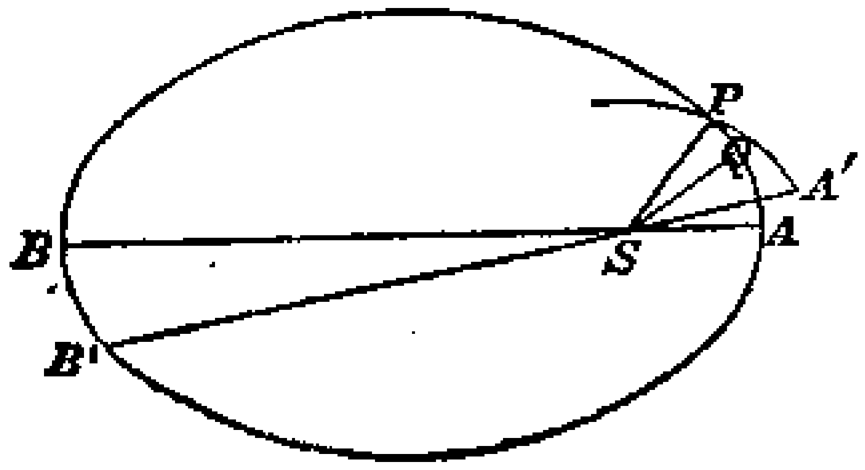
(10.) Let us now consider the motion of a body which is compelled to revolve in an elliptic orbit by the action of a force directed continually to one of the foci. Let A B C D represent the extremities of the axes of the ellipse, and let the force tend to the focus S. Let us suppose the body to have been originally at the upper apse A, and to have received an impulse in the direction A T at right angles, to C A. If no force afterwards acted upon the body, it would have proceeded with a uniform motion along the tangent A T, and the angle contained between the direction of motion and the radius vector would have continually increased. The influence of the force at S, however, is such that while the planet would have described the small space A Q in virtue of the impulse, it is in reality constrained to move in the curvilinear arc A R. The amount of deflection also is more than that which would be required in order to maintain the perpendicularity of the motion with respect to the radius vector; for the new tangential angle S R X is not only less than the obtuse angle S Q T, but is even less than the right angle S A T, representing the angle of projection. From A to B the deflection of motion is more than adequate to compensate for the angular shifting of the radius vector, and the tangential angle in consequence diminishes. The momentary decrement of the angle, however, continually diminishes also from A, and finally vanishes altogether when the planet arrives at B. At this point of the orbit, the deflection of motion exactly compensates for the angular shifting of the radius vector, and the tangential angle remains for an instant invariable. As the planet revolves from B, the angular shifting of the radius vector, now very rapid, from the continued acceleration of the planet, exceeds the corresponding deflection of motion; and the tangential angle gradually opens out, becoming again equal to a right angle at C, when the radius vector has swept over an arc of 180° from A. At this point, therefore, the direction of motion represented by the tangent C Y, is exactly opposite to the direction of the original impulse. As the planet revolves from C to D, the momentary deflection of motion is still inadequate to compensate for the angular displacement of the radius vector, and the tangential angle continues to open out. The momentary increment of the angle, however, which increased from nothing at B, until it attained its maximum at C, has since been continually diminishing, and finally vanishes when the planet arrives at D. At this point the deflection of motion again exactly compensates for the angular displacement of the radius vector, and the tangential angle for an instant neither increases nor diminishes. As the planet revolves from D, the deflection of motion is now more than adequate to compensate for the angular shifting of the radius vector; and, as a necessary consequence, the tangential angle diminishes, becoming once more equal to a right angle when the planet has returned to its original position A. The momentary decrement of the angle increases from nothing at D, until it attains its maximum at A; from which point it continually diminishes, and finally vanishes at B, as has been already stated.



(11.) It appears, then, that when the planet is revolving from mean distance to mean distance through the lower apse, the deflection of motion occasioned at each successive instant by the action of the central force, is *inadequate* to compensate for the angular shifting of the radius vector, and the tangential angle in consequence continually enlarges; but, on the other hand, when the body is revolving from mean distance to mean distance through the upper apse, the deflection of motion is *more than adequate* to compensate for the angular shifting of the radius vector, and the tangential angle in consequence continually diminishes. Hence the tangential angle attains its greatest contraction when the planet in the course of descending to the lower apse has arrived at the mean distance, and it has opened out to its greatest extent when, in the course of ascending to the upper apse, the planet has again receded as far as the mean distance. In the former case it will be represented by S B V, and in the latter by its supplement S D Z. Moreover, the enlargement of the tangential angle is most rapid when the planet is passing through the lower apse, and its diminution most rapid at the upper apse; while again, in each of the positions of mean distance, it remains for an instant invariable.

(12.) It has been stated (6), that if, in any case of a body revolving in a curvilinear orbit, under the influence of a force tending continually to a fixed point, the deflection of motion caused by the force at any assigned point, should exactly compensate for the angular displacement of the radius vector, so that the tangential angle should remain for an instant invariable, the velocity at that point is equal to the velocity in a circle at the same distance. Now, since in the foregoing case the tangential angle remains for an instant invariable when the planet is at the mean distance, we may conclude that the velocity is then equal to the velocity in a circle at the same distance. This theorem was originally deduced by Newton from his investigation of the laws of elliptic motion maintained by the action of a force tending to the focus (*Principia*, lib. i., prop. xvi., cor. 4.).

(13.) Let us now suppose a planet to be revolving in an elliptic orbit, and to be disturbed by a small force acting for a short space of time in the direction of the radius vector. Let us assume the eccentricity of the orbit to be so inconsiderable that the radius vector does not deviate sensibly from a perpendicular to the planet's motion. In such a case the disturbing force tends wholly to increase or diminish the tangential angle according as it acts outwards or inwards with respect to the central force. Let us suppose it to act inwards so as to diminish the tangential angle, and let us first consider the case in which it disturbs the planet a little after the passage of the perihelion. Since the tangential angle is in the course of opening out, the action of the disturbing force will make it equal to the tangential angle at Q, a point less advanced in the orbit. Now when the planet is in the vicinity of either apse, its distance from the focus varies very slowly, and the same is true respecting the velocity and the force. Hence the circumstances which determine the path of the planet at P and Q may be regarded as identical. The new orbit of the planet may therefore be represented by supposing the line of apsides of the original ellipse to have revolved in the direction of the planet's motion through an angle equal to P S Q. In other



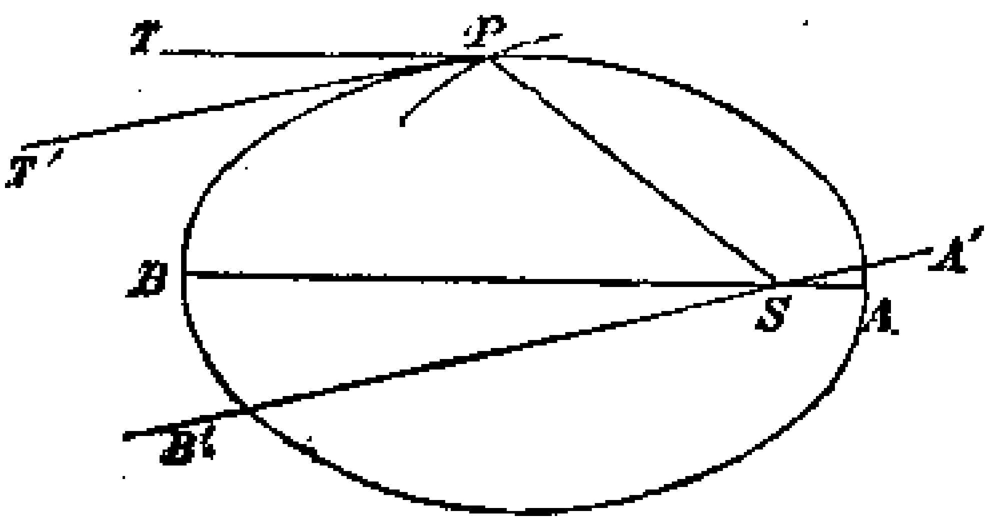
words, the effect of the disturbing force is *to make the line of apsides to progress.*

(14.) It is manifest that the increment of the tangential angle in passing from Q to P is equal to the excess of the angular displacement of the radius vector over the absolute deflection of motion corresponding to the arc included between the same two points. Hence the diminution of the tangential angle at P due to the disturbing force is *less* than the angle P S Q representing the progression of the apsides, by the absolute deflection of the motion of the planet in passing from Q to P.

(15.) Let us now suppose that P has revolved to a sufficient distance from the perihelion to occasion a sensible difference in the circumstances of motion at P and Q. We have seen (11) that the effect of this difference is such as to make the tangential angle open out more slowly at P than at Q. Hence it is evident, that if we trace back the motion of the planet in the new orbit from P, the place of the perihelion will be reached later than if the motion had been similarly traced back from Q, the place of the corresponding tangential angle in the original orbit. The line of apsides in this case will therefore have advanced through an angle A S A′ (see the figure on the preceding page), which will be somewhat *less* than the angle P S Q. It is manifest also, from the circumstance of the velocity and force at P being less than the velocity and force at Q, that the motion of the planet at the lower apse will now be slower than it was in the original orbit; and since the area momentarily described by the radius vector is not altered by the disturbing force, it follows that the perihlion distance of the planet will be greater than it formerly was. Now, the mean distance being independent of the disturbing force (inasmuch as its direct effect is supposed to be confined to an alteration of the tangential angle), it is a necessary consequence of the perihelion distance being *greater* that the aphelion distance should be *less.* It is manifest, therefore, that besides causing a progression of the apsides, the disturbing force tends to *diminish* the eccentricity as the planet revolves from the perihelion.

(16.) It is easy to perceive, from geometrical considerations, that the eccentricity is diminished by the disturbing force, for unless the perihelion distance of the new ellipse was somewhat greater than that of the undisturbed ellipse, it would be impossible for the two orbits to intersect each other in P, so that the planet should subsequently move within its original path—a condition necessarily implied by the diminution of the tangential angle at that point.

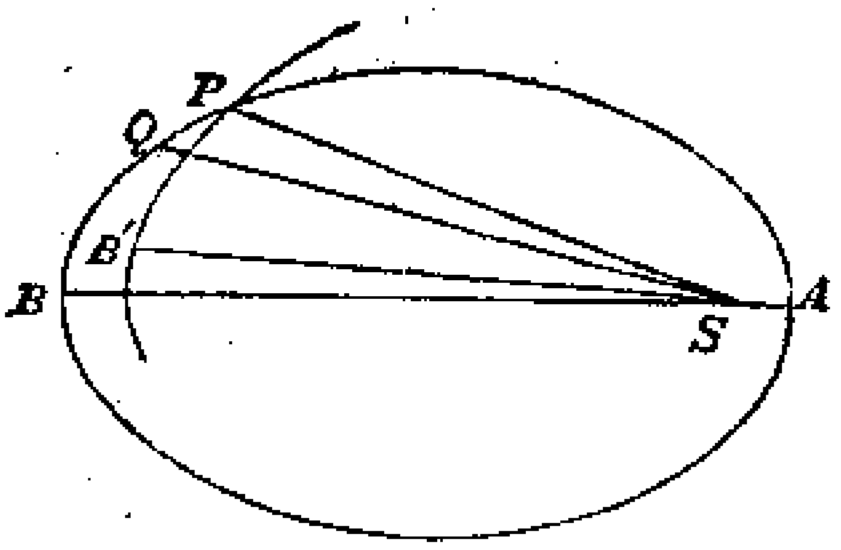
(17.) Let us now suppose that the planet while receding from the sun has arrived at the mean distance, and let the disturbing force act. At this point the momentary variation of the tangential angle vanishes. Now, it has been remarked (7) that the fulfilment of this condition is independent of the actual magnitude of the tangential angle; it will therefore still hold good in the present case, notwithstanding the diminution of the tangential angle occasioned by the action of the disturbing force. But the point at which the momentary variation of the tangential angle vanishes, is that at which the same angle is a maximum or a minimum, and in an elliptic orbit is one of the points



of mean distance. Hence it follows that in the new, as well as in the undisturbed ellipse, the planet is at the mean distance. Let P denote the place of the planet, P T the tangent of the undisturbed orbit, and P T′ the tangent of the new orbit. Now, since in an ellipse the tangent at either extremity of the minor axis is parallel to the major axis, the position of the latter will be determined by drawing A′ S B′ parallel to P T′. The line of apsides has therefore progressed through an angle equal to the amount of deflection occasioned by the disturbing force.

(18.) The eccentricity of the new ellipse will be determined by the angle S P T′, representing the maximum value of the tangential angle. The greater this angle is the more eccentric is the ellipse. In the present case this angle is less than it was in the undisturbed orbit. Hence it appears that the eccentricity is *diminished* by the action of the disturbing force.

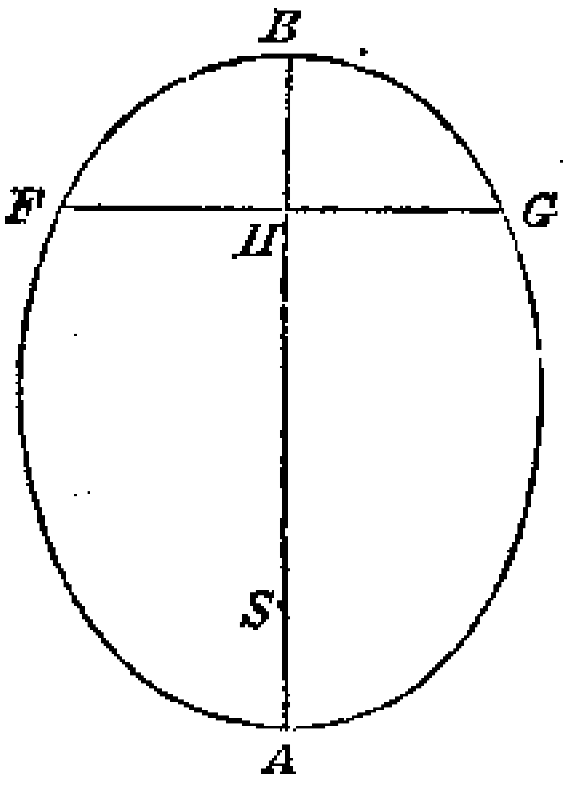
(19.) Let the planet now be moving towards the aphelion, and, when it has arrived at P, let it be disturbed by a small force tending to increase the central force at S. Since the tangential angle is now in the course of contracting, the immediate effect of the disturbing force will be to make it equal to the tangential angle at Q, a point more advanced in the orbit; and since the distance, velocity, and force may be supposed to be equal at P and Q, in consequence of the vicinity of the planet to the apse, the circumstances which determine its path may be regarded as identical at those points. Hence, the new orbit of the planet may be represented by supposing the major axis of the original ellipse to have revolved through an angle equal to P S Q, in a direction contrary to that of the planet's motion; in other words, the line of apsides has *regressed.*



(20.) In the foregoing case it has been assumed that the circumstances of motion at P and Q are identical. Let us suppose, however, that while the planet is moving towards the aphelion, its distance from the apse at the instant when the disturbing force acts, is sufficiently great to occasion a sensible difference in the circumstances of motion at P and Q. Since the tangential angle is closing more slowly at P than at Q (11) the planet will be somewhat longer of arriving at the apse in the new orbit than if it had started in the original orbit from Q. Hence it is obvious that the line of apsides will have revolved through an angle B S B′ (see the above figure), somewhat *less* than the angle P S Q. In this case, then, the regression of the apsides is less considerable than that which results when the planet is in the immediate vicinity of the apse. Again, since the velocity at P is presumed to be sensibly greater than that at Q, while at the same time the motion of the planet in either case is almost perpendicular to the radius vector, it is clear that the velocity of the planet at the aphelion will now be quicker than it was in the original orbit. The eccentricity has therefore been *diminished* by the disturbing force. This, indeed, is readily perceived from the mode in which the two ellipses intersect each other; for the aphelion distance of the new orbit is manifestly *less* than that of the original orbit.

(21.) It will be found, by reasoning precisely similar to that employed above, that when the planet is revolving from the aphelion to the perihelion, the line of apsides will *regress* if the disturbing force should act after the passage of the aphelion, but will *progress* if it should act at the mean distance or before the passage of the perihelion; while again, the eccentricity in each of these three cases will be *increased* by the disturbing force.

(22.) We have seen that if the disturbing force should act when the planet is at the perihelion, the effect is then thrown wholly upon the apsides, which rapidly *progress*; that at the mean distance the apsides also *progress*, (though with less rapidity, the effect now being thrown partly upon the apsides and partly on the eccentricity); but that if the disturbing force acts when the planet arrives at the aphelion, the effect is again thrown wholly upon the apsides, which, however, in this case *regress*. Hence it is obvious, that there must be some intermediate point of the orbit between the mean distance and the aphelion at which the disturbing force produces no effect on the position of the line of apsides. Similarly, it is manifest that there must exist some point between the aphelion and the subsequent point of mean distance, at which the line of apsides does not undergo any change of position from the action of the disturbing force. It may be found by a simple investigation, to which we shall presently allude more particularly, that the two points in question are the extremities F G of the ordinate passing through H the upper focus of the ellipse. In fact, as the planet revolves from F to G through A, the line of apsides everywhere progresses from the action of the disturbing force, the amount of progression increasing from nothing at F until it attains its maximum at A, and subsequently diminishing until it vanishes again at G. Similarly, from G to F through B, the line of apsides everywhere regresses from the same cause, the amount of regression being greatest at A, and diminishing in either direction towards F and G.



(23.) We have seen that if, when the planet is revolving from the lower to the upper apse, the disturbing force act a little after the passage of the perihelion, at the mean distance, or a little before the passage of the aphelion, the eccentricity is in each case *diminished*; but that, on the other hand, when the planet is revolving from the upper to the lower apse, the eccentricity in each of the corresponding cases is *increased* by the action of the disturbing force. Generally it may be shewn, that from A to B through G the eccentricity is everywhere diminished by the action of the disturbing force, the amount of diminution increasing from nothing at A until it attains its maximum at G, and subsequently diminishing until it vanishes at B; and on the other hand, that from B to A through F the eccentricity is everywhere increased by the action of the disturbing force, the amount of increase being greatest at F, and diminishing from that point towards A and B. Thus it appears, that when the variation in the position of the apsides is greatest, the variation of the eccentricity is least, and *vice versâ*.

(24.) If we suppose the disturbing force to act in the direction of the radius vector so as to diminish the central force at S, it will be found, in a similar manner, that the effect both upon the eccentricity and the apsides will now be precisely the reverse of that produced when the disturbing force acts inwards. In this case the eccentricity will increase from A to

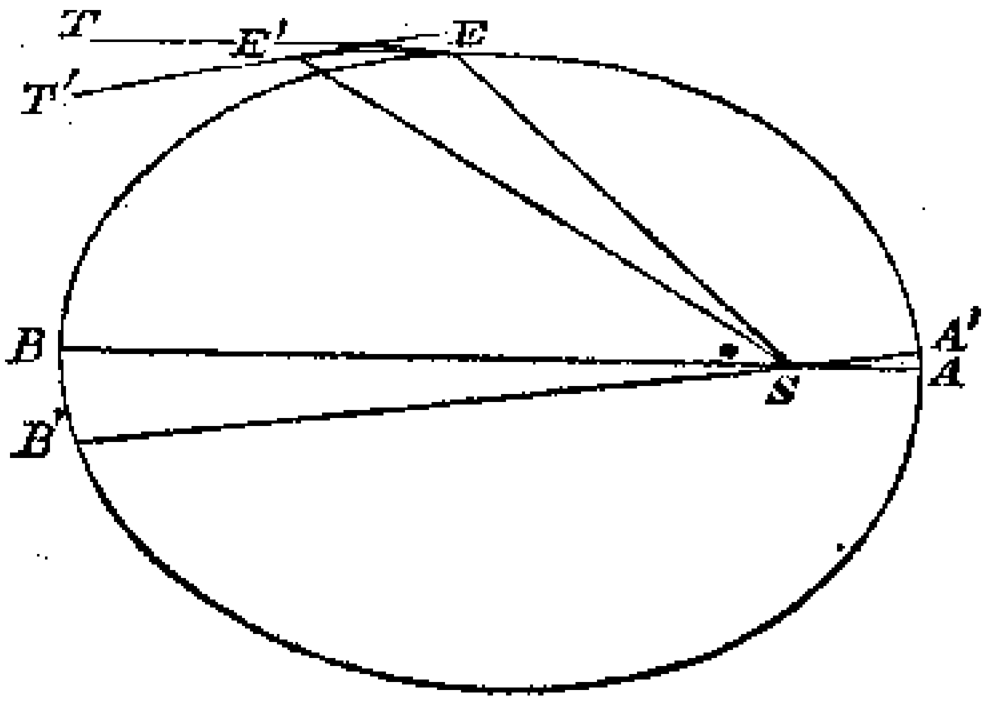
B through G, and will diminish from B to A through F; and on the other hand, the apsides will regress from F to G through A, and will progress from G to F through B. The points where the variations of both elements attain their maximum values, and also those at which they severally vanish, will be the same as in the former case.

(25.) Let us now consider the effect of a small disturbing force acting in a direction perpendicular to the radius vector. In all such cases the eccentricity of the orbit is supposed to be so inconsiderable, that the disturbing force may be regarded as acting in the direction of the tangent, and consequently as tending wholly either to accelerate or retard the motion of the planet. Let us suppose, then, that it tends to increase the velocity of the planet, and first let it act when the planet is at the perihelion. The velocity being now increased, the central force will have less control over the planet, and the latter in consequence taking a wider sweep, will now recede, farther at the mean distance. It is manifest, also, since the tangential angle continually enlarges from the perihelion to the mean distance, that it will now open out to a greater extent than it formerly did. Now, the greater the maximum value of the tangential angle, the more eccentric is the orbit. Hence the effect of the disturbing force is to increase the mean distance, and also the eccentricity. It is manifest that the position of the line of apsides cannot suffer any alteration from the action of such a force.

(26.) Let us now suppose the disturbing force to act at the aphelion. Since the velocity is increased, the central force will be less effective in deflecting the motion of the planet, and the latter in consequence taking a wider circuit, will not approach so near the centre of force at the mean distance as it formerly did. Moreover it is manifest, since the diminution of the tangential angle continues from the aphelion to the mean distance, that when it attains its minimum value, it will be less acute than it formerly was. Hence the effect of the disturbing force in this case is, to increase the mean distance, and to diminish the eccentricity.

(27.) It is manifest that the conclusions above deduced are equally applicable if we suppose the disturbing force to act at a little distance on each side of the apse, whether the latter refer to the perihelion or the aphelion, for the circumstances which determine the path of the planet are then almost the same as if the disturbing force had acted exactly at the apse.

(28.) Let us suppose the planet to be revolving from the lower to the upper apse, and let the disturbing force act when it has arrived at the mean distance E. At this point the deflection of motion in the undisturbed orbit exactly compensates for the angular displacement of the radius vector, and the tangential angle in consequence remains for an instant invariable. The velocity of the planet, however, being now increased by the action of the disturbing force, the momentary deflection of motion will be diminished, and therefore the tangential angle will still continue to open out. Let E′ be the point at which the momentary deflection of motion in the



responding change of direction of the radius vector, and let E′ T′ be a tangent to the orbit at that point. Then, since the angle S E′ T′ is greater than S E T, it follows that the eccentricity is *increased* by the disturbing force. It is evident, however, that the increase of that element is very small, for the planet is in the most favourable position for the retardation of its motion by the central force, and consequently the deflection of motion is speedily brought to an equality with the angular displacement of the radius vector. The position of the major axis of the new orbit will be determined by drawing A′ B′ parallel to E′ T′, whence it is evident that the line of apsides has *progressed*. It may be shewn, in a similar manner, that when the planet is revolving from the upper to the lower apse, the effect of the disturbing force at the mean distance is, to *diminish* the eccentricity, and to make the line of apsides *regress*. In every point of the orbit, the mean distance is obviously increased by the action of the disturbing force.

(29.) Since the eccentricity is increased at E and diminished at B by the action of the disturbing force, it is evident that there is some intermediate point at which the disturbing force produces no effect upon that element. For a similar reason, there must be some point between B and the following point of mean distance at which the eccentricity does not undergo any change from the action of the disturbing force. These neutral points are found by strict investigation to be F G, the extremities of the ordinate passing through the upper focus of the ellipse. (See the figure at page 590).

(30.) Generally it may be shewn, that from F to G through A, the eccentricity is everywhere increased by the action of the disturbing force, the variation increasing from nothing at F until it attains its maximum at A, and subsequently diminishing by equal degrees until it vanishes at G. On the other hand, from G to F through B, the eccentricity is everywhere diminished by the action of the disturbing force, the variation being a maximum at B, from which point it diminishes towards F and G.

(31.) With respect to the line of apsides, it has been found to progress if the planet, while revolving from the perihelion to the aphelion, should be at the mean distance when the disturbing force acts, and to *regress*, if the planet, in the course of revolving from aphelion to perihelion, should have arrived at the corresponding point of the orbit; while, again, at either apse it does not undergo any change of position. Generally it may be shewn, that from A to B through G, the line of apsides progresses, and that from B to A through F, it regresses, the variation attaining its maximum values at F and G, and vanishing at A and B. Thus it appears, that in passing from a disturbing force acting in the direction of the radius vector, to one acting at right angles to that direction, an interchange takes place between the points of maxima and minima of the variations of the eccentricity and the apse.

(32.) If the disturbing force act in a direction contrary to that of the planet's motion so as to diminish the velocity, the effects produced upon the eccentricity and the line of apsides will be precisely the reverse of those abovementioned. The eccentricity will increase from A to B through G, and will diminish from B to A through F; while again the line of apsides will regress from F to G through A, and will progress from G to F through B. Moreover the points at which the variations attain their maximum values, and also those at which they vanish, will be the same as in the case wherein the disturbing force tends to increase the velocity of the planet.

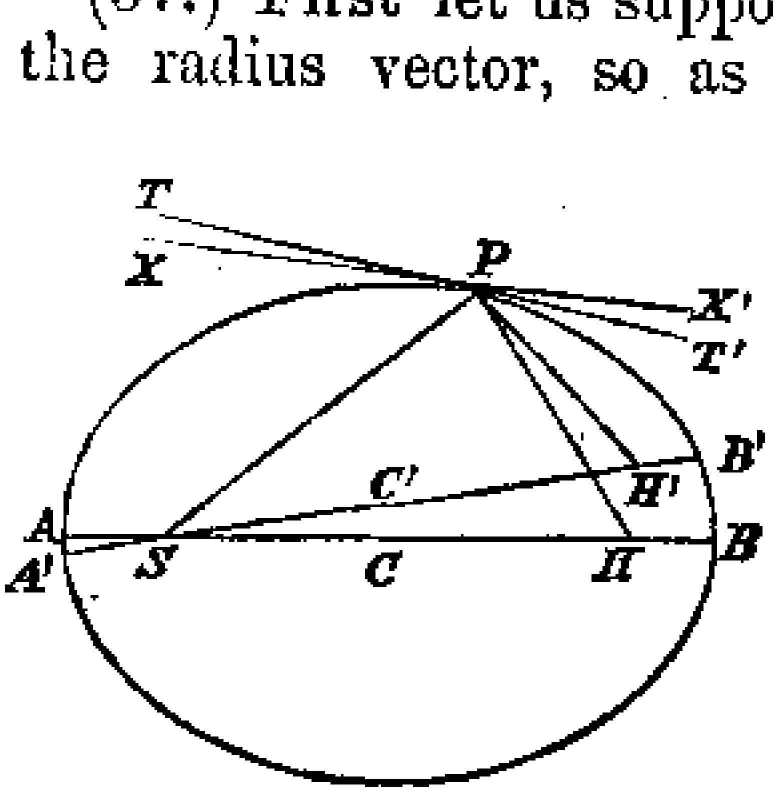
(33.) The variation induced in the position of the line of apsides by a disturbing force of given intensity is greater, as the eccentricity of the orbit is less. The truth of this proposition will appear manifest on a very slight consideration of the subject. It is easily seen that a variation in the position of the line of apsides is tantamount to a variation of the tangential angle in any point of the orbit. Now the variation of the tangential angle is slower as the eccentricity is less, the difference between its least and greatest values continually diminishing, until at length, when the orbit becomes a circle, the difference vanishes altogether, and the tangential angle is constantly of the same magnitude. The same is obviously true if the difference refer to any two values of the tangential angle comprehended within the extreme values. Hence it follows that the displacement of the line of apsides which will be required in order to adapt the orbit to a given alteration in the magnitude of the tangential angle due to a disturbing force of given intensity at any point, will be greater as the eccentricity of the orbit is less.

(34.) Since the tangential angle varies to a greater extent as the disturbing force is more intense, we may therefore infer conversely, that in order to induce an alteration of given magnitude in the position of the line of apsides, the intensity of the disturbing force must be greater as the orbit is more eccentric.

(35.) Hitherto we have supposed the disturbing force to act for a short space of time and then to cease. If its action be constantly kept up as in every case of planetary perturbation, the alteration effected in any of the elements of the orbit during a given interval of time, may be ascertained by investigating the change for each successive instant, and then summing up the results. It is easy by means of the foregoing principles to determine the *character* of the effect produced in any such case, although its exact amount can only be ascertained by a process of computation based on the principles of the infinitesimal calculus.

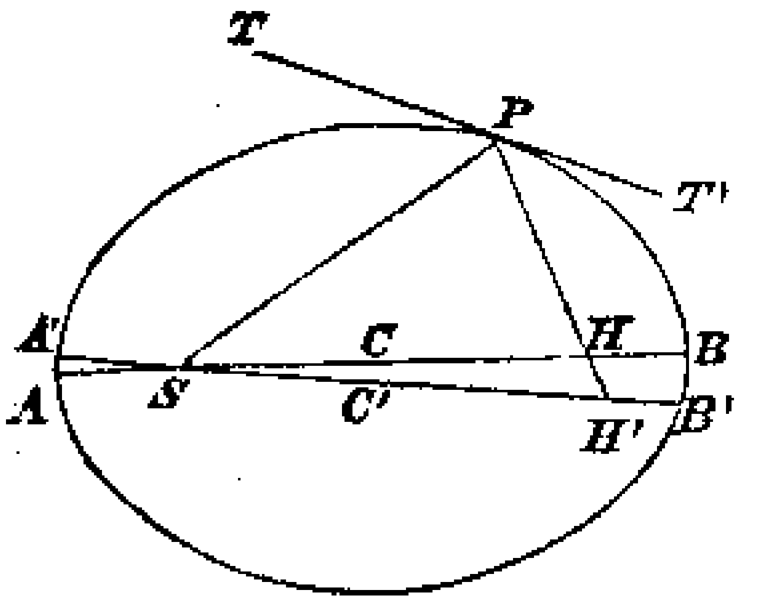
(36.) Mr. Airy has shewn (*Gravitation, notes to Arts.* 50 *and* 65), that by a slight modification of the figure given by Newton in Prop. XVII. of the first book of the *Principia*, the effects produced on the eccentricity and the position of the line of apsides by a force acting either in the direction of the radius vector, or along the tangent of the orbit, may be clearly exhibited to the eye. Sir John Herschel has actually employed this mode of expounding the variations of the elements in question in his recently-published *Outlines of Astronomy*. The simplicity and elegance of the investigation will amply justify its insertion here.

(37.) First let us suppose the disturbing force to act in the direction of the radius vector, so as to increase the attractive force at S. We have seen that the direct effect of such a force is to diminish the tangential angle. Let it act at P, and let the tangent T T′ at that point be deflected in consequence, so as now to occupy the position X X′. Draw P H′ so that the angle H′ P X′ shall be equal to S P X, the new value of the tangential angle. From P, set off P H′ equal to P H. Bisect S H′ in C′. Then is S C′ the new eccentricity of the orbit, and A′S H′B′ the new position of the line of apsides. For, by the property of the ellipse, the two lines



tangent. Hence the upper focus of the new ellipse must be somewhere in the line P H′. Again, by another property of the ellipse, the sum of the same two lines is equal to the major axis. Hence S and H′ will represent the foci of an ellipse whose tangential angle at P is equal to S P X, and whose major axis is equal to that of the undisturbed orbit. But by the principles of dynamics, the major axis of the ellipse is not altered by the disturbing force. Hence the truth of the proposition is manifest. By varying the position of P, and supposing the disturbing force as tending either to increase or diminish the attractive force at S, the different results referred to in (22), (23), and (24) may be very easily deduced.

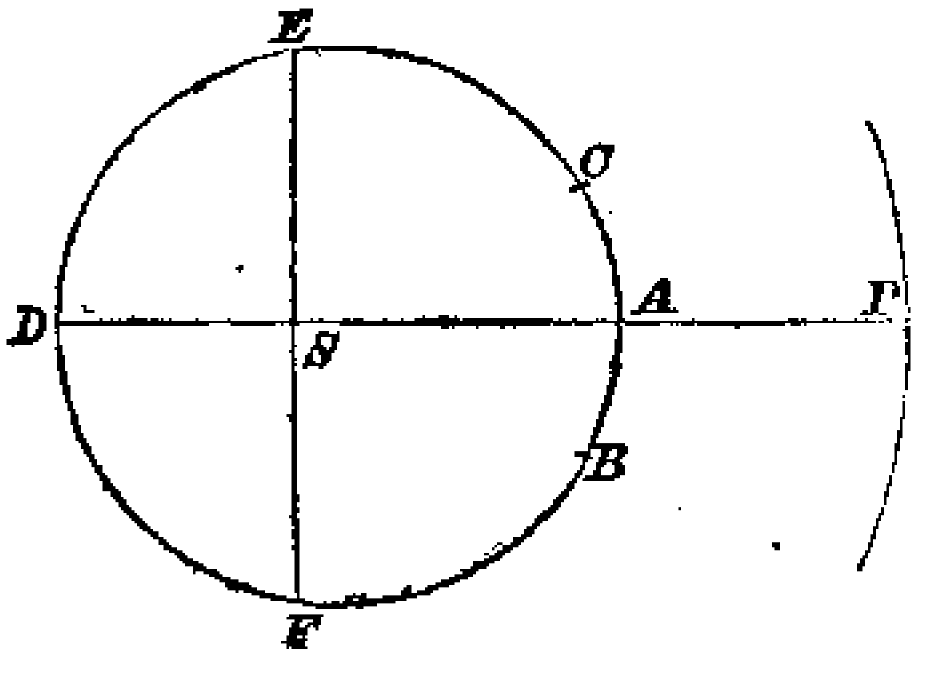
(38.) Next let the disturbing force act in the direction of the tangent, so as to retard or accelerate the velocity of the planet. Let it be supposed to increase the velocity and let P be the point at which it acts. In this case the tangential angle at P is not affected by the disturbing force, but the major axis of the ellipse is increased by its action. Produce P H so that H H′ may represent the increment of the major axis occasioned by the disturbing force. Join S H′, and bisect the line S H in C′. Then will S C′ represent the new eccentricity of the orbit, and A′ S H′ B′ the new position of the line of apsides. The truth of this proposition is so obvious as to render any formal demonstration of it superfluous. The various theorems announced in (30), (31), and 32) are easily deducible from it.



II.

APPLICATION OF THE FOREGOING PRINCIPLES TO CERTAIN CASES OF ACTUAL PERTURBATION.

(39). Let us suppose two comparatively small bodies to be revolving in circular orbits situate in the same plane, round a large central body S, and let the mean motion of the interior revolving body be almost exactly double the mean motion of the exterior one. Let us assume also, for facility of explanation, that the exterior body maintains a fixed position at P, while the interior body performs an entire revolution around S. Join S P by a straight line, cutting the orbit of the interior body in A and D. Then D A P will represent the line of conjunction of the two bodies. Now in those cases of the solar system wherein the mean motion of one revolving body is almost exactly double the mean motion of the other, the effects produced by the mutual perturbation of the two bodies are sensible only near conjunction. Let us suppose that in one of such cases the disturbing influence of the exterior body first becomes sensible when the interior body has arrived at B, a position somewhat less advanced than the line of conjunction. It may be easily shewn that the disturb-



ing force of the exterior body will then be resolvable into two distinct forces, one of which acts in the direction of the radius vector, and the other in the direction of a tangent to the orbit. The former of these forces acts *outwards* throughout the whole of the arc B A C, and consequently tends to diminish the attractive force of the central body S. With respect to the force which acts in the direction of the tangent, or the *tangential force* as it may be called, it tends to accelerate the interior body while revolving from B to A, and to retard it by like degrees while revolving from A to C.

(40.) The disturbing influence exercised by the interior body upon the exterior one, is also sensible only near conjunction. In this case the force resolved in the direction of the radius vector of the disturbed body acts *inwards*, and consequently tends to increase the attractive force of the central body. The tangential force tends to retard the exterior body before conjunction and to accelerate it by like degrees after conjunction.

(41.) If we suppose the orbits to be slightly eccentric, as when the question refers to the mutual action of the celestial bodies, the character of the disturbing forces will be the same as above described. It is manifest that in such a case the disturbing body will occasion a continual change in the elements of the disturbed body during the time of its influence being sensible. Let us suppose, for example, that the orbit A C D B, of the interior body, is slightly eccentric. It is manifest that when the body is revolving from C to B through D, the elements of its orbit will not undergo any material alteration, because the action of the disturbing body over that portion of the orbit is so feeble that it may be neglected without occasioning any sensible error. As it revolves from B to C, however, the elements of the orbit will continually vary from the action of the disturbing force, and when it has arrived in the latter position, it will proceed to revolve in a new elliptic orbit, the difference between the elements of which and those of the orbit in which it was revolving previous to its arrival in B, will generally differ for each synodic revolution. It is manifest, from what has been already said in the foregoing pages, that the variations thus induced in the elements of the orbit of the disturbed body, will depend mainly on the position of the line of apsides of the orbit of each body with respect to the line of conjunction.

(42.) Let us now consider more particularly the perturbations produced in those cases of the planetary system wherein the mean motions of the two revolving bodies are actually characterised by the relation above mentioned. One very striking instance of such a relation occurs among the primary bodies of the system. The mean motion of Uranus is almost exactly double that of Neptune. A similar relation holds good between the first and second, and the second and third satellites of Jupiter; and also between the first and third, and the second and fourth satellites of Saturn (counting outwards from the planet). We shall first consider the mutual action of Uranus and Neptune.

(43.) According to the most recent researches of astronomers, the period of Uranus is 84.014 years, and that of Neptune, 164.6181 years. Hence when Uranus has completed two revolutions, Neptune will have completed one revolution, and will also have advanced in a second revolution over an arc corresponding to 3.4099 years. It is manifest, therefore, that if the two planets be supposed to have been originally in conjunction, Uranus after completing two revolutions, will require to advance a little *beyond* into conjunction with that planet. It may be easily shown that Uranus will require to advance through an arc equal to 15° 15′, before it again comes up with Neptune. The same is true with respect to the third conjunction of the planets, and thus it is evident that the line of conjunction on each of such occasions will advance through an arc equal to 15° 15′ in the direction of the motions of the two planets.

(44.) It has been remarked (41), that the alteration induced in the elements of either planet on the occasion of its conjunction with the disturbing planet, will depend upon the position of the line of apsides of the orbit of each planet with respect to the line of conjunction. Let us consider the effect which Neptune tends to produce on the eccentricity and the longitude of the perihelion of Uranus. If the line of apsides of the disturbed planet be oblique with respect to the line of conjunction, it is manifest that the variation of the eccentricity cannot effect a complete compensation, and therefore a change will ultimately be produced in the value of that element, which will be maintained until the planet again arrives within the sphere of the disturbing force on the occasion of approaching the next conjunction. It is easy to see, therefore, that in such a case the eccentricity of the orbit would undergo an alteration at each successive conjunction. Let us suppose, however, that the eccentricity constantly retains the same value (which is actually true in so far as regards that part of the eccentricity which is essentially co-existent with the disturbing force, and which is the only part we are here considering), and let us enquire into the circumstances under which this condition of permanence can be maintained. Now it appears from (24), (30), and (32), that if the line of apsides coincide with the line of conjunction, the eccentricity will not undergo any change from the action either of the radial or the tangential force. It is manifest, however, that this coincidence can only be maintained by a progressive motion of the line of apsides, equal to that of the line of conjunction. Now if the perihelion of the planet be turned towards the disturbing body, it follows, from (24), that the line of apsides will *regress*, and therefore, instead of constantly coinciding with the line of conjunction, it will immediately commence to deviate from that line by receding in the opposite direction. But if the aphelion of the planet be turned towards the disturbing body, the line of apsides will then *progress* (24), from the action of the disturbing forces; and, by duly adjusting the eccentricity, the progression may be made exactly equal to the progression of the line of conjunction. In this case, then, the variation of the eccentricity occasioned by the disturbing force will effect a complete compensation at each conjunction, leaving the eccentricity of the undisturbed portion of the orbit unaltered, while at the same time the line of apsides will advance regularly in the direction of the planet's motion with an angular velocity equal to that of the line of conjunction, or at the rate of 15° 15′ in each synodic revolution of the two planets.

(45.) The eccentricity above referred to, may be considered as an inequality of a perturbative character, inasmuch as the necessity for its existence depends wholly on the action of the disturbing force. It is right to bear in mind, however, that its maintenance is mainly due to the central force. The direct effect of the disturbing force is, merely to produce a slight variation of the eccentricity (which, however, effects a complete compensation at each conjunction), and to cause a constant progression of the line of apsides.

(46.) The more nearly the mean motions of the two planets are commensurable, the *less* will be the angle through which the line of apsides will have to advance during each synodic revolution of the two planets, and consequently the *greater* will be the eccentricity depending upon the disturbing force.

(47.) The progressive movement of the line of apsides is mainly due to the part of the disturbing force which acts in the direction of the radius vector. For, in the first place, the tangential force vanishes altogether at the point of conjunction, where the disturbing force is most intense; and, in the second place, its influence in altering the position of the apsides is very feeble near conjunction (where alone the disturbing force is sensible), since, by (31), its effect vanishes at the aphelion, which, in this case, always coincides with the point of conjunction. On the other hand, the radial force is not only most intense at conjunction, but is also most effective (24) at the same point *.

(48.) If we had considered the perturbation of Neptune by Uranus, it would readily appear, by (22), that it is the perihelion of the disturbed orbit which, in this case, would require to be turned towards the point of conjunction, in order that the line of apsides might progress; and it is manifest that, by a due adjustment of the eccentricity, the amount of advance during each synodic revolution might be rendered exactly equal to the advance of the line of conjunction. Thus the eccentricity would constantly retain the same value, while the line of apsides would regularly progress, coinciding always with the line of conjunction.

(49.) With a view to illustrate the foregoing remarks, we insert the principal terms of the mutual perturbations of both planets, exclusive of those arising from independent eccentricity, the influence of which we are not at present considering. In calculating these terms, a mean distance equal to 19.18239 has been assigned to Uranus. The mean distance of Neptune has been assumed equal to 30.0363, agreeably to the researches of Mr. Walker of Washington, U. S. The mass of each planet is supposed to be equal to $\frac{1}{20,000}$, the sun's mass being represented by unity. The angle ϕ denotes the excess of the mean longitude of Uranus above that of Neptune.

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* If we exclude from consideration the tangential force, as incapable of exercising any influence upon the *character* of the perturbative effect, it is easy to see that the disturbing force, by acting outwards in the direction of the radius vector, tends continually to twist round the line of apsides in the direction of the planet's motion. First, let the planet be advancing towards conjunction. In this case the tangential angle is obtuse, and the planet is naturally receding from the position in which the radius vector is perpendicular to the tangent; but the deflective influence of the central force is so powerful, that the tangential angle is actually contracting, and is again rapidly approaching to a right angle. Hence the disturbing force, by weakening the influence of the central body, diminishes the deflection of the orbit, and thereby retards the perpendicularity of the tangent with respect to the radius vector. It is manifest, therefore, that the planet will require to revolve through an angle of somewhat more than 180° from the perihelion before it arrives at the aphelion, or, in other words, the line of apsides will have progressed. With respect to the influence of the disturbing force after the passage of the aphelion, it is to be remarked, that if the central force were diminished *in due proportion* throughout the whole semi-ellipse, extending from the aphelion to the perihelion, the position of the line of apsides would not undergo any change. It is evident, therefore, that the *character* of the effect produced after the passage of the aphelion (whether progression or regression) will be the same as if the central force was *increased* a little before the arrival of the planet in the perihelion. Now when the planet is approaching the perihelion, the tangential angle is rapidly opening out, from the natural tendency of the body to persevere in the same direction; and the efficacy of the central force consists in opposing its enlargement, and thereby retarding the arrival of the planet at the apse. It is evident, therefore, that the effect of an increase of the central force will be to retard, in a still greater degree, the arrival of the planet in such a position. It appears, then, that the action of Neptune upon Uranus causes the line of apsides to progress both before and after conjunction. It may be shewn, in a similar way, that the action of Uranus upon Neptune will

PERTURBATION OF TRUE LONGITUDE.

Neptune disturbing Uranus.

$$\begin{aligned}\delta v_1 = &- 33''.537 \sin \phi \\ &- 815.596 \sin 2\phi \\ &+ 13.943 \sin 3\phi \\ &+ 3.146 \sin 4\phi \\ &+ 1.055 \sin 5\phi \\ &+ 0.423 \sin 6\phi\end{aligned}$$

Uranus disturbing Neptune.

$$\begin{aligned}\delta v_2 = &+ 252''.836 \sin \phi \\ &- 10.648 \sin 2\phi \\ &- 2.094 \sin 3\phi \\ &- 0.665 \sin 4\phi \\ &- 0.261 \sin 5\phi \\ &- 0.115 \sin 6\phi\end{aligned}$$

PERTURBATION OF RADIUS VECTOR.

Neptune disturbing Uranus.

$$\begin{aligned}\delta r_1 = &+ 0.00085 \cos \phi \\ &+ 0.03738 \cos 2\phi \\ &- 0.00083 \cos 3\phi \\ &- 0.00021 \cos 4\phi \\ &- 0.00008 \cos 5\phi \\ &- 0.00003 \cos 6\phi\end{aligned}$$

Uranus disturbing Neptune.

$$\begin{aligned}\delta r_2 = &- 0.01635 \cos \phi \\ &+ 0.00119 \cos 2\phi \\ &+ 0.00028 \cos 3\phi \\ &+ 0.00010 \cos 4\phi \\ &+ 0.00004 \cos 5\phi \\ &+ 0.00002 \cos 6\phi\end{aligned}$$

(50.) The enormous magnitude of one of the terms in each of these four columns relatively to the others cannot fail to strike the reader. It is to be remarked also, that while in the case of Neptune disturbing Uranus, the preponderating term is the *second* in the column, both when the perturbation of longitude and that of the radius vector are considered, on the other hand, in the case of Uranus disturbing Neptune, it is the *first* term in each column which is the preponderating one. Finally, the *sign* of the proponderating term in the column representing the action of Neptune upon the longitude of Uranus is *negative*, while that of the corresponding term in the column which represents the action of Uranus upon the longitude of Neptune is *positive;* and the contrary holds good when the question refers to the perturbation of the radius vector. All these points may be easily explained by reference to the principles which govern the mutual action of the two planets.

(51.) First let us consider the predominant term in the perturbation of the longitude of Uranus by the action of Neptune. It has been stated that ϕ denotes the excess of the mean longitude of Uranus over the mean longitude of Neptune. Hence, if during a synodic revolution we reckon the longitudes from the point of conjunction we have $\phi = n_1 t - n_2 t$, and $2\phi = (2n_1 - 2n_2)\,t = \Big(n_1 - (2n_2 - n_1)\Big)\,t$, n_1, n_2 denoting the mean annual motions of Uranus and Neptune. Now $n_2 = 7872''.77$, and consequently $2n_2 = 15745''.54$. Again, $n_1 = 15425''.64$. Hence $2n_2 - n_1 = 319''.90$, which is a very small quantity relatively to n_1 or n_2, and therefore $2\phi = \Big(n_1 - (2n_2 - n_1)\Big)\,t = n_1 t$ very nearly. We have, therefore, $815''.596 \sin 2\phi = 815.596 \sin n_1 t$ *plus* a small quantity of variable value. Now $815''.596 \sin n_1 t$ represents the principal term of the equation of the centre in an ellipse whose eccentricity is equal to 0.00197, and, since the sign of the term is negative, it necessarily follows that the aphelion is turned towards the point of conjunction.

(52.) It appears, then, that if we refer the motion of the planet to the line of conjunction, the predominant term of the perturbation in longitude becomes confounded with the principal inequality of an ellipse whose aphelion coincides with the point of conjunction. The same conclusion is obviously deducible from an examination of the corresponding term in the perturbation of the radius vector.

(53.) Let us now consider the predominant term in the column representing the action of Uranus upon the longitude of Neptune. In this case $\varphi = n_1t - n_2t = (n_1 - n_2)\ \mathrm{t} = \Big(n_2 - (2\ n_2 - n_1) \Big) = n_2t$ nearly, and consequently $252''.836 \sin \varphi = 252''.836 \sin n_2t$ *plus* a small quantity of variable value. Now $252''.836 \sin n_2t$ represents the principal term of the equation of the centre in an ellipse whose eccentricity is equal to 0.0006. Hence arises an elliptic inequality analogous to that produced by the action of Neptune upon Uranus, the only difference being, that in this case it is the perihelion which is turned to the point of conjunction, a circumstance implied by the *positive* sign of the inequality. It is manifest that the corresponding term in the perturbation of the radius vector is consistent with this conclusion.

(54.) It appears, then, that the predominant term in the perturbation of the longitude of each planet represents the equation of the centre in an ellipse, in the focus of which the sun is placed. It is manifest, therefore, that the inequality is maintained during each synodic revolution by the action of the central force, being perturbative only in so far as the line of apsides is continually twisted round in the direction of the planet's motion. The remaining terms of perturbation in each case may be considered as representing the effects more directly due to the disturbing force.

(55.) The mean motions of the two planets being very nearly commensurable, it is manifest that a slight change effected in the value of either, would exercise a very considerable influence on the displacement of the line of conjunction, and consequently would affect, in an equal degree, the progression of the line of apsides. Now the eccentricity thus depending on perturbation must always be adjusted to the motion of the apsides, increasing as that diminishes, and *vice versâ*. When the motion of the apsides is very slow, a slight diminution of its value occasions an enormous increase of the eccentricity. Now, in the present case, the more nearly the mean motion of the interior planet approaches to twice the mean motion of the exterior one, the slower will be the motion of the points of conjunction, and, consequently, so will be that of the line of apsides. Hence it is manifest, that the eccentricities of the two planets will increase indefinitely as their mean motions satisfy with greater accuracy the relation just mentioned. This relation corresponds to a mean distance of Neptune equal to 30.4507, the radius of the terrestrial orbit being assumed equal to unity.

(56.) When the mutual action of the planets is viewed through the medium of analysis, this principle exhibits itself in the form of the coefficient of the predominant term, which has for a divisor the square of change in the mean motion of either planet will occasion an enormous alteration in the value of the coefficient, which is divided by the square of the same quantity.

(57.) The remaining terms of the perturbation vary only in a degree commensurate with the change which may be effected in the mean motion of either planet. This circumstance arises from their being mainly dependent on the direct action of the disturbing force, the intensity of which cannot be expected to undergo a considerable change in consequence of a slight alteration in the relative values of the mean distances of the two bodies.

(58.) We have hitherto supposed that neither the orbit of Uranus nor that of Neptune possesses any eccentricity except what arises from their mutual action. In reality, however, both orbits are slightly eccentric, independently of the effects of perturbation. In consequence of this circumstance, the mutual distance of the two planets will vary at each successive conjunction; whence it is manifest that the intensity of their disturbing forces will undergo a corresponding variation. Now, in consequence of the near commensurability of the mean motions of the two planets, the line of conjunction shifts with extreme slowness, its displacement during a synodic revolution amounting only to 15° 15′. The duration of a synodic revolution is 171.6 years; whence it follows that the line of conjunction will not accomplish a complete revolution before the lapse of 4051 years. During this period the disturbing forces of the two planets will be constantly varying in intensity, returning only at its close to their original values.

(59). It is easy to see that this variation of the intensity of the disturbing forces of the two planets will occasion corresponding variations in the elements of both orbits, requiring an equal lapse of time to effect a complete compensation. Hence the mean distance, eccentricity, and longitude of the perihelion of either planet, will be subject to an excessively slow variation, which in each case will pass through the cycle of its values in a period of 4051 years. The variation of the mean distance will produce a corresponding variation in the mean motion of each planet, and hence will originate an inequality in the mean longitude analogous to the long inequality of Jupiter and Saturn, and several others to which we have had occasion to allude in the course of this work.

(60.) The circumstances which determine the long inequality of Uranus and Neptune are less favourable to its magnitude than those which determine the analogous inequality in the longitudes of Jupiter and Saturn, inasmuch as the masses, eccentricities, and inclinations of the disturbing planets are less in the former case than in the latter. In one respect, however, the magnitude of the inequality is liable to be much greater in the case of Uranus and Neptune than in that of Jupiter and Saturn, or any other two planets yet discovered, whose mean motions are nearly commensurable. In the case of Jupiter and Saturn, every three conjunctions take place in different parts of the orbit, and it is merely the minute quantity which remains outstanding after every such triple conjunction, that is allowed to accumulate upon the longitude. With respect to the long inequality of the Earth and Venus, the accession to the mean longitude is only what remains uncompensated after every fifth conjunction of the two planets. On the other hand, in the case of the long inequality of Uranus and Neptune, every two successive conjunctions occur in the same part of the orbit, the interval included between them being merely the small displacement arising from the absence of perfect commensurability in the mean motions of the two planets. In consequence of this circumstance, it happens that the whole effect produced by the disturbing planet at each successive conjunction is accumulated upon the mean longitude.

(61.) A similar conclusion is suggested by the analytical view of the subject. In the case of Jupiter and Saturn, the inequality is mainly represented by a class of terms, which are only of the *third* order of magnitude with respect to the eccentricities and inclinations of the two planets. The long inequality of the Earth and Venus depends upon a series of terms, the most considerable of which are only of the *fifth* order of magnitude with respect to the eccentricities and the inclinations. On the other hand, the long inequality of Uranus and Neptune is mainly contained among a class of terms which are as high as the *first* order of magnitude relatively to the same elements.

(62.) The more nearly the mean motion of Uranus approaches to double the mean motion of Neptune, the more slowly will the line of conjunction of the two planets advance, and consequently the longer will the inequality in the mean longitude continue to vary in the same direction. Hence it is manifest, that the maximum value of the inequality will increase as the mean motions of the two planets are more nearly commensurable.

(63.) It appears from the foregoing consideration, that the more perfect commensurability of the mean motions of the two planets tends to promote the ultimate magnitude of the long inequality, by prolonging the time during which it continues to accumulate upon the mean longitude. We have seen that the elliptic inequality depending upon perturbation increases also with the more perfect commensurability of the mean motions of the two planets, in consequence of the slower motion of the line of conjunction hence resulting, which creates the necessity of a greater amount of eccentricity, so as to oppose an adequate resistance to the disturbing force, in its tendency to twist round the line of apsides which must always advance at the same rate as the line of conjunction. In the former case the inequality results from the direct action of the disturbing planet at each successive conjunction, and depends, for its ultimate magnitude, on the length of time during which the effects thus produced are allowed to accumulate upon the mean longitude. In the latter case the inequality arises from the powerful agency of the central force, and is developed in a single synodic revolution.

(64.) It has been stated that the system of Jupiter's satellites presents two instances in which the mean motion of one of the disturbing bodies is almost exactly double the mean motion of the other. In effect, the first satellite performs a sidereal revolution in $1^d\ 18^h\ 27^m\ 34^s$, and the second satellite in $3^d\ 13^h\ 13^m\ 42^s$. Hence two revolutions of the first satellite will be completed in $3^d\ 12^h\ 56^m\ 8^s$, an interval of time which falls short of one period of the second satellite by only $17^m\ 34^s$. In consequence of this circumstance, the line of conjunction of the two satellites shifts with extreme slowness, *regressing* through an arc of little more than 2°, at the close of each synodic revolution. Hence arises in the motion of each satellite a large elliptic inequality of a perturbative character, resembling that produced by the mutual action of Uranus and Neptune, with this interesting distinction—that as the line of conjunction now *regresses*, it is the *lower* apse of the *interior* body and the *upper* apse of the *exterior* one which that the line of apsides of the orbit of either body may always coincide with the line of conjunction.

(65.) The third satellite of Jupiter accomplishes a sidereal revolution in $7^d\ 3^h\ 42^m\ 32^s$. Now two periods of the second satellite are equal to $7^d\ 2^h\ 27^m\ 24^s$, which differs from one period of the third by only $1^h\ 15^m\ 8^s$. This case, then, is clearly analogous to that of the first and second satellites. In fact it is easy to infer, from the remarkable relation between the mean motions of the three interior satellites mentioned at page 92, that the mean motion of the second satellite exceeds twice the mean motion of the third by a quantity which is exactly equal to the excess of the mean motion of the first satellite over twice the mean motion of the second. The line of conjunction of the second and third satellite will therefore regress at the same rate as the line of conjunction of the first and second, and hence will arise a large inequality in the motion of each satellite, resembling the one mentioned in the foregoing article. The motion of the second satellite is thus affected by two elliptic inequalities of a perturbative character, depending upon the combined action of the first and third satellites, and in consequence of the remarkable relation which subsists between the mean longitudes of the three interior satellites, the two inequalities are thoroughly confounded together, so as to assume the complexion of only one great inequality (see p. 89).

(66.) Astronomers have been unable to discover the slightest trace of independent eccentricity in the orbit of the first satellite of Jupiter. With respect to the orbits of the second and third satellites, the independent eccentricity is in either case exceedingly small. In consequence of this circumstance, no sensible evidence has been derived from observation, of the existence of a long inequality in the mean longitude of any of the satellites, depending on the near commensurability of their mean motions.

(67.) Some of our readers may perhaps find it difficult to reconcile the foregoing remark with the fact of Bradley's discovery of a great inequality in the three interior satellites, the period of which he found to extend to 437 days, which vastly exceeds the duration of a synodic revolution of either of the satellites. It is to be borne in mind, however, that the existence of this inequality was indicated solely by observations of eclipses of the satellites. Now, in the case of an elliptic inequality of a perturbative character, depending on the mutual action of any two of the satellites, it will manifestly pass through the cycle of its values when the two satellites return to the same position with respect to the line of conjunction. If, however, the inequality be considered solely with reference to its influence upon the times of the eclipses of the satellites, it will not in either case effect a compensation during the period comprised between two successive oppositions of the satellite with respect to Jupiter; for while the line of conjunction of the satellites has regressed, in virtue of the relation between their mean motions, the planet whose position, relatively to the sun, determines the time of the eclipse, has revolved in the opposite direction, and it is manifest that a complete restoration of the inequality cannot be established until the satellites have returned to the same position with respect to the line of conjunction, and the axis of Jupiter's shadow. Hence the long inequality discovered by Bradley is rather apparent than real, being merely the consequence of adopting a restrictive view of the mode in which the elliptical perturbation affects the of the solutions of the inverse problem of perturbation due to these distinguished geometers, or detract from the merit of their researches in so far as the main object of them was concerned; namely, the ascertainment of the position of the disturbing body with a view to its physical discovery.

(72.) No difficulty can be experienced in arriving at the conclusion, that elements widely different from the true values might serve to indicate the position of the disturbing body with sufficient accuracy, provided the following two facts be borne in mind:—first, that the action of Neptune upon Uranus is sensible only near conjunction; secondly, that during the interval embracing the observations of Uranus which formed the groundwork of the investigations of both Adams and Le Verrier, there happened only one conjunction of the two planets. Thus the disturbing influence which Neptune exercises upon Uranus is sensible only for about twenty years before, and about an equal interval after, conjunction. Again, the last conjunction happened in the year 1822, and, as the period of a synodic revolution of the two planets is 171.6 years, it follows that the previous (mean) conjunction happened in the year 1650. Now the earliest observation of Uranus is one by Flamsteed in the year 1690, at which epoch the action of Neptune was, therefore, quite insensible.

(73.) It is manifest from the foregoing considerations, that the question relative to the discovery of the disturbing planet was resolvable by means of any elements which might be capable of representing the intensity and direction of the disturbing force on the occasion of the last conjunction in 1822. Now, when it is borne in mind that the mean distance, the eccentricity, the longitude of the perihelion, and the mass of the disturbing body may be varied at pleasure, it is not difficult to see that this object may be effected by means of a variety of sets of elements all very different from the real elements of the planet. Thus if the mean distance be assumed too great, the error arising in consequence may be obviated by increasing the eccentricity in a corresponding degree, and placing the perihelion so as to coincide nearly with the point of conjunction. Moreover, if it should happen that the *intensity* of the disturbing force is represented with a less degree of accuracy than its *direction* by such an adjustment of the elements of the orbits, this defect might be remedied by assigning a suitable value to the mass of the disturbing body. It is by such an adjustment of the elements of the disturbing planet, that Le Verrier and Adams succeeded in indicating its actual position with such remarkable precision, as may be easily seen by comparing their elements with those subsequently deduced from actual observation. It is not difficult to conceive that if a mean distance less than that of the true value had been assumed, the direction of the disturbing force might have been represented by increasing the eccentricity and turning the aphelion to the point of conjunction.

(74.) If the observations of Uranus, upon which the researches of Le Verrier and Adams were based, had embraced more than one conjunction of that planet with Neptune, the elements of the hypothetical planet would manifestly have been confined within narrower limits. It is probable that the difficulty which both of these geometers experienced in accounting for Flamsteed's observation of 1690, arose from the circumstance of the planets of their respective theories being capable of occasioning considerable disturbance in the motion of Uranus at an epoch when the action of Neptune was totally insensible. This view of the astronomers, by applying to the elliptic motion of Uranus the perturbations produced by Neptune *as represented by the formulæ of analysis*, have succeeded in satisfying the observation of 1690 with almost perfect accuracy, the outstanding error being less than 1″. The question appears to admit of a definitive solution by adopting the following mode of procedure:—Since the action of Neptune upon Uranus continued insensible from 1670 to 1800, it necessarily follows that the motion of Uranus, after subducting from it the effects produced by the disturbing action of the other planets, was purely elliptic during the whole of the interval of time included between these two epochs. Hence it is obvious, that if the elements of Uranus be deduced from a sufficient number of observations made within the included interval, the motion of the planet, when calculated from such elements, ought to satisfy the totality of the observations, extending from 1690, the year of Flamsteed's earliest observation, down to 1800, or even a few years later.

(75.) The elements of Neptune being considerably different from those of the hypothetical planets of Le Verrier and Adams, and its mean motion being nearly commensurable with the mean motion of Uranus, the theory of its action upon the latter planet presents a wide discordance, when compared with the theory of either of the geometers just mentioned. It is to be borne in mind, however, that this circumstance is immaterial, when the question relates merely to the perturbations produced in the motion of Uranus, on the occasion of one conjunction with Neptune. Prof. Peirce, however, took a different view of the subject. He contended, on the ground of the discordance above referred to, that Neptune was not the planet designated by geometry, and that, in fact, its discovery must be regarded as a happy accident. "The solutions of Adams and Le Verrier," says he, "are perfectly correct for the assumption to which they are limited, and must be classed with the boldest and most brilliant attempts at analytical investigation, richly entitling their authors to all the *éclat* which has been lavished upon them on account of the singular success with which they are thought to have been crowned. But their investigations are nevertheless wholly inapplicable to the theory of the mutual perturbations of Uranus and Neptune. The successive periods of conjunction and opposition, occurring at intervals of eighty-four years, that is, in about the time of a revolution of Uranus, this planet is always at the same part of its orbit when it is most affected by the action of Neptune. The action of Neptune consequently assumes a fixed, permanent undisturbed character, so that it can hardly be recognised as perturbation by the practical observer. It is far otherwise with the ordinary class of perturbations, where the place of greatest disturbance varies from point to point of the orbit: thus the place of greatest disturbance, in the case of the theoretical planet, would not have remained stationary, but have varied 80° upon the orbit of Uranus at each successive conjunction and opposition; so that the disturbance could not in this case be disguised to any great extent under the fixed laws of ordinary elliptic motion. In the case of Neptune, its action on Uranus is to be detected in the comparatively small differences between its character and that of an elliptic motion, and the difference between the influence at opposition and that at conjunction."*

(76.) The assertion of Prof. Peirce—that the investigations of Adams and Le Verrier are inapplicable to the theory of the mutual action of Uranus

Uranus and Neptune, the disturbing force of the latter planet was represented very nearly, both in intensity and direction, by the disturbing force of either of the hypothetical planets of Adams and Le Verrier. Hence, as like causes produce like effects, we are warranted in concluding that the irregularities which either of the hypothetical planets would have been capable of producing on that occasion are exactly commensurate in magnitude with those actually produced by the planet Neptune, and therefore afford an equal hold to the geometer for investigating the position of the disturbing body. In fact, it will appear obvious on the slightest consideration of the subject, that the mere circumstance of the near commensurability of the mean motions of Uranus and Neptune cannot exercise any influence upon the magnitude of the perturbations produced at one conjunction of the two planets. The ellipticity which accompanies such a relation of the mean motions, has its magnitude adjusted so as to form an opposing obstacle of adequate inertia, if we may use the expression, to the disturbing force, by preventing the line of apsides from revolving at a more rapid rate than that at which the line of (mean) conjunction revolves, but, in so far as its own existence is concerned, it is maintained solely by the action of the central force.

(79.) Prof. Peirce has exhibited a comparison between the numerical values of the perturbations of Uranus, as computed in the one case by himself from an analytical investigation of the action of Neptune, and in the other case by Mr. Adams, from a similar investigation of the action of his second hypothetical planet. The enormous discordance between the results derived from these two distinct sources, appears to Prof. Peirce to constitute a sufficient refutation of what he considers the fallacious notion "that the less distance of Neptune than the planet of geometry is compensated by its smaller mass, so that its action upon Uranus is the same with that which was predicted."* He remarks that the difference of the perturbations produced by the two planets is just balanced by the difference due to the corrections of the elements of Uranus, so that the corresponding effects upon the longitude of that planet are equal in both theories.

(80.) We may reply, with reference to this mode of viewing the subject, that the formulæ of analysis do not furnish a direct criterion of the disturbing action of a planet during one synodic revolution. It must be borne in mind, that all those terms which are not absolutely elliptical (or, in other words, which do not *rigorously* satisfy the differential equation of the second order relative to elliptic motion) are contained in these formulæ, even although they may be to a great extent due to the central force. Such is the case with respect to the great elliptic inequality in the perturbations of Uranus and Neptune depending on the near commensurability of their mean motions. Again, there are terms representing inequalities of long duration which hardly undergo any sensible deviation from ellipticity during one synodic revolution. In effect, the analytical theory of the action of a planet supplies a fund of terms adequate to meet the requirements of the ever-shifting position of the line of conjunction with respect to the orbits of the disturbing and disturbed planets, throughout indefinite ages, both past and future. Now the action of the planet during a short interval of time is embedded in these terms, so that it is impossible to estimate its magnitude by the numerical values of the terms corresponding to the same interval.

effected by eliminating from the numerical results all the portion which sensibly coincides with elliptic motion during the period of time under consideration. If both planets revolved in circular orbits, the amount of perturbation thus extinguishable from the analytical formulæ would be the same in each synodic revolution; but when the orbits possess any independent eccentricity, it will generally be different. It will at once appear, from a bare inspection of the numerical results given by Mr. Adams at the close of his memoir*, that by far the greater portion of the perturbations of a planet as represented by the formulæ of analysis, coincides sensibly with elliptic motion during a synodic revolution of the two planets. Now, since the action of Neptune upon Uranus was insensible from 1670 to 1800, the planet must have revolved during the whole of the included interval of time in an elliptic orbit, the elements of which resulted from the action of the disturbing body on the occasion of the previous conjunction in 1650. Hence it follows, that the numerical values of the perturbations for the same period of time, as derived from the analytical formulæ, admit of being represented by corrections to the elliptic elements of the planet's motion; and it is manifest that their subsequent deviations from the results of such corrections will indicate the action of the disturbing body on the occasion of the conjunction of the two planets in 1822. If the perturbations produced by the hypothetical planets of Le Verrier and Adams were treated in the same way, the direct action of the disturbing body would similarly emerge in each case from the ellipticity in which it is embedded, and it is manifest that the results thus obtained would coincide, or very nearly so, with those relative to the direct action of Neptune.

(81.) It has been stated, in the account of the discovery of the planet Neptune (Chap. XII.), that Le Verrier, besides determining the precise position of the disturbing body, assigned at the same time the limits of longitude within which it was probably confined, and also the limits of the elements of its orbit. In this part of his investigation he attributed a large probable error to the observations, distinguishing them according to the degree of confidence to which he considered them to be entitled on the score of accuracy. The error of Flamsteed's observation of 1690, was estimated by him to be 25″. The subsequent observations of the planet down to the epoch of its discovery in 1781, were supposed to be erroneous, in some cases to 15″; in others to only 10″. The modern observations of the planet, extending from 1781 to 1845, were all estimated to have a probable error of 5″. It must be acknowledged that the conclusions at which he arrived, relative to the limits of the elements of the disturbing body, have not been subsequently borne out by the results deduced from actual observations of the planet Neptune. Thus he found that the mean distance of the disturbing body could not be greater than 37.90 nor less than 35.04. Now we have seen that the most probable value of the mean distance of the planet Neptune is 30.0363. This large discordance between theory and observation cannot be removed by attributing to the observations of Uranus a larger probable error than that which Le Verrier assigned to them; since it appears that the action of Neptune accounts for the irregularities of that planet to a degree of accuracy which obviates the necessity of supposing the observations to be erroneous, even to so great an extent as Le Verrier estimated them to be. The following table, constructed by Prof. Peirce, exhibits an interesting view of the residual errors in the longitude of Uranus, corresponding to four distinct theories of the planet. The numbers in the second column are the results of Le Verrier's attempt to account for the irregularities of the planet without supposing it to be influenced by any foreign cause of disturbance. The third and fourth columns contain the errors of the theories of Le Verrier and Adams, founded on the assumption of the existence of a disturbing planet. The fifth column exhibits the residual errors of the planet after taking into account the action of Neptune as calculated by Prof. Peirce, from the elements of Walker. The mass of Neptune employed in these calculations was deduced by Prof. Peirce from Bond's observations of the satellite discovered by Lassell. The results due to Le Verrier inserted in the second and third columns, represent the excess of theory over observation. The same is true with respect to the numbers in the fourth column containing the results of Adams's theory; but, in consequence of this circumstance, their signs are contrary to those of the same numbers in Peirce's table. Prof. Peirce does not state whether the numbers in the fifth column represent the excesses of theory above observation, or those of observation above theory; but this point is immaterial in so far as our present purpose is concerned.

RESIDUAL DIFFERENCES BETWEEN THE THEORETICAL AND OBSERVED LONGITUDES OF URANUS.

Date.	Without any external planet.	By Le Verrier's theory of a disturbing planet mass = $\frac{1}{9322}$.	By Adams's theory (Hyp. II.) mass of disturbing planet = $\frac{1}{6666}$.	By Peirce's theory of the planet Neptune mass = $\frac{1}{19840}$.
1845	+ 6″.5	− 0″.3		− 0″.9
1840	+ 0.7	+ 2.2	− 1.3	− 1.1
1835	− 4.5	− 0.8	+ 1.2	+ 2.0
1829	− 7.8	− 2.2	− 2.0	+ 0.8
1824	− 7.6	− 5.4	− 1.7	− 2.0
1819	+ 3.8	+ 0.4	+ 2.2	+ 1.0
1813	+ 4.5	− 0.9	+ 1.0	− 0.3
1808	+ 3.8	+ 0.8	0.0	− 0.4
1803	− 3.4	+ 0.8	− 1.6	+ 0.8
1797	− 6.7	− 1.0	+ 0.5	+ 0.3
1792	− 7.8	+ 0.3	+ 1.1	+ 0.3
1787	+ 2.0	− 1.2	+ 0.2	− 0.5
1782	+ 20.5	+ 2.3	0.0	− 3.0
1769	+ 123.3	+ 3.7	− 1.8	− 6.0
1756	+ 230.9	− 4.0	+ 4.0	+ 4.0
1715	+ 279.6	+ 5.5	+ 6.6	+ 8.7
1690	+ 289.0	− 19.9	− 50.0	+ 0.8

(82.) Le Verrier has attempted to defend the conclusions at which he arrived relative to the limits of the mean distance of the hypothetical planet upon the ground that he assigned too small a probable error to the observations. "When the calculation of the limits of the mean distance," says he, "is resumed with other probable errors of observation than 5″, it will readily be seen that the interval included between these limits by no means varies proportionally to the uncertainty of the data. It varies much more rapidly. Thus while for a probable error of 5″ in the data, we find 2.86 for the interval between the limits of the mean distance, this interval so diminishes with the probable error, that when the latter is reduced to half this quantity we no longer find any value of the mean distance which can satisfy the question. And on the contrary, when the probable error of the modern observations is extended beyond 5″, the inferior and superior limits of the mean distance will be found to vary with rapidity, and to leave the greatest latitude in the choice of that auxiliary."* These remarks are not borne out by the results contained in the foregoing table. The action of the planet Neptune accounts for the irregularities of Uranus, without even supposing so large an error as 5″ in the modern observations, and yet its mean distance is 30.0363, which is far below Le Verrier's inferior limit of the mean distance. The modern observations of Uranus are satisfied to within 3″ by the action of Neptune, whereas Le Verrier found that if the probable error of these observations was reduced to 2″.5, there is no mean distance of the hypothetical planet which could satisfy the observations. The trifling difference between 3″ and 2″.5 can hardly serve to account for the discrepancy which here presents itself between the results of Le Verrier's theoretical researches, and those relative to the perturbations actually produced by Neptune.

It is manifest, without pursuing the subject any further, that the limits assigned by Le Verrier to the mean distance of the hypothetical planet, are at direct variance with the mean distance of Neptune, as deduced from actual observation. Nor is it a matter of any importance, whether the elements employed in calculating the action of Neptune do or do not represent the true orbit of that planet. It is sufficient that an orbit can be assigned, the mean distance of which lies far below the inferior limit assigned by Le Verrier, in which if a planet of a given mass be supposed to revolve, its action will be capable of completely accounting for the irregularities of Uranus.

(83.) The question then arises, where are we to look for the origin of this discordance between theory and observation? In order to arrive at some conclusion upon this point, it is necessary to direct attention to the method of investigation by which Le Verrier deduced his final results respecting the hypothetical planet. The groundwork of these results consisted of thirty-three equations of condition. Le Verrier concluded from his previous researches that $\frac{a}{a'}$, the ratio of the mean distance of Uranus to that of the hypothetical planet, must be very nearly equal to .51, and that ε, its mean longitude at the beginning of the year 1800, must be contained somewhere between 234° and 270°. He, therefore, in his final investigation assumed $\frac{a}{a'} = 51+02\gamma$, and $\varepsilon = 252° + 18°\zeta$, γ being a quantity which he imagined would not differ much from unity, while ζ was supposed by him to lie somewhere between $+1$ and -1. Besides these quantities each of the equations contained other seven unknown quantities, namely, the corrections of the four elements of Uranus, and the eccentricity, longitude of perihelion, and mass of the disturbing planet. The corrections γ and ζ enter into the equations in a very complicated manner; but if we suppose these quantities to be known, the equations assume a linear form, and the values of the remaining quantities may be obtained without any serious difficulty. Le Verrier proposed to give the equations a linear form by employing particular values of γ and ς, assuming such values as he expected would not be very distant from the real values. For this purpose he selected six particular sets of values, namely, $(\gamma = -1, \varsigma = 0)$, $(\gamma = 0, \varsigma = -1)$, $(\gamma = 0, \varsigma = 0)$, $(\gamma = 0, \varsigma = 1)$, $(\gamma = 1, \varsigma = -1)$, $(\gamma = 1, \varsigma = 0)$; and, substituting these successively in the thirty-three equations of condition, he obtained six distinct groups of equations involving the remaining seven unknown quantities. These equations being all linear, he at once derived from them the particular values of six of the unknown quantities, and finally, by substitution, he obtained six sets of equations corresponding to the particular values of γ and ς, which involved only one unknown quantity, namely, the mass of the disturbing planet. These equations would be the particular forms assumed by the original equations of condition, if the six unknown quantities referred to had been in the first instance eliminated from them, and then each particular set of values had been substituted for γ and ς. Le Verrier proposed, therefore, to construct the general equations involving γ, ς, and m' by means of the six sets of particular equations involving m' alone. He assumed, moreover, that the general form of the equation might be represented by an algebraic function of the second degree with respect to γ and ς. From these equations he deduced the most accurate values of γ, ς, and m'. In this manner he found $\gamma = 1.029$, $\varsigma = -0.65$, and $m' = 1.072$, the mass of the sun being supposed equal to 10,000. The limiting values of these quantities were determined by a discussion of the equations, founded on the supposition of the various observations being erroneous to the extent already mentioned. The most accurate value of γ assigned 36.1539 as the mean distance of the hypothetical planet, and the discussion of limits gave 37.90 and 35.04 for the extreme values of that element.

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* Comptes Rendus, tome xxvii., p. 330.

(84.) Prof. Peirce has suggested, that the circumstance of the mean distance of Neptune not being comprehended within the limits assigned by Le Verrier to the mean distance of the hypothetical planet, may have arisen from an oversight on the part of the latter, in not having taken into account the peculiar character of the perturbations which would be produced if there existed a relation of perfect commensurability between the mean motions of the disturbing and disturbed planets. "An important change, indeed, in the character of the perturbations," says he, "takes place near the distance 35.3; so that the continuous law by which such inferences are justified is abruptly broken at this point, and it was hence an oversight in M. Le Verrier to extend his inner limit to the distance 35. A planet at the distance 35.3 would revolve about the sun in 210 years, which is exactly two and a half times the period of the revolution of Uranus. Now, if the times of revolution of two planets were exactly as 2 to 5, the effects of their mutual influence would be peculiar and complicated, and even a near approach to this ratio gives rise to those remarkable irregularities of motion which are exhibited in Jupiter and Saturn, and which greatly perplexed geometers until they were traced to their origin by Laplace. This distance of 35.3, then, is a complete barrier to any logical deduction, and the investigations with regard to the outer space cannot be extended to the interior."*

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* Proc. Amer. Acad., vol. i., p. 66.

(85.) Plausible as these remarks may at first sight appear to be, we venture to assert, with all due respect to Prof. Peirce, that they will be found, upon an attentive examination of the subject, to have no essential connection with the point at issue. It must be borne in mind that the question under consideration, as it suggested itself to Le Verrier, was this—within what limits may the mean distance of the hypothetical planet vary, so that a determinate number of observations of Uranus included between the years 1690 and 1845 may be satisfied by the disturbing action of the planet within certain assignable limits of error? Now, the consequences resulting from a near approach to commensurability in the mean motions of the two planets can only be developed in the course of many synodic revolutions. As to an exact commensurability of the mean motions, it will be found, on a very slight consideration of the subject, to be incompatible with the mutual action of the two planets; but at all events, it is manifest that such a condition cannot exercise any peculiar influence on the perturbations of either planet during a short interval of time. Hence it follows, that whether the mean motions of the disturbing and disturbed planets be to each other exactly as 2 to 5, or whether they be very nearly in this ratio, the effects produced by their mutual action must be sensibly the same during the limited period of time over which the observations extend. We may remark further, that innumerable instances of commensurability may be suggested between the limits assigned by Le Verrier, besides the relation alluded to in the foregoing passage by Prof. Peirce, and that the inequality arising from a near approach to such a relation in any case, may be rendered theoretically as great as we please by bringing the mean motions sufficiently near to the condition of perfect commensurability. It is manifest, however, that the contingent occurrence of any such relation between the mean motions of the two planets, is entirely foreign to the question under examination, which merely relates to the direct action of Neptune during the interval comprised between they ears 1690 and 1845, a period of time embracing less than one synodic revolution of the two planets.

(86.) But it may be asserted, in support of the argument of Prof. Peirce, that, although the influence of an exact commensurability of the mean motions of the two planets, or of a near approach to that relation, is unimportant, in so far as the mutual action of the two bodies during a short interval of time is concerned, still Le Verrier, by deducing the limits of the mean distance of the hypothetical planet from analytical formulæ involving the mean motions of the two planets as arbitrary constants, without taking into consideration the discontinuity occasioned by the passage of any of the terms of the formulæ through infinity in consequence of a relation of commensurability between the mean motions, committed an error of reasoning, which could not fail to vitiate his results. To this it may be replied, that the formulæ which Le Verrier used as the groundwork of his researches do not contain the terms involving the relation between the mean motions alluded to by Prof. Peirce. But, in point of fact, the method employed by Le Verrier in his final researches on the hypothetical planet is totally independent of any relation of commensurability whatever. The action of the disturbing body is computed for a certain number of values of the mean distance, and a general equation * of the second degree, involving the true correction of that element, is then constructed from

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* In this and in every subsequent instance in which the word *equation* is used, the

the particular results by interpolation. It is manifest that, by such a method, he gets rid of the embarrassment which might be occasioned by the passage through infinity arising from a relation of perfect commensurability in the mean motions of the two planets; so that any objection to his reasoning founded upon that ground, cannot be admitted to possess any weight. Indeed any attempt to determine the limiting values of the elements of the disturbing planet by the general formulæ of perturbation, would seem to be utterly impracticable in the present state of analysis.

(87.) The question, however, still remains for solution—how are we to account for the circumstance of the mean distance of Neptune not being included within the limits assigned by Le Verrier to the mean distance of the hypothetical planet? It appears to me that the discordance is attributable partly to Le Verrier having arbitrarily assumed the form of the general expression involving γ and ζ, and partly to his having fixed the particular values of these corrections too near each other. If, indeed, we could be assured beforehand of the *whereabouts* of the most accurate values of γ and ζ, it plainly follows that the general equations involving these quantities would be most faithfully represented by an algebraic function of the second degree, provided that, in the construction of the latter, such particular values had been employed as were at no great distance from the approximate values. That Le Verrier was under the impression of the most accurate values of γ and ζ being included within the limits of the interpolated values, is evident from the following words used by him:—"*Dans la solution à laquelle nous arriverons en définitive, γ sera à très peu près égal à* 1, *et ζ sera compris entre* 0 *et* — 1; *en sorte que les valeurs particulières des fonctions calculées pour les divers états des variables que nous venons d'indiquer, assurent, dans les environs de la solution qui convient au problème, l'exactitude de la marche des expressions algebriques approchées auxquelles nous parviendrons.*"*

(88.) Now if we assume as the elements of the planet Neptune those deduced from observation by Walker, we shall obtain $\gamma = 6.432$, $\zeta = -1.493$. It will be seen, however, that these values are very far removed from those particular values by means of which Le Verrier constructed the general equations involving γ, ζ, and m'. Since it appears, then, that the interval between the limits of the mean distance vastly exceeds the corresponding interval deducible from the researches of Le Verrier, it may fairly be questioned whether an algebraic function of the second degree, constructed from particular values of γ and ζ, widely distant from the real values, is capable of representing with sufficient fidelity the form of the general equations involving γ, ζ, and m', such as they would result after the elimination of the other unknown quantities. If we could be assured that any values of γ and ζ included within the limits of the particular values employed by Le Verrier in his researches were capable of satisfying the observations of Uranus with tolerable precision, it might then be expected that the equations of the second degree, constructed by that geometer, would assign values of γ and ζ differing only in a slight degree from the approximate values. This would follow as an obvious consequence of the vicinity of the particular values of γ and ζ to the approximate values, combined with the absence of any rapid variation of the algebraic function representing the equations. Now Le Verrier found,

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terms are supposed to be all ranged on one side so as to constitute an algebraic function equal to zero, and it is to such a function that the expression is considered to apply.

by his previous researches, that the observations would be very nearly satisfied by supposing $\gamma = 0$, $\varepsilon = 0$. It might, therefore, be inferred that the equations would assign to γ and ε values of inconsiderable magnitude, which would probably satisfy the observations to a still greater degree of accuracy. But if the algebraic function of the second degree, involving γ, ε, and m', did not represent with sufficient fidelity the equations resulting from the elimination of the other unknown quantities of the problem, it would manifestly follow that any values of γ and ε, *considerably* removed from the particular values by means of which Le Verrier constructed the algebraic equations, would fail, in a corresponding degree, to satisfy these equations, ***even although such values should satisfy the observations of the planet as well as any others.***

(89.) According to this view of the subject, the extreme values of the mean distance resulting from Le Verrier's researches ought to be considered as indicating, not the limits within which the observations of Uranus might be satisfied, by attributing to each of them a probable error of a certain magnitude, but those within which an algebraic function of the second degree, involving γ, ε, and m', constructed from certain particular values of the two former quantities, is capable of representing the equations resulting from the elimination of the other unknown quantities of the problem. Admitting this to be true, it would follow that if Le Verrier had assumed 30.0363 as his approximate mean distance, and constructed his equations by means of a series of particular values of γ and ε, corresponding to mean distances in the immediate vicinity of the approximate value, he would have obtained for the latter merely a slight correction, while at the same time he would have deduced limiting values which would have totally excluded the results of his actual researches.

(90.) The conclusion definitively suggested by the foregoing remarks is, that Le Verrier's theory of limits is rather specious than real, in so far as it relates to the possibility of representing the observations of Uranus within a certain range of mean distance upon the supposition of each of them containing a probable error of a certain magnitude. It is not pretended, however, that these remarks involve the true explanation of the remarkable discordance which prevails between the results of Le Verrier's researches and those deducible from observations of the planet Neptune. They are merely put forth with the view of indicating a probable mode of accounting for the discordance, in the absence of a thorough investigation of the subject. Until this be accomplished there is full warrant for suspecting the legitimacy of the method by which Le Verrier arrived at his results. This is a point, however, of very trivial importance, which does not in the remotest degree affect the merits of that illustrious geometer, in so far as his theoretical discovery of the planet Neptune is concerned.

(91.) We shall conclude with a few remarks on a recent publication entitled "*Report to the Smithsonian Institution on the History of the Discovery of Neptune.*" *

The author of this Report, Mr. Gould, of Cambridge, U.S., while giving full credit to both Le Verrier and Adams for the accuracy of their respective solutions of the inverse problem of perturbation, stoutly maintains the opinion expressed by his countryman, Prof. Peirce, to the effect that the remarkable connexion which was found to subsist between the theoretical researches of these geometers and the actual position of the planet Neptune, must be regarded as wholly fortuitous. Let us examine some of the arguments which he adduces in support of this peculiar view of the subject.

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* Published at Washington, 1850.

(92.) It has been stated (p. 185) that Mr. Adams, in his final communication to the Astronomer Royal, remarked that the observations of Uranus would in all probability be satisfied best by adopting for the hypothetical planet a mean distance equal to 33.6. He was led to entertain this opinion by a comparison of the errors of his theory for the three oppositions of Uranus in 1843–44–45, resulting from the two hypotheses of the mean distance which he had already employed in his researches. It is manifest, when this inference is viewed in connexion with his previous results, that Adams had renounced all faith in even an approximation to Bode's law of the distances of the planets, and that the current of his researches was rapidly conducting him to a mean distance of the hypothetical planet agreeing with the actual mean distance of the planet Neptune. Mr. Gould seeks to depreciate the merit of this sagacious conclusion by contending that a mean distance equal to 33.6 would give erroneous results. "Le Verrier," says he, "has shewn that the assumption of even 35 as the mean distance would lead to *intolerable* discordances. Peirce has further proved that an important change in the character of the perturbations takes place near the distance 35.3. It is therefore evident that no claims can be based upon the rough inference alluded to."

It has been mentioned that Peirce objected to the reasoning by which Le Verrier established the inferior limit of the mean distance of the hypothetical planet (35.04), on the ground of the continuity of the investigation being broken at the distance 35.3 by the commensurability of the mean motions of the two planets. It is plain, therefore, that the propositions announced by these two geometers are mutually incompatible; and yet Mr. Gould adduces them as confirmatory of each other! We have already had occasion to remark that there do not exist grounds for supposing that either of them is entitled to any confidence.

(93.) Mr. Gould admits that Le Verrier may be considered the discoverer of the planet Neptune, in so far as he proved not only that it was impossible to represent the motions of Uranus without the assumption of some unknown disturbing body, but that the perturbations were of that analytical form which belongs to an exterior planet.

Now it appears to me, that the latter assertion is at direct variance with the actual state of the question. Le Verrier demonstrated, by his researches, that the perturbations were such as would be produced by the direct action of an exterior planet during the interval of time over which the observations extended; but, with respect to the *analytical form* of these perturbations, it depended on the elements of the disturbing planet, which were beyond the scope of investigation, and in fact turned out to be entirely different from those deduced by Le Verrier.

(94.) Mr. Gould further remarks that Le Verrier omitted the consideration of the terms depending on a near approach to commensurability; but that this, although certainly a defect, cannot be considered as an error in the theory, since within the limits where he had reason to suppose that the orbit was situated, these terms are almost uniformly negligible.

With reference to this point it may be remarked, that the irregularities in the motion of Uranus depended on the direct action of Neptune during the period of last conjunction, and not on the analytical theory of that planet, which involves all the consequences liable to be developed in the lapse of indefinite ages. Hence it is manifest that the absence of any resemblance between the theory of the hypothetical planet of Le Verrier and that of the planet Neptune, cannot be considered as affecting in the slightest degree the merit of Le Verrier's researches, in so far as they had for their object the discovery of the disturbing body. Even in the case of Neptune, the terms to which Mr. Gould alludes do not exercise any sensible influence on the *action* of the planet between the years 1690 and 1845.

(95.) With reference to the same geometer Mr. Gould makes the following statement:—"His laborious and elegant researches have been crowned with brilliant success, and M. Le Verrier himself rewarded by the consciousness of having been the immediate occasion of the discovery of Neptune. *And although the agreement of Neptune's direction at the time of the discovery with the direction of the theoretical planet was but accidental, it almost seems as though the heavens strove to show themselves propitious, so happy was the accident, so wonderful the coincidence.*" *

(96.) Leaving the above passage to the reader's own reflections, we proceed to notice one or two other statements of Mr. Gould's. Referring to the assertion of Sir John Herschel, in his "*Outlines of Astronomy,*" that the longitude and radius vector of the hypothetical planet, whether of Adams or Le Verrier, very nearly coincided with the longitude and radius vector of Neptune during the period of its action being sensible, Mr. Gould remarks:—"But surely it cannot be considered as an analogy between the two orbits, that the perihelion of the one was so near the aphelion of the other."

The analogy between the two orbits, demanded by the question relative to the disturbing body, was confined solely to a pretty close coincidence of the paths of the hypothetical and real planets during the period of the disturbing force being sensible. Even in this case it was a near agreement of the longitudes, rather than of the distances, which was required by the conditions of the problem. With respect to the absolute identity of the two orbits, the establishment of such a condition was an object of no importance in so far as the discovery of the disturbing body was concerned. Sir John Herschel, in the work to which Mr. Gould refers—so far from attempting to demonstrate any resemblance between the elements of Neptune on the one hand, and those of the hypothetical planet of either Le Verrier or Adams on the other—on the contrary, utterly repudiates the existence of any necessary connexion between such an analogy and the question relative to the discovery of the disturbing body. But, apart from all consideration of this circumstance, it seems surprising that Mr. Gould should urge such an objection to the identity of the two orbits as that above cited, when it is borne in mind that in the one case the orbit is very eccentric, and in the other case is almost circular.

(97.) Mr. Gould concludes his Report with a remark the object of which is to reconcile the conflicting results of observation and theory. "The combined labours of Le Verrier and Peirce," says he, "have incontrovertibly proved that, by reducing the limits of error assumed for the modern observations to 3″, there can be but two possible solutions of the problem. There are two different mean distances of least possible error, one of which is 36, and the other 30. The one is included within the theory and limits of Le Verrier, and corresponds with Adams's solution; the other is the orbit of Neptune."*

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* Report, p. 51.

With respect to the existence of two mean distances of least possible error, with an interval included between them, any mean distance corresponding to which is incapable of satisfying the observations with sufficient accuracy, it seems to be in the highest degree improbable. This will be readily seen by reference to the theoretical researches of Le Verrier and Adams. The elements of the first and second planet of Adams, and those which Le Verrier deduced from his final investigation, exhibit a successive diminution of the mean distance. Now, in each of these three cases, the mean distance was greater than the true value; but this defect was remedied by increasing the eccentricity in a corresponding degree, and placing the perihelion near the point of conjunction of Neptune and Uranus. By this means the distances of the disturbing body were rendered in each case very nearly equal to the true distances in the part of the orbit where considerable precision was indispensable; and the effect of the error in the mean distance was thrown upon the opposite portion of the orbit extending on each side of the aphelion, where it was incapable of exercising any influence. The following table will exhibit this view of the subject in a clearer light:—

Hypothetical Planet.	Mean Distance.	Perihelion Distance.	Aphelion Distance.	Longitude of Perihelion.
Adams, Hyp. I. .	38.400	32.216	44.584	315° 57′
„ Hyp. II. .	37.478	32.958	41.998	299 11
Le Verrier . . .	36.154	32.264	40.044	284 45

It appears from these results, that the perihelion distance is almost the same for each of the three planets, and that the longitude of the perihelion does not in any case differ materially from the longitude of Uranus and Neptune (273°) on the occasion of their last conjunction, about the beginning of the year 1822. On the other hand, the aphelion distance varies nearly at a rate corresponding with the variation of the mean distance. Now if we suppose the mean distance of the hypothetical planet to be diminished below the value assigned by Le Verrier, so as to approach nearer the mean distance of Neptune, have we not strong reason to believe that, by similarly throwing the effect of the change mainly upon the aphelion distance, where it would be altogether uninfluential, the observations of Uranus would be satisfied with the same degree of precision as in the foregoing cases? Indeed it seems very probable that this object might be accomplished by employing any mean distance within a range extending considerably both above and below the mean distance of Neptune, the perihelion being turned towards the point of conjunction when the mean distance was greater, and the aphelion being turned towards the same point when the mean distance was less, than the true value.

(98.) We may recapitulate the conclusions suggested by the discovery of the planet Neptune in the following terms:—Two contemporary geometers,

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* Report, p. 55.

Mr. Adams in England, and M. Le Verrier in France, undertook about the same time to investigate the irregularities of Uranus upon the supposition of their being produced by the action of an exterior planet, and, independently of each other, arrived at a very approximate determination of the position of the disturbing body. Upon this ground, therefore, they are severally entitled to the honour associated with the theoretical discovery of the planet Neptune. With respect to Le Verrier's researches on the limits of the orbit of the disturbing body, they have not been borne out by the results of actual observations; but this circumstance, attributable in all probability to the intricacy of the subject and the imperfect state of analysis, does not in the slightest degree impugn his claims to the great discovery just mentioned. The American astronomers and mathematicians have more especially distinguished themselves by their labours in connexion with the planet Neptune, since the epoch of its physical discovery. The results that have been deduced from Bond's observations of the satellite of Neptune and the mathematical researches of Walker and Peirce, unquestionably exhibit a degree of consistency with the actual observations of Uranus and Neptune which has not been paralleled by any similar efforts on this side of the Atlantic, while at the same time they tend to throw much interesting light on the theory of both planets. The peculiar views which Prof. Peirce was led to entertain, respecting the researches of the distinguished geometers to whom the theoretical discovery of Neptune is due, may perhaps be attributed to his having devoted his attention too exclusively to the analytical formulæ representing the action of the planets, without taking into sufficient consideration the mode in which the disturbing forces directly operate. These views were announced by Prof. Peirce in a spirit of candour and moderation highly honourable to his character as a philosopher. They are beyond all doubt erroneous, but the trifling inadvertence into which he was thus betrayed does not detract from the merit of his more substantial labours in connexion with the theory of Uranus and Neptune.

IV.

REMARKS ON THE LUNAR INEQUALITY TERMED THE EVECTION.

One of the most remarkable instances of perturbation which occurs in the solar system is the inequality in the moon's longitude termed the evection. So long as the moon was observed merely in eclipses, this inequality continued to escape the notice of astronomers. When Hipparchus, however, after having constructed the astrolabe, succeeded in determining the position of the moon in quadratures, he found that the results could not be generally reconciled with the existing theory of her motion. That great astronomer, having no similar observations of the moon anterior to his own accessible to him, was unable to arrive at a definitive conclusion respecting the anomaly; but he formally pointed out its existence, and executed a series of valuable observations with the view of aiding future astronomers in their researches on the subject. It is well known that the discovery of the law of this famous inequality is due to Ptolemy. The account which he has given of the inequality as it presented itself to his observations *, would seem to imply a law of variation

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* Syntaxis, lib. v., cap. ii.

materially different from that suggested by the term representing the same inequality in the modern theory of the moon's motion. He states that the observed places of the moon in quadratures, whether those recorded by Hipparchus or those actually determined by himself, were found in some instances to agree very well with the computed places; in other instances to differ considerably, being sometimes in excess and at other times in defect. By attentively pursuing the inequality through its various phases, he found that it was generally insensible in sizygees. It also vanished in the quadratures when the moon was in the apogee or perigee of her epicycle (in other words, when the line of apsides was in quadratures); but it increased from those points towards the mean points of the orbit where it was greatest (in other words, it increased as the line of apsides revolved from the quadratures to the sizygees). Moreover, when the first anomaly (the equation of the centre) was subtractive, the observed place of the moon was in defect, in consequence of the new inequality; and when the first anomaly was additive, the observed place was in advance of the computed place, from the same cause.

It appeared, then, that while the inequality vanished in sizygees, its effect in quadratures was invariably to augment the equation of the centre, unless the line of apsides was in quadratures, when it vanished altogether Ptolemy, from observations of the moon in sizygees, had determined the maximum value of the equation of the centre to be 5° 1′*. In consequence of the new inequality, its value, as indicated by observations in quadratures, generally exceeded 5° 1′, increasing from that value to 7° 40′ as the line of apsides revolved from quadratures to sizygees. Hence it followed that the maximum effect of the new inequality amounted to 2° 39′.

In modern astronomy the inequality in the moon's longitude, depending on the combined effects of the equation of the centre and the evection, is represented thus:—

$$\partial_\prime = + 6^\circ\, 18' \sin \mathrm{A} + 1^\circ\, 20' \sin \Big(2\,(☾ - ☉) - \mathrm{A}\Big),$$

where A represents the mean anomaly of the moon, ☾ the mean longitude of the moon, and ☉ the mean longitude of the sun.

Nothing can at first sight appear more different than the ancient and modern modes of representing the two inequalities. With respect to the equation of the centre, its magnitude is materially different in the two cases. The evection, however, differs not merely in absolute magnitude, but also in the law of its variation. According to Ptolemy the zero points of the inequality were *fixed* in position, being constantly situate in the sizygees, while its maximum value was *variable*. On the other hand, it is manifest, from the second term of the above equation, that the zero points of the modern inequality are *variable* in position relatively to the line of sizygees, but that its absolute magnitude is *constant*.

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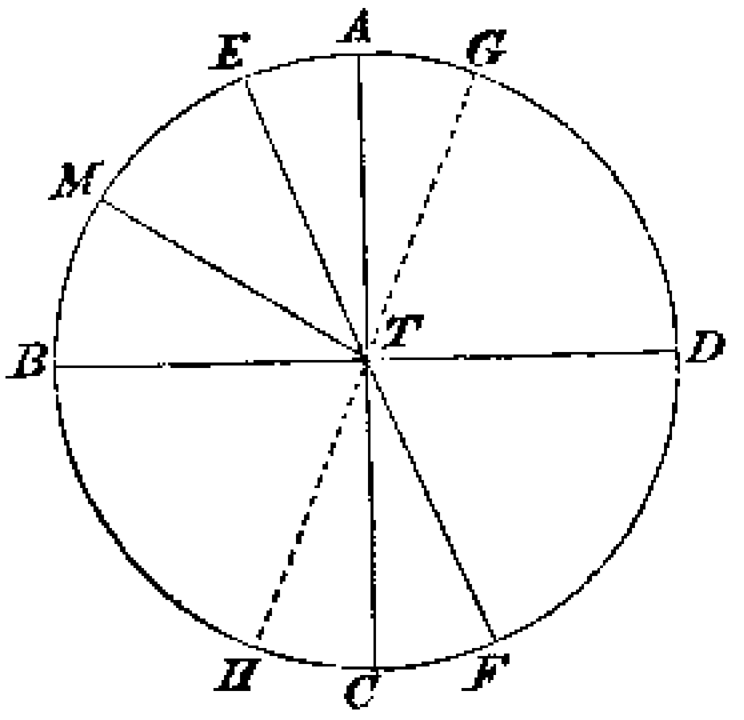
* Ptolemy determined the ratio of the epicycle of the lunar orbit to the deferent, or, in other words, the maximum value of the equation of the centre, from three eclipses of the moon observed at Babylon, about 700 years before the Christian era, and also from three similar eclipses observed by himself. In both cases he found the ratio to be as $5\frac{13}{60}$ to 60, which gives 5° 1′ for the equation of the centre (*Syntaxis*, lib. iv.). Delambre, having computed the equation of the centre by the modern analytical formulæ, found that the three Chaldean eclipses assigned 4° 59′ 16″ as its value, and that the three eclipses of Ptolemy made it equal to 4° 59′ 42″. The close agreement of these results affords a strong presumption, that the two sets of eclipses employed by Ptolemy in his calculations were selected on account of their mutual consistency, from a vast mass of similar ob-



Notwithstanding the striking points of difference referred to in the foregoing remark, the effects produced by the combination of the two constituent inequalities are identical in both cases as respects the law of variation, and are also nearly so in respect of absolute magnitude. This may be easily shewn in the following manner. Let A C represent the line of sizygees, B D the line of quadratures, E F the line of apsides, M the place of the moon in her orbit.

Let $A T E = \phi$, $A T M = \text{☾} - \odot = \theta$.

Hence $A = E T M = \theta - \phi$.

$$2(\text{☾} - \odot) - A = 2\,\theta - (\theta - \phi) = \theta + \phi.$$

$$\begin{aligned}\text{Therefore } \delta v &= 6^\circ\,18'\sin A + 1^\circ\,20'\sin\Big(2(\text{☾} - \odot) - A\Big),\\ &= 6^\circ\,18'\sin(\theta - \phi) + 1^\circ\,20'\sin(\theta + \phi),\\ &= 4^\circ\,58'\sin(\theta - \phi) + 1^\circ\,20'\sin(\theta - \phi) + 1^\circ\,20'\sin(\theta + \phi),\\ &= 4^\circ\,58'\sin A + 2^\circ\,40'\cos\phi\sin\theta.\end{aligned}$$

The first of these terms is manifestly the equation of the centre as deduced by Ptolemy from observations of the moon in sizygees. The second term also represents the evection as it exhibited itself to that astronomer. Thus let us suppose the moon to be in either of the sizygees. In such a case $\theta = 0$, or 180°, and consequently the second term vanishes.

Hence $\delta v = 4^\circ\,58'\sin A$.

Again, if the moon be in quadratures, we have $\theta = 90^\circ$, and therefore

$$\begin{aligned}\delta v &= 4^\circ\,58'\sin A + 2^\circ\,40'\cos\phi\\ &= 4^\circ\,58'\cos\phi + 2^\circ\,40'\cos\phi,\\ &= 7^\circ\,38'\cos\phi.\end{aligned}$$

In this case, then, the two inequalities conspire together. The effect is obviously a maximum, when $\phi = 0$, or 180°. We have then

$$\delta v = \pm\,7^\circ\,38'.$$

These conclusions agree with Ptolemy's description, subject to a slight difference in the numerical values. Indeed the precision with which that astronomer determined the combined effects of the two inequalities in sizygees and quadratures, is one of the most astonishing circumstances connected with the ancient astronomy.

Since the evection as represented by Ptolemy has always the same sign in the quadratures as the equation of the centre, it is manifestly positive when the moon is revolving from conjunction to opposition, and negative throughout the remaining half of the orbit, or *vice versâ;* according as the perigee is situate in the first and fourth, or in the second and third quadrants of the lunar orbit, counting from the point of conjunction in the direction of the moon's motion.

In order to determine the zero points of the evection as represented by modern astronomers, we have

$$\sin(\theta + \phi) = 0;$$

$$\therefore \theta = -\phi, \text{ or } 180^\circ - \phi.$$

Hence it is manifest that by drawing G H, making with A C the same angle which E F makes with it, the extremities G, H, will indicate the zero points of the inequality.

It has been stated (p. 424) that Horrocks first explained the evection of the lunar orbit to be variable, and attributing a libratory motion to the line of apsides. Allusion has also been made to the difficulty experienced for some time in computing, by the theory of gravitation, the motion of the lunar apogee, upon which the inequality to a great extent depends. It is worthy of remark that in the original edition of the "Principia," published in 1687, Newton states * that he computed the motion of the lunar apogee in sizygees and quadratures, and also the mean motion. He asserts that he found the daily progression in sizygees to be 23′, the daily regression in quadratures to be 16⅓′, and the mean annual motion to be 40°. He remarks that these results do not accord exactly with the tables, a circumstance which he thinks may be attributable to the errors of the observations. The calculations being very intricate and embarrassed with approximations, and the results not possessing all the accuracy that was desirable, he refrained from publishing the details of his researches on the subject. (*Computationes autem, ut nimis perplexas et approximationibus impeditas, neque satis accuratas, apponere non lubet.*)

The results which Newton obtained on this occasion cannot by any means be considered very inaccurate, when the intricacy of the subject and the imperfect state of analysis in his time are taken into account. They give 11° 21′ for the monthly progression of the apogee in sizygees, and 8° 1′ for the monthly regression in quadratures. The modern tables of the moon assign, in round numbers, 11° and 9° as the corresponding values of the motion of the apogee. Newton found the mean annual progression of the apogee to be 40°; the modern tables of the moon make it 40° 40′ 32″.

Newton appears to have been so dissatisfied with his researches on this subject, in all probability from the circumstance of the results not presenting a more complete accordance with those deducible from observation, that he suppressed all allusion to them in the second edition of the "Principia," published in 1713, under the superintendence of Cotes. Whatever may have been the method of investigation employed by him on this occasion, it was manifestly one which was capable of grappling with the main difficulties of the question. It is not improbable that a careful inspection of those manuscripts of Newton, which are still in existence, might serve to throw some light on this interesting point.

V.

NOTE RESPECTING HORROCKS.

At page 421 I have hazarded the conjecture that it was duties of a religious nature which called away Horrocks so peremptorily, while engaged in looking out for the transit of Venus on the 24th of November, 1639. This is confirmed by a note which the late Prof. Rigaud discovered in one of Hearne's Memorandum Books preserved in the Bodleian Library, Oxford, from which it appears that Horrocks was a hard-working curate at Hoole, subsisting upon a wretched pittance (*Rigaud's Correspondence of Eminent Men of the Seventeenth Century*, vol. ii. p. 112). It appears also, from one of Flamsteed's letters to Collins, contained in the same work, that Crabtree's death occurred in the year 1652, and not shortly after that of Horrocks, as Wallis erroneously stated in the dedicatory epistle to Lord Brouncker, inserted at the commencement of the "*Opera Posthuma*" of the latter.

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* Principia, lib. iii. prop. xxxv., scholium.

VI.

ACCOUNT OF SOME RECENT RESULTS OF ASTRONOMICAL OBSERVATION.

Two instances of a total eclipse of the sun have recently furnished opportunities of observing the circumstances usually attending these phenomena. The first of these eclipses happened on the 8th of August, 1850. It was visible only in the Pacific Ocean. An account of the phenomenon as observed by M. Kutczycki at Honolulu, the chief town of the Sandwich Isles, appeared in the *Comptes Rendus* for the 21st of April, 1851. The second eclipse happened on the 28th of July, 1851. Being visible in the northern countries of Europe, it was observed by a great number of astronomers. Two important facts were satisfactorily established by the observations of these eclipses. In the first place, the reddish protuberances usually visible on such occasions, appeared in some instances to be isolated from the moon's limb. Secondly, those protuberances that were visible towards the point of immersion, were seen gradually to diminish as if concealed by the passage of the moon over the solar disk; while, on the other hand, those towards the point of emersion appeared to enlarge as if gradually disclosed to view by the same cause. Both these facts tend to support the opinion that the protuberances are solar phenomena. A serious difficulty attending the explanation of their physical cause, consists in the material difference of aspect which they exhibit to spectators distant from each other by only a very short interval.

Five more planets revolving between the orbits of Mars and Jupiter, have been discovered in addition to those referred to in the body of this work (see p. 240). Three of these bodies were discovered in the year 1850. The first (Parthenope) was discovered by De Gasparis on the 11th of May; the second (Victoria), by Hind on the 13th of September; and the third (Egeria), by De Gasparis on the 2nd of November. The remaining two planets were discovered in the course of the year 1851. The first of these (Irene) was discovered by Hind on the 19th of May, 1851. By a singular coincidence, De Gasparis also independently discovered this planet on the 23rd of the same month. The second planet (Eunomia) was discovered by De Gasparis on the 29th of July. Parthenope revolves round the sun in 1401 days, Victoria in 1303 days, Egeria in 1496 days, Irene in 1510 days, and Eunomia in 1424 days. These numbers, of course, can only be regarded as provisional. The total number of asteroids now discovered amounts to fifteen. It is not improbable that hundreds of these minute bodies may be revolving in the same region.

On the 4th of December, 1850, intelligence reached this country that on the 15th of the previous month, Mr. Bond, Director of the Observatory of Cambridge, U. S., had discovered a new ring round Saturn, interior to the bright rings already known to exist. It soon turned out that the same phenomenon had been observed in England by Mr. Dawes on the 29th of November, before he received any intimation of Mr. Bond's discovery. The most surprising circumstance, however, connected with the phenomenon is, that it was actually observed as early as the year 1838, by Dr. Galle of Berlin; although no further notice seems to have been taken of it till the announcement of its rediscovery as above mentioned. The ring now forms an interesting object of observation to astronomers armed with powerful telescopes. In brightness it is very much inferior to the outer rings. Its breadth is equal to about two-fifths of the interval included between the bright rings and the body of the planet. It would appear from most of the observations that it is not a distinct appendage of the planet, but simply a continuation of the inner bright ring.

On the 24th of October, 1851, Mr. Lassell discovered two new satellites revolving round Uranus. He has subsequently succeeded in seeing them with his powerful reflector, on every occasion on which he looked for them. He finds that the observations may be pretty well satisfied by supposing the period of the inner satellite to be 2.506 days, and that of the outer satellite to be 4.150 days. It appears, therefore, that they are interior to the two bright satellites discovered by Sir William Herschel in 1787. From the diagram of their positions inserted in the *Monthly Proceedings* of the Astronomical Society for November, 1851, they appear, like the other satellites, to revolve in orbits nearly perpendicular to the plane of the ecliptic.

It has been mentioned (p. 139) that a comet discovered by M. Faye, in the year 1843, was found to revolve in an elliptic orbit, and that its perturbations for the ensuing revolution were calculated by Le Verrier, who arrived at the conclusion that its passage through the perihelion would take place on the 2nd of April, 1851. It is a gratifying fact that the comet has actually returned at the appointed time. It was first seen by Prof. Challis, with the Northumberland refractor, on the 28th of November, 1850. The observations of its apparent position have been found to present a remarkable agreement with the corresponding results derivable from the calculations of M. Le Verrier.

Allusion has been made at page 243 to the discovery of a small ultra-zodiacal planet (Metis) at the observatory of E. Cooper, Esq., of Markree, in the north of Ireland. An achievement of vastly greater importance has since emanated from that observatory in the shape of a catalogue of 14,888 stars near the ecliptic, the places of which, in general, are not to be found in any catalogues hitherto published. This catalogue was constructed from observations made in the years 1848, 1849, and 1850, and was published in 1851, the expense of printing having been defrayed by the Government, upon the recommendation of the Royal Society. A second catalogue, destined to contain the places of about 12,000 additional stars, observed in the year 1851, is in the course of preparation at the same observatory. Mr. Cooper and his active assistant, Mr. Graham, are also engaged in executing a series of celestial maps upon a magnificent scale. Each map has a range of 8° both in right ascension and in declination. The scale is four times larger than that of the Berlin maps. It is contemplated to insert in these maps all the stars within their range which have either been observed at Markree, or have been already published in other catalogues. The epoch of reduction is 1850.0. The advantages which cannot fail to accrue to astronomical science from the construction of these maps is incalculable. It must be acknowledged that the labours at Markree Observatory exhibit a loftiness of aim as well as a unity of design, and a spirit of skilful perseverance, which not only serves effectually to remove that establishment from the category of mere amateur observatories, but entitles it to an honourable place in the highest class of those institutions that have been founded for the promotion of astronomical science.

In concluding this note it may be stated, that the Astronomer Royal has now (February, 1852) completed the arrangements at the Royal Observatory for recording transits of stars by means of an electro-magnetic apparatus. The accuracy of this method may be relied on to the twentieth of a second of time. It is contemplated, in connexion with this improvement, to transmit Greenwich time, by means of the electric telegraph, to all the most important places in the kingdom. The realisation of this project will constitute a boon of inestimable value to the outports, by affording on all occasions a reliable standard for the regulation of chronometers. The successful construction of the submarine telegraph between Dover and Calais will also enable the Royal Observatory to record transits simultaneously with the Royal Observatory of Paris and other similar establishments on the Continent, by which means their respective longitudes relatively to each other may be more accurately ascertained. The immense importance of this object must be obvious to any person who possesses an ordinary acquaintance with astronomical science.

VII.

COPY OF THE NOTE OF THE OBSERVATION OF γ DRACONIS, *made by Bradley*, at *Kew*, *with the zenith sector of Molyneux, on the* 21*st of December*, 1725 ; *the discordance of which with the results of previous observations, revealed to him the first glimpse of his immortal discovery of the Aberration of Light.*

It has been mentioned at page 337 that the original note of the observation, of which the subjoined words are an exact copy, was found a few years since by Prof. Rigaud, among the manuscripts of Bradley, written upon a loose piece of paper.

Dec 21^{st} Tuesday 5^h 40′ sider time
Adjusted y^e mark to y^e Plumb line
& then y^e Index stood at 8
5^h 48′ 22″ y^e star entred
49 $52\frac{1}{2}$ Star at y^e Cross
51 24 Star went out
could
As soon as I let go y^e course
∧
screw I perceived y^e star too
much to y^e right hand &
so it continued till it passed
y^e Cross thread and within a quarter
was
of a minute after it had passed
graduat
I turned y^e fine screw till I saw
∧
y^e light of y^e star perfectly
bissected and after y^e obser
vation I found y^e index
at $11\frac{3}{4}$, so that by this
observation y^e
mark is about $3''\frac{3}{4}$
too much south
but adjusting
y^e mark and plumb line
I found y^e index at 8

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ADDITIONS AND CORRECTIONS.

PAGE vi, line 2 from top, *for* Ferdinand, *read* Frederick.
,, 20, line 34 from top, *for* into, *read* in.
,, 51, line 43 from top, *for* has *read* have.
,, 70, line 21 from top, *for* that, *read* twice that.
,, 70, line 9 from bottom, *for* 10″, *read* 18″.
,, 81, line 1 from bottom, after Sciences *read* 1707.
,, 81, line 9 from bottom, *for* Pond, *read* Pound ; *read* also similarly pp. 84, 130.
,, 82, line 4 from top. Bradley's tables of Jupiter's satellites, although not published till 1749, were printed off in 1719. His discovery of aberration did not occur till a few years afterwards. This circumstance accounts for the erroneous value which he assigned to the equation of light in his tables.
,, 119, line 20 from top, *for* third, *read* first.
,, 122, line 9 from bottom, *for* September *read* June.
,, 150, line 2 from bottom, *for* 62^s *read* 52^s.
,, 172, line 34 from top, *for* 1846 *read* 1845.
,, 188, line 6 from bottom, *for* $\frac{1}{9500}$ *read* $\frac{1}{9312}$.
,, 197, line 1 from top, *for* 1850 *read* 1851.
,, 212, line 13 from top. The utility of the transits of the inferior planets for determining the solar parallax was first announced by James Gregory. (p. 428.) Halley merely pointed out the peculiar advantages which the transits of Venus offered for this purpose.
,, 217 lines 4 and 5 from top, *for* pole *read* axis.
,, 253, line 17 from top, *for* $\frac{1}{300}$ *read* $\frac{1}{30}$.
,, 415, line 1 from top, *for* Vitellionem *read* Vitellion.
,, 512, line 27 from top. The stars of M. Rümker's great catalogue have been observed at Hamburgh : they are consequently visible in the northern hemisphere.
,, 542, line 3 from bottom, *for* 1.6 *read* 1.2.
,, 543, line 2 from top, *for* 2.014, which is equal to 1.6 + 414, *read* 1.614, which is equal to 1.2 + 414.

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